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**Richard Funston**

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Determinants of the Outcomes of Midterm Congressional Elections



**K. J. Holsti**

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The Primary Goals of Political Parties: A Clarification of Positive Theory

**Gerald S. Strom, Stephen V. Stephens, Lawrence S. Mayer and I. J. Good, Nathaniel Beck, Gordon Tullock, John A. Ferejohn and Morris P. Fiorina, R. E. Goodin and K. W. S. Roberts, Eric M. Uslaner and J. Ronnie Davis, Norman Frohlich, Guillermo Owen, David H. Koehler, Peter Bernholz, Joe Oppenheimer**

Participation, Coalitions, Vote Trading

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Published Quarterly by

*The American Political Science Association*

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September 1975

No. 3

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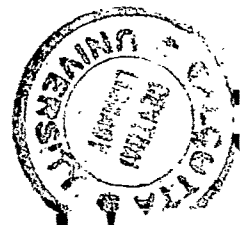
Office of publication: Curtis Reed Plaza, Menasha, Wisconsin.

Foreign Agent: P. S. King and Staples, Ltd., Great Smith Street, Westminster, London.

Second class postage paid at Washington, D.C., and at additional mailing offices.

Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.

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## ARTICLES

- 795 The Supreme Court and Critical Elections.** Several years ago Professor Robert Dahl argued that the traditional concern over the Supreme Court's power of judicial review was largely unfounded. Dahl demonstrated that seldom, if ever, had the Court been successful in blocking the will of a law-making majority. This paper argues that, had Dahl considered his data from a different perspective, he would have discovered that, by virtue of the recruitment process, the Court will rarely even attempt to thwart a law-making majority. Examining Dahl's data in the context of the Survey Research Center's election classification scheme, the paper focuses on the Court's relation to patterns of partisan change to show that the traditional philosophic concern with the counter-majoritarian nature of judicial review is largely divorced from empirical reality and has relevance only during periods of partisan realignment within the political system as a whole. The paper buttresses the argument that the Court's "yea-saying" power is more important than its "nay-saying" power, a realization which can serve as the premise from which a logically consistent justification of the Court's power of judicial review may be dialectically constructed.

By RICHARD FUNSTON, Associate Professor of Political Science, San Diego State University.

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- 812 Determinants of the Outcomes of Midterm Congressional Elections.** An explanatory model for the outcomes of midterm congressional elections is developed. Midterms are a referendum on the performance of the President and his administration's management of the economy. The explanatory model of midterm congressional elections is sufficiently powerful so as to yield honest and accurate pre-election predictions of the national two-party vote in midterm elections. These predictions have usually outperformed pre-election forecasts based on survey data. The model is extended by considering the translation of votes into seats, models of the electorate as a whole and of the individual voter, and the causes of the off-year loss by the President's party.

By EDWARD R. TUFTE, Professor of Public Affairs, Princeton University.

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- 827 Underdevelopment and the "Gap" Theory of International Conflict.** A common hypothesis about the sources of international conflict holds that war and turmoil will be an inevitable consequence of the widening "gap" between the developed and underdeveloped states. This view is based on a common Western image of underdeveloped countries which assumes that striving for economic betterment is universal in all underdeveloped countries, and is primarily a grass-roots phenomenon. This essay challenges the hypothesis and the assumptions upon which it is based. It argues that the images of underdevelopment generated by economists using aggregate data are in many cases incorrect or distorted. Studies by anthropologists which are based on micro- rather than macrodata produce quite different impressions of the underdeveloped society. The human costs involved are for the most part overlooked in development schemes, and the wholesale importation of Western economic development strategies has led in many cases not only to a poor allocation of resources, but also to many of the problems the developed societies are now facing, including urban congestion, rising crime rates, higher incidence of mental breakdown, and the like.

The paper concludes with a critical review of common liberal solutions to development problems, and suggests that one strategy possible for some developing countries is increased isolation from the international system. International conflict may result not only because the underdeveloped states wish to close the "gap," but because some may choose deliberately to reduce their dependency on the West. Conflict may be generated through isolation as well as through increased interaction.

By K. J. HOLSTI, Professor of Political Science, The University of British Columbia.

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- 840 The Primary Goals of Political Parties: A Clarification of Positive Theory.** Positive or rational choice theorists have tended to suppress under the rubric of "winning" elections a critical distinction in the goals of political parties (or candidates)—the distinction between the primary goal of office and the goal of the benefits derived from the control of office. The distinction, however, has strategic consequences. Logically, the office-seeker should follow the vote-maximization strategy put forth by Downs, whereas the benefit-seeker should find Riker's minimal winning coalition most congenial. The distinction in goals and strategies also implies divergent ways of organizing political parties. A concern for benefits logically leads to the development of structures designed to insure that the party's officeholders will deliver the desired benefits. The office-seeking goal implies structures which free the party and the office-seeker to maneuver in response to electoral needs. Thus there are two positive theories resting upon two primary political goals. In their differences we find an explanation of the tensions in democratic parties.

By JOSEPH A. SCHLESINGER, Professor of Political Science, Michigan State University.

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- 850 Variations in Elite Perceptions of American States as Referents for Public Policy Making.** Following Walker, investigators have assumed that state executives frequently seek policy guidance from other states. This study expands the "diffusion of innovation" literature by demonstrating that levels of elite consensus

about which American states have the better agencies vary by policy area. It is hypothesized that state administrators in policy areas characterized by general agreement about the better state programs are more influential in their own state's policy-making process than are state administrators in policy areas where there are no acknowledged leaders. Data gathered by mail questionnaire from upper-level state executives in ten American states provide support for the hypothesis. Finally, results from state expenditure studies, which also indicate that different mixes of actors are influential in state policy making depending upon the policy area involved, are found to be consistent with this interpretation.

By FRED W. GRUPP, JR., Research Associate and Executive Director, The Peoples of Connecticut Project, Department of Sociology, University of Connecticut, and ALAN R. RICHARDS, Professor of Political Science, Louisiana State University.

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- 859 **Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe.** This paper is a partial systematic test of Morton A. Kaplan's "theory" of alliance behavior in balance of power international systems first proposed in his well-known *System and Process in International Politics* (1957). Three hypotheses are inferred from Kaplan's writings predicting that in a stable balance of power system, (a) alliances will occur randomly with respect to time; (b) the time intervals between alliances will also be randomly distributed; and (c) a decline in systemic alliance formation rates precedes system changing events, such as general war. We check these hypotheses by applying probability theory, specifically a Poisson model, to the analysis of new data on fifty-five alliances among the five major European powers during the period 1814-1914. Because our research questions are so general, our findings should not be regarded as definitive; however, the data very strongly support our hypotheses. We conclude that Kaplan's verbal model of a balance of power international system has had its credibility enhanced as a result of this paper.

By PATRICK J. MCGOWAN, Associate Professor of International Relations, University of Southern California, and ROBERT M. ROOD, Assistant Professor of Government and International Studies, University of South Carolina.

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- 871 **Models, Measurement and Sources of Error: Civil Conflict in Black Africa.** Cross-national research is plagued by several methodological problems that threaten to distort results and hence raise questions concerning the adequacy of substantive findings. The extent and impact of three of these methodological problems—measurement, sampling, and specification errors—are assessed for a recent model of civil conflict developed by Gurr and Duvall. Concepts in their model are measured with two distinct data sets to estimate measurement error; to assess sampling error, measures are applied to a sample of black African nations which were excluded from their analysis; and new concepts are introduced to assess error in theoretical specification. Although all forms of error are found to exist in their work and to have some distorting effects on their model, it is concluded that some of the general model is accurate. However, discovery of (1) the concepts and data sets that contain the greatest error, and (2) the sources of sampling and specification error, is used to improve substantive findings about the causes of manifest political conflict.

By MARY B. WELFLING, Research Associate and Lecturer, Yale University.

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- 889 **Nondecisions and Power: The Two Faces of Bachrach and Baratz.** The concept "nondecision" was advanced by Bachrach and Baratz as a means of identifying certain areas of community power neglected by reputational and issue analysis approaches. While it is descriptively suggestive of certain possible areas of neglect, it has not been shaped into a useful analytic tool, and does not make any demands which cannot be met by decision-making analysis. Its terms are, in fact, somewhat confused since it fails to differentiate between nondecisions brought about by covert control, and those which may be more generally attributable to a mobilization of bias. These together seem to comprise what Bachrach and Baratz describe as the "second face of power," which has been neglected, they argue, because of inadequate consideration of the meaning of power and related concepts. But their own approach to these does not aid empirical analysis. It simply encourages concern for the minutiae of political action, whereas the simplified view of power adopted here suggests the advantages of a contextual approach.

By GEOFFREY DEBNAM, Senior Lecturer in Political Studies, University of Otago, New Zealand.

**Comment.** By PETER BACHRACH, Professor of Political Science, Temple University, and MORTON S. BARATZ, Vice Chancellor for Academic Affairs, University of Maryland, Baltimore County.

**Rejoinder.** By GEOFFREY DEBNAM.

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**908 Not Voting.** If a person were rational in the sense of maximizing his own utility, it seems unlikely he would ever vote in a mass election. The time and effort expended in voting normally outweigh the infinitesimal chance of influencing the election, making voting seem irrational. But millions do vote. Addressing this paradox, Ferejohn and Fiorina suggested in 1974 that hypothesizing a different form of rational decision making, the "minimax regret" criterion, makes voting rational under rather general conditions. The several contributions arrayed here carry the debate further. Strom reviews the extant arguments and proposes a revised utility-maximization hypothesis. The works by Stephens, Mayer and Good, Beck, and Tullock take up various problems with the minimax regret criterion, including especially the assumption that the voter has no idea about the likelihood of different outcomes for the election. Ferejohn and Fiorina respond to each author and present presidential election data that support the minimax regret hypothesis more than the utility maximization hypothesis. Finally, Goodin and Roberts suggest that because egoistical preferences are weak in the voting decision, voters may be following their usually less visible ethical preferences instead.

**On the Apparent Paradox of Participation: A New Proposal.**

By GERALD S. STROM, Assistant Professor of Political Science, University of Illinois at Chicago Circle.

**The Paradox of Not Voting: Comment.**

By STEPHEN V. STEPHENS, Baltimore, Maryland.

**Is Minimax Regret Applicable to Voting Decisions?**

By LAWRENCE S. MAYER, Research Statistician and Lecturer in Statistics, Princeton University, and I. J. GOOD, University Professor of Statistics, Virginia Polytechnic Institute and University.

**The Paradox of Minimax Regret.**

By NATHANIEL BECK, Assistant Professor of Political Science, Washington State University.

**The Paradox of Not Voting for Oneself.**

By GORDON TULLOCK, University Professor of Political Science, Virginia Polytechnic Institute and State University.

**Closeness Counts Only in Horseshoes and Dancing.**

By JOHN A. FEREJOHN, Associate Professor of Political Science, and MORRIS P. FIORINA, Associate Professor of Political Science, California Institute of Technology.

**The Ethical Voter.**

By R. E. GOODIN, Assistant Professor of Government and Politics, University of Maryland, and K. W. S. ROBERTS, Research Fellow in Economics, St. John's College, Oxford University.

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**929 The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities.** In an article, "The Paradox of Vote Trading," (*APSR* 67 [December, 1973]) William H. Riker and Steven J. Brams have argued that systematic logrolling among all members of a legislature produces a paradox: While each trade is individually rational, the effects of externalities offset the potential gains from exchanging votes and each voter finds himself worse off than he would have been by voting sincerely. We extend the results of Riker and Brams to a unanimity decision rule and find that a paradox of vote trading holds for that decision rule as well as for simple majority rule. Under a unanimity rule, however, trades which would be collectively rational (i.e., which would produce a Pareto optimal result) are not individually rational; the non-trader is the beneficiary under such a decision rule. Finally, we pose the question Riker and Brams suggested: Is the paradox of vote trading inescapable? Except under very restrictive conditions, we find that it is. However, given certain assumptions about the distributions of individual utilities, we present proofs of the necessary and sufficient conditions for the Pareto optimality of vote trading and argue that in actual legislative situations, when vote trading is Pareto optimal, learning behavior should serve to extricate the members from the paradox of vote trading.

By ERIC M. USLANER, Assistant Professor of Government and Politics, University of Maryland, and J. RONNIE DAVIS, Professor of Economics, University of Florida.

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**943 The Instability of Minimum Winning Coalitions.** This paper examines William Riker's thesis that only minimum winning coalitions form in  $n$ -person zero-sum symmetric games. It demonstrates that Riker's conclusion is false by identifying the conditions under which larger than minimum winning coalitions can form. Since these conditions are quite general it indicates that Riker's conclusion is valid only for a highly restricted class of games. This class of games is identified as those in which players not in a minimum winning coalition have no incentive to form any coalitions among themselves. These games are characterized

as games inessential over coalitions of losers. Only in these games can minimum winning coalitions be expected to form exclusively. In all other games, larger than minimum winning coalitions are possible.

By NORMAN FROHLICH, Associate Professor of Government, University of Texas, Austin.

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- 947 **Evaluation of a Presidential Election Game.** The Shapley value of the presidential election "game" is approximated by the method of multilinear extensions; the likely error in this approximation is computed by studying the error in the electoral college game. A comparison with the Banzhaf ratio is also given. The several biases inherent in the electoral system are analyzed from a mathematical point of view. It is shown that the system incorporates a strong bias in favor of the larger states, and a small bias in favor of the smallest states.

By GUILLERMO OWEN, Professor of Mathematical Sciences, Rice University.

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- 954 **Vote Trading and The Voting Paradox: A Proof of Logical Equivalence.** Riker and Brams have demonstrated the paradox of vote trading ("... that rational trades by all members [may] make everyone worse off"). In so doing the authors indicate the existence of an apparent disequilibrium when vote trading occurs. I extend this latter point and prove that the preference conditions required for vote trading are the same as those which produce the cyclical majority; the conditions for vote trading and the cyclical majority are logically equivalent. The conclusion briefly indicates the impact of this finding with respect to the work of a number of other authors and gives some idea of the restrictions which would be required to eliminate vote trading among rational legislators.

By DAVID H. KOEHLER, Associate Professor of Government, American University.

**Comment.** By PETER BERNHOLZ, Professor of Economics, Universität Basel, Switzerland.

**Comment.** By JOE OPPENHEIMER, Associate Professor of Government, University of Texas, Austin.

**Rejoinder.** By DAVID H. KOEHLER.

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## 970 COMMUNICATIONS

From Jack A. Goldstone, Marshall Berman, Allan Bloom, Michael C. Stratford, Glenn Tinder

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## 976 EDITORIAL COMMENT

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## 979 BOOK REVIEWS AND ESSAYS

**Crises and Sequences in Collective Theory Development.** Since 1954, the Committee on Comparative Politics has provided leadership in the comparative field, and one of its central objectives has been to construct a theory of political development. The books in the series that were published in the 1960s lacked rigorous design, although they did provide data and low-level generalizations which could be used in the theory-building task. This essay focuses primarily on *Crises and Sequences in Political Development*, which is authored solely by Committee members and reports on the results of their theoretical work thus far. The Committee takes the "intuitive empirical generalization" approach to theory development—in contrast with systematic empirical generalization and the analytic-deductive procedure. It is unlikely, however, that the Committee's approach will lead to the formulation of a coherent set of interrelated propositions within which empirical phenomena can be explained. But the Committee's work is not atypical of the theoretical literature in political science, which reflects the reward structure of the discipline. The building of powerful theories will be facilitated when emphasis is placed on the development of clearly falsifiable propositions rather than on the development of loose conceptual frameworks.

By ROBERT T. HOLT, Professor of Political Science, University of Minnesota, and JOHN E. TURNER, Regents Professor of Political Science, University of Minnesota.

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# The Supreme Court and Critical Elections\*

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Among judicial agencies throughout the world, past and present, the Supreme Court of the United States stands out as a uniquely powerful political institution. No other court has ever been so continuously, consistently, and intimately involved in the policy-making processes of government. Foremost among the Court's interpretive and thus political powers is its power to nullify an act of Congress by declaring it to be unconstitutional.

This uniquely American institution of judicial review, placing the ultimate power of constitutional policy making in the hands of a small group of unelected individuals, has for decades created a disquietude among American legal and political theorists. These scholars have produced a voluminous body of literature, largely impressionistic and prescriptive, seeking to account for the existence of judicial review and, more importantly, attempting to create a consistent theory of its appropriate scope.<sup>1</sup> While so large a volume of theoretical writing cannot be completely reduced to a single intellectual dichotomy, in very general terms the analysts of judicial review have tended to divide, on normative grounds, into two schools of thought, commonly referred to as judicial activism and judicial self-restraint.

It is important to recognize, however, that at base both the activists and the advocates of restraint share a similar conception of the Supreme Court's function in the American political system: Both see the Court as the protector of minority rights against a majority tyranny. The problem, then, to which both the theorists of judicial activism and the philosophers of judicial self-restraint address themselves is: When should the Court interfere with the popular will? Or, put differently, what constitutes unconstitutional action? The advocates of judicial restraint believe that "unconstitutional" must be defined nar-

rowly.<sup>2</sup> In their view, the Constitution is a vague, imprecise charter which allows great leeway to the dominant political forces. The Court must on occasion move to protect a minority, whether it be an ethnic minority, or a religious minority, or a minority of wealthy capitalists irrationally persecuted by a hostile proletariat, but, for reasons of democratic principle or of political prudence, these instances must be infrequent and well justified. Activists, on the other hand, believe the Constitution to be a more precise document; therefore, they define "unconstitutional" more broadly.<sup>3</sup> As a consequence, they believe that the Court must interfere with the popular will more frequently. Moreover, they are more optimistic about the Court's political strengths than are the advocates of restraint, and so they believe that the Court not only *should* but *can* interfere more frequently. It bears emphasis, however, that both schools believe that the Court's function is to protect minority rights.

## The Dahl-Dooley Hypothesis

Several years ago in a justly famed article Professor Robert Dahl argued that the traditional concern of public lawyers about the Supreme Court's power of judicial review was largely unfounded.<sup>4</sup> The Court cannot and does not, Dahl argued, function to protect minorities. With admirable rigor, he demonstrated that seldom, if ever, had the Court been successful in blocking the will of a law-making majority on an important policy issue.

But Professor Dahl's article is perhaps even more significant for what it implied. Dahl per-

<sup>2</sup> See, for example, Herbert Wechsler, "Toward Neutral Principles of Constitutional Law," *Harvard Law Review*, 73 (November, 1959), 1-35; Learned Hand, *The Bill of Rights* (New York: Atheneum, 1965). See generally Bickel, pp. 34-65.

<sup>3</sup> See, for example, Hugo Black, *A Constitutional Faith* (New York: Knopf, 1968); Laurent B. Frantz, "Is the First Amendment Law? A Reply to Professor Mendelson," *California Law Review*, 51 (October, 1963), 729-754. See generally Bickel, pp. 73-98.

<sup>4</sup> Robert Dahl, "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law*, 6 (Fall, 1957), 279-295.

After the final draft of this manuscript was virtually completed, an additional commentary bearing on its thesis became available. David Adamany, "Legitimacy, Realigning Elections, and the Supreme Court," *Wisconsin Law Review* (September, 1973), pp. 790-846. Professor Adamany's article is a thoroughgoing, conscientious—if ultimately wrong-headed—critique of Dahl's work on the Court, as well as that of professors Charles Black and Alexander Bickel.

\* I wish to acknowledge the assistance of the San Diego State College Foundation which partially funded the research reported here, through a Faculty Research Grant. Many persons, both students and colleagues, have contributed to the development of this paper, but two deserve special thanks: Professor Charles Andrain, San Diego State University, and Professor Richard P. Longaker, University of California, Los Angeles. While responsibility for the paper is solely mine, I have greatly profited from these two scholars' comments and suggestions.

<sup>1</sup> See, for example, Leonard Levy, ed., *Judicial Review and the Supreme Court: Selected Essays* (New York: Harper & Row, 1967); Alexander Bickel, *The Least Dangerous Branch: The Supreme Court at the Bar of Politics* (Indianapolis: Bobbs-Merrill, 1962), pp. 34-110.

suasively demonstrated that the Court could not long block the desires of a dominant political coalition, but he merely suggested that the Court would not often *wish* to block the majority will. National politics in the United States, Dahl tells us, has been dominated by relatively cohesive alliances that endure over long periods of time.<sup>5</sup> Because the Court is a political institution whose members are recruited with their political preferences and prejudices in mind, it is inevitably part of the dominant political alliance, except for transitional periods during which the old alliance is crumbling and a new one rising to take its place. During these periods, presumably, the Court, by virtue of the life tenure of the Justices, will be out of step with the political times. Professor Dahl's tabular data, however, while persuasive, do not support his conclusion on this point. Indeed, they are completely unrelated to it. Rather, the conclusion was apparently arrived at impressionistically on the basis of logical deduction. When the Court strikes down congressional legislation as unconstitutional, Dahl demonstrated, it is seldom successful in the long run in thwarting the majority policy. *But the crucial point is that most of the time the Court will not be striking down national legislation.* Dahl did not examine this point at length, nor, therefore, did he consider when the Court would be most likely to act in a counter-majoritarian fashion.<sup>6</sup>

In light of the tremendous expenditures of time and intellectual effort scholars have made in consideration of the problems presented by the institution of judicial review, Professor Dahl's suggestions are relatively iconoclastic and, thus, worthy of more serious examination. I propose, therefore, to do what Dahl did not—that is, to test the following hypothesis: Over long periods of time, the Supreme Court reflects the will of the dominant political forces;<sup>7</sup> however, during transitional periods, in which the Court is a holdover

from the old coalition, the Court will be more likely to perform the counter-majoritarian functions ascribed to it by traditional theory. The hypothesis, in other words, is that, as Mr. Dooley so cryptically put it, "the Supreme Court follows the election returns."

In order to examine the Dahl-Dooley hypothesis, however, some sort of indirect test is clearly necessitated. Essentially the problem we are addressing is: When are Supreme Court decisions most likely to conflict with the will of a national majority? As Dahl himself noted, in any strict sense, there is simply no way of establishing with any high degree of confidence whether a given policy alternative was or was not supported by a majority of Americans.<sup>8</sup> But let us assume, as did Dahl, that one may equate the "law-making majority" with the "national majority."<sup>9</sup> In other words, granting that the connection between one's vote and one's policy preferences is perhaps tenuous, let us, nevertheless, also grant that however tenuous there is at least *some* relation between a voter's preference among candidates or parties and his preferences among alternative public policies.<sup>10</sup> Indeed, recent scholarship suggests that past conclusions about the lack of ideological awareness among the American electorate may have been unique to the Eisenhower era.<sup>11</sup> While the importance of issues in deciding elections will undoubtedly change from one election to another, there is evidence that even in a campaign not notable for its issue saliency most people were concerned with a number of specific issues and that these issue concerns had a measurable effect upon their voting choices.<sup>12</sup>

<sup>5</sup> Dahl, "Decision-Making," pp. 283–284.

<sup>6</sup> Dahl, "Decision-Making," p. 284. See also note 7. While it may never be possible rigorously and definitively to resolve the question of whether a legislature *really* represents a "majority," especially given the evanescence of majorities and the difficulties involved in defining "representation," such evidence as we have suggests that Congress is not markedly out of line with its constituents. See, for example, Warren Miller and Donald Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 45–56.

<sup>7</sup> See V. O. Key, *The Responsible Electorate* (Cambridge, Mass.: Belknap Press, 1966). See also Richard Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), 429–449.

<sup>8</sup> Gerald Pomper, "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review*, 66 (June, 1972), 415–428. But see Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970).

<sup>9</sup> David RePass, "Issue Saliency and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400. But see Nelson Polsby and Aaron Wildavsky, *Presidential Elections: Strategies of American Electoral Politics*, 2nd edition (New York: Scribner, 1971), pp. 293–302.

<sup>5</sup> Dahl, p. 293.

<sup>6</sup> Dahl, pp. 293–295.

<sup>7</sup> Although for purposes of comparability with Dahl I have chosen to equate a "law-making majority" with a "popular majority," this point is not critical to the argument. The coalition dominant within the American political process at any given period may be dominant because of its numbers, or because of its wealth, or because of its monopolization of the media or the instruments of force, or for some other reason. The argument here is that the Court will reflect the values of this dominant coalition. The reasons why that coalition is politically dominant are irrelevant to the hypothesis. Thus, the argument is equally applicable whether one employs a "pluralist" or an "elitist" concept of American politics. See Robert Dahl, *Pluralist Democracy in the United States: Conflict and Consent* (Chicago: Rand McNally, 1967). But see William Connolly, ed., *The Bias of Pluralism* (New York: Atherton, 1969); Peter Bachrach, *The Theory of Democratic Elitism: A Critique* (Boston: Little, Brown, and Co., 1967).

Some, however, will object to equating the preferences of the "law-making majority" with the will of the "national majority" on the grounds that a large segment of the American public does not vote.<sup>13</sup> It will be argued, therefore, that while the elected representatives may quite accurately reflect the will of a majority of the electorate, an "electoral majority" and a "national majority" are two quite separate and distinct entities. This objection, however, if valid, is completely valid only for recent American political history and will not hold as we move back in time. American voter turnout in the nineteenth century was quite impressive.<sup>14</sup> Indeed, some citizens of the frontier were known to be so moved by a sense of civic duty as to exercise their franchise more than once or even from the grave.

If then we may, on this rather tenuous foundation, take a "law-making majority" to be for operational purposes the equivalent of a "national majority," it is possible to test the hypothesis. If we take the preferences of the law-making majority to represent the will of the national majority, and if we take as our operational definition of the preferences of the law-making majority federal legislation passed by majorities in the House and Senate and signed by the president, then we may conclude that the Supreme Court is out of line with the majority will when it holds congressional statutes unconstitutional.<sup>15</sup> We may then examine over time the behavior of the Court in exercising its power to declare congressional statutes unconstitutional in order to determine if this behavior occurs randomly, as the traditional conception of the Court's function would suggest, or if it tends to occur at given, identifiable intervals, as Dahl's thesis implies.<sup>16</sup>

#### Electoral Realignment and the Party System

To examine the hypothesis that the Supreme Court follows the election returns, the first thing to which we should turn are the election returns. Were American national elections, especially presidential elections, mere popularity contests,

there would be a considerable degree of electoral flux or instability in American political history. If a party's victory in a presidential election depended upon nothing more than the personal attractiveness of its candidate, each party would win in random fashion, unless one wishes to assume that one party would have a monopoly on all of the attractive candidates. Examining American political history, however, one does not find that such behavior has been the case. Instead, as Dahl pointed out, Americans' electoral behavior has been quite stable, with one party or another dominating national politics for long periods of time. How to explain such behavior?

The Survey Research Center, of course, explains it by emphasizing the influence of party identity upon a voter's choice between presidential candidates.<sup>17</sup> Elections, according to this view, are essentially reaffirmations of party allegiance. The outcome of any given election may be seen as the result of short-term forces acting upon the underlying distribution of party loyalties within the electorate. The study of electoral behavior, then, becomes the study of the flow of partisan allegiances rather than the study of single elections.

To facilitate such study, the S.R.C. has, of course, formulated an electoral classification scheme characterizing each presidential election as (1) maintaining, (2) deviating, or (3) realigning, depending upon whether the movement of the vote results in the election of the candidate of the majority or minority party and upon whether the movement of the vote is associated with transient, short-term forces or with a basic shift in long-term partisan loyalties.<sup>18</sup> In a maintaining election, the current pattern of party differences is maintained, and the candidate of the majority party is elected. In a deviating election, the net short-term forces are both to the advantage of the minority party and strong enough to influence the election of its presidential candidate, but the electorate's

<sup>17</sup> Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: Wiley, 1966).

<sup>18</sup> Campbell et al., pp. 63-77. The seminal work on cyclical electoral patterns was, of course, V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3-18. For a most sophisticated attempt to synthesize the work on political party systems, explaining election cycles in general theoretical terms, see Thomas P. Jahnige, "Critical Elections and Social Change: Towards a Dynamic Explanation of National Party Competition in the United States," *Polity*, 3 (June, 1971), 465-500.

It bears emphasis that, given the still very crude state of the art, the Survey Research Center's classification scheme is applicable only to presidential elections. If and when it can be refined and extended to other elections, e.g., congressional; it will much further advance the study of the relationship between social change and institutional response.

<sup>13</sup> But see William Andrews, "American Voting Participation," *Western Political Quarterly*, 19 (December, 1966), 639-652.

<sup>14</sup> Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March, 1965), 7-28.

<sup>15</sup> The existence of the institution of federalism in America introduces certain problems for this analysis as indeed it does for a great many other analyses of American politics. I have chosen to limit the analysis to cases involving federal statutes on the grounds that it is logically impossible to conclude anything about the relationship between the Supreme Court and "majority" rule on the basis of cases involving state statutes.

<sup>16</sup> But see note 67. The limited nature of the data will permit of only very modest conclusions.

underlying pattern or distribution of partisan loyalties is unaffected. In a realigning election, however, popular political feeling is so intense that the basic underlying patterns of the electorate's party identifications are changed, and a new party balance is created which endures over several decades.<sup>19</sup> Such fundamental shifts in national political alignments, usually associated with national crises, are not, of course, actually accomplished in a single election. One, instead, must speak of realignment phases or of a realigning electoral era.<sup>20</sup> For example, the partisan realignment which produced New Deal Democracy was begun by Al Smith in 1928, when he changed the image of the Democratic party from that which William Jennings Bryan had given it and captured small pluralities in the populous, Catholic, urban Northeast; it was completed by Roosevelt's sweep in 1936.<sup>21</sup> Naturally enough, there have been very few realigning phases in American political history.<sup>22</sup>

But what has this to do with the Supreme Court? Simply this—this conceptual framework allows us to gauge voter preference over time, and, it will be recalled, we are using voter preference as an indicator of the majority will. If we construct a simple, chronological chart of the ebb and flow of electoral patterns and compare it with the behavior of the Court in exercising its power of judicial review, we shall then be able to test the Dahl-Dooley thesis that the Court is never long out of line with the dominant political coalition, except during transitional periods, which we shall identify as realignment phases.

First, however, an intermediate step may be

taken. It may be useful to determine for whom and for what the electorate voted, since our operational definition of the majority will is not simply the electoral preference but rather the action of the law-making majority. Fortunately, for this purpose, Professors William Nisbet Chambers and Walter Dean Burnham have already applied the S.R.C. classification scheme to a study of the development of American political parties.<sup>23</sup> Chambers and Burnham's study of the history of American voting alignments reveals that over time American party politics has experienced five major patterns of party competition or, as Chambers and Burnham call them, five national party systems.<sup>24</sup>

The first national party system identified by Chambers and Burnham existed from 1789 until 1820. This really was a transitional phase between the establishment of the American State and the acceptance of and regularization of its political patterns.<sup>25</sup> Because those roles or functions which political parties could or should play were not yet well defined, understood, or accepted, the party system which developed in the 1790s exhibited certain peculiarities which set it off from all succeeding party systems. The parties were much more centrally organized than today; and, because the idea of a political opposition as legitimate was not yet firmly entrenched, each of the opposing political coalitions viewed the other as subversive, resulting in an abnormally intense ideological behavior not usually characteristic of American party politics.

Chambers and Burnham, like most contemporary American historians, discount the importance of Jefferson's election in 1800. It certainly did not constitute a realignment with any significance for national policy making. Despite Jefferson's lib-

<sup>19</sup> See also Gerald Pomper, "Classification of Presidential Elections," *Journal of Politics*, 29 (August, 1967), 535-566, distinguishing between realigning and converting elections depending upon whether the dominant party of the previous era continues its dominant status.

<sup>20</sup> See James L. Sundquist, "Whither the American Party System?", *Political Science Quarterly*, 88 (December, 1973), 559-581.

<sup>21</sup> See Ruth Silva, *Rum, Religion, and Votes: 1928 Re-Examined* (University Park: Pennsylvania State University Press, 1962); Samuel Eldersveld, "The Influence of Metropolitan Party Pluralities in Presidential Elections since 1920: A Study of Twelve Key Cities," *American Political Science Review*, 43 (December, 1949), 1189-1206.

<sup>22</sup> Depending upon whose count one uses, there have been three, four, or six. Compare Campbell et al., *Elections and the Political Order* with William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party Systems: States of Political Development* (New York: Oxford, 1967); and James L. Sundquist, *Dynamics of the Party System: Alignment and Realignment of Political Parties in the United States* (Washington, D.C.: Brookings, 1973); and Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, 29 (Spring, 1965), 16-37.

<sup>23</sup> Chambers and Burnham, *American Party Systems*; see also Sundquist, *Dynamics of the Party System*. But see Douglas Price, "'Critical Elections' and Party History: A Critical View," *Polity*, 4 (December, 1971), 236-242.

<sup>24</sup> The following brief summary of the findings reported in *American Party Systems* relies heavily upon Professor Burnham's concluding essay in that volume. For a more extensive treatment, the reader is urged to consult Burnham, "Party Systems and the Political Process," in *American Party Systems*, pp. 277-307. See also Arthur Schlesinger, Jr., ed., *The Coming to Power: Critical Presidential Elections in American History* (New York: Chelsea House, 1971). The general patterns identified by Chambers and Burnham are essentially the same as those advanced by Jahnige, "Critical Elections and Social Change," p. 468.

<sup>25</sup> See Paul Goodman, "The First American Party System," in *American Party Systems*, pp. 56-89. See also Joseph Charles, *The Origins of the American Party System* (New York: Harper & Row, 1961); William Nisbet Chambers, *Political Parties in a New Nation: The American Experience, 1776-1809* (New York: Oxford, 1963).

eral, laissez-faire statements, there was no appreciable change in national economic policy; neomercantilism remained the order of the day. Likewise, the Republican leadership appears to have been hardly less elitist than that of the Federalists.<sup>26</sup> These similarities, coupled with the fact that national elections remained largely undemocratized, may account for the collapse of the first party system. By 1820, when Monroe ran unopposed for the presidency, there evidently were insufficient points of political conflict to sustain a system of national, multiparty competition.

The emergence of recognizably modern parties occurred during the second American party system, following a partisan realignment in response to the election of John Quincy Adams.<sup>27</sup> The growing popular demand for democratization of the franchise coincided with popular revulsion at the "deal" by which Adams was elected from within the House of Representatives and coalesced around the charismatic figure of Andrew Jackson. The resulting system of partisan competition contributed to the decentralization of political power in America. The period saw the rise of the convention and the decline of the centralized congressional caucus as a mechanism for the selection of presidential candidates. Under Jackson and his successors, the federal government progressively declined in power vis-à-vis the states, eventually completely withdrawing from intervention in the economic sector. In part this was due, of course, to the rise of slavery as a political issue.<sup>28</sup> The pursuit of any active federal economic policy necessarily exacerbated the emerging sectional conflict. As a corollary, the presidency experienced a decline in prestige at the expense of the locally-elected Congress. A further consequence of this democratization and decentralization of the political process, however, was the pragmatic, nonprogrammatic character of both major parties. Because the two national parties lacked consistent, coherent principles the second American party system was extremely unstable. Both the Whigs and the Democrats were motley collections of contradictory elements which were incapable of reconciling within themselves the

divisive influence of the sectional conflict over slavery.<sup>29</sup>

For this reason, an entirely sectional party emerged. The central reality of the realignment which produced the Republican party was the restructuring of all political relationships along sectional lines. Were the cancer of slavery to be contained in order that it might be removed from the body politic, this was the only institutional arrangement possible. By the 1850s, a textbook example of a prerevolutionary situation had developed in the United States.<sup>30</sup> New elites, representing the urban, industrial Northeast, were challenging the hegemony of the old, southern, agrarian elite which had dominated national policy making under the Jacksonian system. Were the Union to survive, entirely new ways of doing things had to be adopted, created, or invented. Certainly the period immediately following the Civil War was one of great ferment, experimentation, and change. Not only was slavery abolished but also, for a time, attempts were made to involve the national government in a program of elevating the freedman to a position of first-class citizenship, while simultaneously the federal government initiated a series of measures designed to foster economic expansion.<sup>31</sup>

By the 1870s, however, the radical phase of the third party system had collapsed. This collapse was ratified in the infamous bargain of 1876 by which, in exchange for the White House, the Republican party abandoned its efforts to create a viable Republican structure in the South. For the next twenty years, the division of partisan competition was so close that national political life was virtually stalemated. At the same time, political institutions generally were declining in significance relative to, or were being overshadowed by, the changes being wrought in the society at large by the corporate-industrial revolution.<sup>32</sup>

These changes were, in turn, rendering the arrangements of the third party system obsolete.

<sup>26</sup> Roy F. Nichols, *The Disruption of American Democracy* (New York: Free Press, 1967).

<sup>27</sup> Burnham, "Party Systems and the Political Process," pp. 295-296; Charles A. Beard and Mary Beard, *The Rise of American Civilization* (New York: Macmillan, 1927), pp. 52-122.

<sup>28</sup> See Kenneth Stampp, *The Era of Reconstruction, 1865-1877* (New York: Alfred A. Knopf, 1965); James G. Randall and David Donald, *The Civil War and Reconstruction* (Boston: Heath, 1961).

<sup>29</sup> See Robert McCloskey, *American Conservatism in the Age of Enterprise, 1865-1910* (Cambridge, Mass.: Harvard University Press, 1951); Richard Hofstadter, *The American Political Tradition and the Men Who Made It* (New York: Random House, 1948), pp. 164-205; Sidney Fine, *Laissez Faire and the General-Welfare State: A Study of Conflict in American Thought, 1865-1901* (Ann Arbor, Mich.: University of Michigan Press, 1956).

<sup>23</sup> See Leonard Levy, *Jefferson and Civil Liberties: The Darker Side* (Cambridge, Mass.: Harvard University Press, 1963); Walter Berns, "Freedom of the Press and the Alien and Sedition Laws: A Reappraisal," *The Supreme Court Review* (1970), pp. 109-160.

<sup>27</sup> See Richard McCormick, "Political Development and the Second Party System," in Chambers & Burnham, *American Party Systems*, pp. 90-116.

<sup>28</sup> Burnham, "Party Systems and the Political Process," pp. 292-295. See also Dwight L. Dumond, *Antislavery Origins of the Civil War in the United States* (Ann Arbor, Mich.: University of Michigan Press, 1959); Roy F. Nichols, *The Stakes of Power, 1845-1877* (New York: Hill and Wang, 1961).

The old politics was structured in such a way that it was incapable of taking into account certain demographic and economic changes of major importance. The result was to exclude by definition certain groups from participation in the political process. In particular, powerlessness was the lot of the western farmers and a growing, urban, immigrant proletariat.<sup>33</sup> Rationally, these two groups should have joined forces against the conservative, industrial elite which had come to dominate both major parties within the third party system. This did not happen, however. For complex reasons of structural weakness, one of the excluded groups, the western farmers, managed to capture one of the two major parties, the Democratic, without the assistance of the immigrants in the Northeast. But the populism of William Jennings Bryan not only was based upon opposition to industrial capital but also contained an element of nativist hostility toward the "new immigration."<sup>34</sup> The immigrants responded by shunning a Democratic party openly prejudiced against them. Moreover, in 1893 the country's worst economic depression to that date occurred while a Democrat was President. The response of the industrial workers was the same as it was to be in 1932, an electoral abhorrence of the party in power. The consequent political realignment produced a system of noncompetition, nonparticipation, one-party states, one-party sections, and Republican dominance which lasted until the late 1920s.<sup>35</sup>

The realignment that produced the winning coalition of rural and urban underprivileged, labor, ethnic minorities, and the academic elite which we associate with Franklin Roosevelt actually was presaged by the Smith campaign of 1928.<sup>36</sup> The "Happy Warrior" was able to wrest control of the Democratic party from the southern-rural-colonial forces which had dominated it; his campaign attracted a huge bloc of new immigrant voters into the political system, precipitating a realignment in the urban Northeast. The stable sectionalism of the fourth party system was then

entirely shattered a year later by the force of the Great Depression.<sup>37</sup>

The consequences of this last realignment in American politics are familiar to all. A bureaucratized, welfare-warfare state became the major reality of American politics. The Democratic party replaced the GOP as the dominant party in a system of partisan competition based upon appeals to economic self-interest and group identification.<sup>38</sup> A mixed system of welfare capitalism has emerged, within which the federal government is actively involved in the creation, promotion, and perpetuation of countervailing sources of power, and the executive branch has moved into the ascendancy as the center of policy planning and initiation.

Some analysts have speculated that we are experiencing yet another realignment.<sup>39</sup> But, although some evidence has been adduced which would support that thesis,<sup>40</sup> the S.R.C. electoral classification scheme is not a predictive device; it is not possible, therefore, to say with any certainty whether we are or are not in a realigning phase, let alone what the consequences of such a realignment might be.

#### Periodicity in Supreme Court Behavior

The fact that the S.R.C. electoral classification scheme can be validly and profitably applied to a study of the development of American party politics raises the possibility that it might with equal profit be applied to a discussion of the development of American constitutional politics. Interestingly enough, many students of the Court have remarked that its history has been delimited by five fairly specific periods.<sup>41</sup> But attempts to

<sup>33</sup> See William Leuchtenberg, *Franklin D. Roosevelt and the New Deal* (New York: Harper & Row, 1963).

<sup>34</sup> See Theodore Lowi, *The End of Liberalism* (New York: Norton, 1969).

<sup>35</sup> Richard Scammon and Benjamin Wattenberg, *The Real Majority* (New York: Coward, McCann & Geoghegan, 1970); Kevin Phillips, *The Emerging Republican Majority* (New Rochelle, New York: Arlington House, 1969).

<sup>36</sup> See Samuel Lubell, *The Hidden Crisis in American Politics* (New York: W. W. Norton & Co., 1970); Herbert Weisberg and Jerrold Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (December, 1970), 1167-1185. But see Sundquist, "Whither the American Party System?," and *Dynamics of the Party System*. Rather than a realignment, Sundquist anticipates a reversal of the current trend toward party disintegration and a new strengthening of party attachments along the lines of cleavage established in the New Deal era. As such, Sundquist seems to have in mind what Pomper would call a "converting" era. See Pomper, "Classification of Presidential Elections."

<sup>41</sup> See, for example, Leonard Levy, ed., *American Constitutional Law: Historical Essays* (New York: Harper & Row, 1966), pp. 1-9; Robert Scigliano,

<sup>33</sup> See Harold U. Faulkner, *Politics, Reform, and Expansion* (New York: Harper & Row, 1959); John Hicks, *The Populist Revolt* (Minneapolis: University of Minnesota Press, 1931).

<sup>34</sup> See John Higham, *Strangers in the Land: Patterns of American Nativism, 1860-1925* (New York: Atheneum, 1970).

<sup>35</sup> Burnham suggests that this result was accomplished by the withdrawal from political participation of the urban working class, creating a political void which was filled elsewhere by the rise of socialist parties. Burnham, "Party Systems and the Political Process," p. 301. But see Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace & World, 1955).

<sup>36</sup> See Silva, *Rum, Religion, and Votes*.



employ the voting behavior research in order to correlate the Court's behavior over time with the dynamics of the system at large have been few and inchoate at best,<sup>42</sup> even though such a correlation would provide the "most obviously plausible example of synchronization of institutional-role and policy-output change with critical realignment" in American politics.<sup>43</sup> In each of these five periods of the Court's history, certain characteristic doctrines have prevailed, only to be abandoned in a later period. The Court seems always to have some special interest to protect or value to advance. Have these interests or primary legal norms been opposed to or consonant with the primary values expressed within the larger political system?

The first and formative period of the Supreme Court's history is identified with the Chief Justiceship of John Marshall. Under Marshall, the primary legal norms advanced by the Court were national supremacy,<sup>44</sup> judicial power,<sup>45</sup> and the protection of private property.<sup>46</sup> During Marshall's tenure, the Court labored to establish the supremacy of the national government over the states through broad interpretations of such constitutional provisions as the commerce clause and

the necessary and proper clause.<sup>47</sup> Similarly, it asserted its own power to review the validity of state legislative and judicial actions, thereby increasing not only the power of the federal government generally but also the power of the federal judiciary in particular.<sup>48</sup> Nationalism also proved to be a useful tool for the protection of vested rights. Since most of the statutes which interfered with private property during this era were state laws, interpretations that severely limited states' rights were frequently employed to invalidate such statutes.<sup>49</sup>

There would indeed seem to be a certain coincidence between the values asserted or advanced by the Court under Marshall and the dominant norms of the first American party system identified by Chambers and Burnham. That party system was characterized by the centrality of its organization, and, although more moderate in their elitism than the Federalists, the Republicans were nonetheless committed to the protection of aristocratic privilege, a part of which involved the protection of property rights. Thus, the harmony which developed between Marshall's Supreme Court and Jefferson's successors may have been less than accidental.<sup>50</sup>

Perhaps not surprisingly, Marshall's successor was none other than Andrew Jackson's right-hand man, Roger Brooke Taney. Just as Jackson and his successors dismantled the neomercantilist system and withdrew the federal government from the economic sector, so the Taney Court was less inclined to invalidate state legislation and viewed corporate enterprise with a less hospitable eye

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*The Supreme Court and the Presidency* (New York: Free Press, 1971), p. viii. See also Glendon Schubert, *Judicial Policy-Making* (Glenview, Illinois: Scott, Foresman, 1965), pp. 131-157.

<sup>42</sup> Adamany, "Legitimacy, Realignment Elections, and the Supreme Court," is really more suggestive than it is rigorously empirical.

Sheldon Goldman and Thomas P. Jahnige have briefly examined the coincidence between periods of electoral realignment and the Court-curbing periods identified by Stuart S. Nagel. They have found a distinct correlation. But such a focus is, by definition, concerned not with the Court's behavior but with the actions of others directed at the Court. Sheldon Goldman and Thomas P. Jahnige, *The Federal Courts as a Political System* (New York: Harper & Row, 1971), pp. 261-268; see also Stuart S. Nagel, *The Legal Process from a Behavioral Perspective* (Homewood, Ill.: Dorsey Press, 1969), pp. 260-279.

Nagel and Wallace Mendelson have discussed the effect for Supreme Court policy making of party differences between Congress and the Court, but their work has not been guided by critical election theory. Nagel, *The Legal Process*, pp. 245-259; Mendelson, "Judicial Review and Party Politics," *Vanderbilt Law Review*, 12 (March, 1959), 447-457.

<sup>43</sup> Burnham, *Critical Elections*, p. 10, fn. 15.

<sup>44</sup> See, for example, *Fletcher v. Peck*, 6 Cranch 87 (1810); *McCulloch v. Maryland*, 4 Wheaton 316 (1819).

<sup>45</sup> See, for example, *Marbury v. Madison*, 1 Cranch 137 (1803); *Cohens v. Virginia*, 6 Wheaton 264 (1821).

<sup>46</sup> See, for example, *Dartmouth College v. Woodward*, 4 Wheaton 518 (1819); *Sturges v. Crowninshield*, 4 Wheaton 122 (1819).

<sup>47</sup> See, for example, *Gibbons v. Ogden*, 9 Wheaton 1 (1824); *Brown v. Maryland*, 12 Wheaton 419 (1827); *McCulloch v. Maryland*, 4 Wheaton 316 (1819).

<sup>48</sup> See, for example, *Fletcher v. Peck*, 6 Cranch 87 (1810); *Martin v. Hunter's Lessee*, 1 Wheaton 304 (1816); *Cohens v. Virginia*, 6 Wheaton 264 (1821).

<sup>49</sup> See, for example, *Fletcher v. Peck*, 6 Cranch 87 (1810); *Dartmouth College v. Woodward*, 4 Wheaton 518 (1819); *Sturges v. Crowninshield*, 4 Wheaton 122 (1819).

<sup>50</sup> Differences of personality and temperament as much as differences of politics may account for the enmity which existed between the Court headed by Marshall and the executive branch headed by Jefferson. With the removal of Jefferson from the White House and thus the removal of the personality factor, the Marshall Court and the succeeding Republican presidents got along quite well. See Richard Ellis, *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (New York: Oxford, 1971); Alfred Kelly and Winfred Harbison, *The American Constitution: Its Origin and Development* (New York: Norton, 1970), pp. 202-247; Julian Boyd, "The Chasm That Separated Thomas Jefferson and John Marshall," in *Essays on the American Constitution*, ed. Gottfried Dietze (Englewood Cliffs, New Jersey: Prentice-Hall, 1964), pp. 3-20.

than had Marshall.<sup>51</sup> During this period, the federal government declined in power vis-à-vis the states, and the presidency experienced a precipitate drop in prestige. Although the Court under Taney did not suffer an equivalent decline, it did behave similarly in that it withdrew to a position somewhat more like an impartial arbiter between states and nation, rather than functioning, like the Marshall Court, as an active champion for the central government. Finally, just as the second party system was characterized by an extreme sectionalism of voting patterns, so too the Court under Taney increasingly came to divide along sectional lines in its disposition of cases, with the southerners, as in the other two branches, holding a slim but stable majority.<sup>52</sup>

The infamous *Dred Scott* decision and its subsequent repudiation upon the battlefield brought about a decline in the Court's prestige which lasted throughout the decade of the 1860s. As a result, the Supreme Court did not participate in the radical phase of the third party system. The program which was initially followed by the Republican victors of the realignment of 1852-1860 was twofold: on the one hand, a policy of federal protection of civil rights, especially those of the newly freed blacks; on the other, a policy of federal intervention in the economy in order to encourage the development of commercial and corporate enterprise. By 1870, the first half of this program had been abandoned. This abandonment of the black man by the Republican leadership was formalized in the bargain of 1876 which produced a sectional, partisan deadlock that lasted for the next decade and a half. This solution came to be tacitly accepted by public opinion, and in the 1880s was recognized as legitimate by the Supreme Court in a series of decisions which emasculated most of the comprehensive radical program for the protection of the Negro.<sup>53</sup> On the other hand, the Court, having regained its strength and prestige, did its part in advancing the second half of the Republican program, federal encouragement of industrial capitalism.<sup>54</sup> Not only did the Court thus validate the practices of the dominant political coalition but also it laid the groundwork for new doctrines of constitutional limitation which might be employed to protect corporate

property if the dominant political values were to change.

If anything, the Supreme Court *anticipated* the electoral realignment which brought about the fourth party system. By 1893, as already noted, the stage was set for a political coalition between the western farmers and the immigrant laborers. Such a coalition, of course, did not occur. But, before it was clear that this would be the case, while the industrial elite still feared such a coalition might prevail, the Supreme Court in a series of classic decisions in 1894-95 did what it could to undermine the legitimacy of a massive assault on established elite rule.<sup>55</sup> When it became clear, however, that the feared coalition would not emerge and that the result of the realignment of 1888-1896 was to elevate McKinley Republicanism to a position of preeminence in the political system, the Court began a campaign completely in consonance with the values represented by the dominant party: laissez-faire at home and imperial dominance abroad. With respect to the latter, the Court, in a series of decisions at the turn of the century, validated America's rise to the position of imperial power, holding in essence that the Constitution allowed Americans to "take up the white man's burden."<sup>56</sup> As for the domestic principle of laissez-faire economics, this period saw the Court employ the due process clause of the Fourteenth Amendment as a formidable weapon against an assortment of state legislation that sought to protect consumers, unions, farmers, unorganized labor, women, and children against the abuses of business enterprise.<sup>57</sup> The high point of the Court's attempts to control public policy was, of course, reached during seventeen months in 1935-36, when the Court launched a frontal attack upon the New Deal.<sup>58</sup> Public opinion, though opposed to "packing" the Court, was outraged, and the Court beat a strategic, if belated, retreat, abandoning its role as economic policy maker.<sup>59</sup>

<sup>51</sup> For example, *United States v. E. C. Knight Co.*, 156 U. S. 1 (1895); *Pollock v. Farmers' Loan & Trust Co.*, 157 U. S. 429 (1895); *Pollock v. Farmers' Loan & Trust Co.*, 158 U. S. 601 (1895). See Alan Westin, "The Supreme Court, The Populist Movement and The Campaign of 1896," *Journal of Politics*, 15 (February, 1953), 3-41.

<sup>52</sup> For example, *DeLima v. Bidwell*, 182 U. S. 1 (1901); *Downes v. Bidwell*, 182 U. S. 244 (1901); *Dorr v. United States*, 195 U. S. 138 (1904); *Rasmussen v. United States*, 197 U. S. 516 (1905).

<sup>53</sup> For example, *Smyth v. Ames*, 169 U. S. 466 (1898); *Lochner v. New York*, 198 U. S. 45 (1905); *Coppage v. Kansas*, 236 U. S. 1 (1915).

<sup>54</sup> For example, *Schechter Bros. Poultry Corp. v. United States*, 295 U. S. 495 (1935); *United States v. Butler*, 297 U. S. 1 (1936); *Carter v. Carter Coal Co.*, 298 U. S. 238 (1936).

<sup>55</sup> See Robert Stern, "The Problems of Yesteryear—Commerce and Due Process," *Vanderbilt Law Re-*

<sup>51</sup> See, for example, *Mayor of New York v. Miln*, 11 Peters 102 (1837); *Charles River Bridge Co. v. Warren Bridge Co.*, 11 Peters 420 (1837); *Bank of Augusta v. Earle*, 13 Peters 519 (1839).

<sup>52</sup> See, for example, *Dred Scott v. Sandford* 19 Howard 393 (1857).

<sup>53</sup> For example, *Civil Rights Cases*, 109 U. S. 3 (1883).

<sup>54</sup> *Knox v. Lee*, 12 Wallace 457 (1871); *Chicago, Milwaukee & St. Paul Ry. Co. v. Minnesota*, 134 U. S. 418 (1890).

Table 1. Relations Between Electoral Realignment, American Party Systems, and Supreme Court Decision Making

Date	Party System	Judiciary (by Chief Justice)	Exemplary Cases
1800	Jeffersonian Republican	Marshall	
1820			
Realignment phase			
1828	Jacksonian Democracy	Taney	Worcester v. Georgia 6 Peters 515 (1832)
1852			Dred Scott v. Sandford, 19 Howard 393 (1857)
Realignment phase			
1860	Radical Republican	Chase and Waite	
1888			Pollock v. Farmers' Loan & Trust Co., 157 U.S. 429 (1895)
Realignment phase			
1896	McKinley Republican	Fuller, White, Taft, and Hughes	
1928			Schechter v. U.S., 295 U.S. 495 (1935), U.S. v. Butler, 297 U.S. 1 (1936)
Realignment phase			
1936	New Deal Democracy	Stone, Vinson, and Warren	[Lamont v. Postmaster General, 381 U.S. 307 (1965); Shapiro v. Thompson, 394 U.S. 618 (1969)]
????			

In the most recent period of the Court's history, constitutional law has been brought into accord with the norms of the New Deal Democrats.<sup>60</sup> The precedents of economic due process have been scuttled, and the authority of the government to intervene in the economy is virtually undisputed. From the New Deal to the Great Society, the Court has sustained the welfare state. At the same time, the Court has liberalized the constitutional law of civil liberties and, in particular, has advanced on many fronts the value of egalitarianism, the doctrine of the "common man."

On the basis of this cursory analysis, it would certainly seem that the Supreme Court does follow the election returns over time. Table 1, moreover, suggests that some of the Court's most notable collisions with the elected branches have

indeed occurred during periods of partisan realignment.<sup>61</sup> The analysis, however, may be advanced with somewhat more rigor by examining the data statistically.

First of all, it should be made clear that the time span to be used for this exercise begins in 1801, with the appointment of the great Chief Justice, John Marshall, and ends in June, 1969, with the retirement of Mr. Chief Justice Warren. Thus, the overall period covers approximately 168 years.<sup>62</sup> The initial demarcation has been limited to 1801 for two reasons. First, the presence of Washington during the initial eight years of the Republic's life caused partisan competition for national office to

<sup>60</sup> While I have examined the relation between judicial decision making and the party systems during maintaining periods of stable partisan competition, Adamany has focused upon the relationship between the Court and the elected branches during realignment phases. Adamany, "Legitimacy, Realignment Elections, and the Supreme Court," pp. 820-843. His historical survey tends to substantiate the thesis advanced here, although the conclusions which he draws from his analysis differ from my own.

<sup>62</sup> To be perfectly precise, the time span from Marshall's appointment on January 31, 1801, to Warren's retirement on June 23, 1969, is 168 years, 4 months, and 23 days.

view, 4 (April, 1951), 446-468; Robert McCloskey, "Economic Due Process and the Supreme Court: An Exhumation and Reburial," *The Supreme Court Review* (1962) pp. 34-62.

<sup>60</sup> See Philip B. Kurland, *Politics, the Constitution, and the Warren Court* (Chicago: University of Chicago Press, 1970); Arthur S. Miller, *The Supreme Court and American Capitalism* (New York: Free Press, 1968); Lowi, *The End of Liberalism*.

remain *sub rosa*, and the party system itself had little chance to develop. The two elections of Washington and the election in 1796 of Adams are so exceptional as to defy classification under the S.R.C. scheme.<sup>63</sup> Second, until the appointment of Marshall to the bench, the Supreme Court was hardly a coordinate branch of the government. It handled very few cases and was so lacking in power and prestige that many men of distinction refused to serve on it.<sup>64</sup> With Marshall's appointment and Jefferson's inauguration, however, both the Court and the party system appear to have become well enough established that the electoral classification scheme can be applied to the study of both with some degree of validity. Since both of these events occurred in 1801, that date has been selected as the starting point. Any statistical advantage which may be gained by chopping off the first twelve years of the nation's history is probably lost by carrying the analysis through the 1968 Term of Court. If we are in a realigning phase, then at least some of the seventeen cases decided since 1960 in which the Warren Court declared federal legislation to be unconstitutional should be counted as coming within a transitional period. Because the S.R.C. classification scheme is not predictive, however, these cases have been counted as occurring during a period of stable party competition. If anything, this should skew the data against the hypothesis even more than the elimination of the 12 years from 1789 to 1801 would have biased the analysis in its favor.

According to the way in which I count the cases, there have been ninety-four during this 168-year period which are of interest to us, i.e., cases in which the Court declared federal legislation to be unconstitutional either in whole or in part.<sup>65</sup> Table 2 presents the total incidence of the Supreme Court's exercise of its power of judicial review in relation to the realignment phases identified by Chambers and Burnham utilizing the Survey Research Center's electoral classification scheme. Absolute figures, however, do not tell us very much. Since we are dealing here with a case of concurrent variation in a paired time series, what is required is a measurement of central tendency. During realignment phases or, as they might be called, critical periods, was the tendency of the Court to declare federal legislation unconstitutional significantly greater than during noncritical periods of stable party competition? To

<sup>63</sup> Indeed, Sellers, "The Equilibrium Cycle," classifies the two elections of Washington as "deviating" elections, a classification which poses certain logical difficulties.

<sup>64</sup> See Robert Steamer, *The Supreme Court in Crisis: A History of Conflict* (Boston: University of Massachusetts Press, 1971), pp. 3-23.

<sup>65</sup> Appendices fully elaborating the data base are available from the author upon request.

**Table 2. Incidence of all Supreme Court Cases Declaring Congressional Legislation Unconstitutional**

	Total Years	Number of Cases	Index
Critical periods <sup>a</sup>	32	21	0.66 <sup>c</sup>
Noncritical periods <sup>b</sup>	136	73	0.54 <sup>d</sup>
Coefficient of Counter-Majoritarianism <sup>e</sup>			1.2

<sup>a</sup> As identified by Chambers and Burnham. Chambers and Burnham identify four "critical periods" of partisan realignment; while each of these realignment phases is not precisely eight (8) years in length, the author has adopted eight-year periods for purposes of statistical simplicity and comparability. This period, obviously divisible by four, is long enough to allow for broad-scale partisan realignment to manifest itself, yet still short enough to be meaningful within the context of this study.

<sup>b</sup> As identified by Chambers and Burnham.

<sup>c</sup> The index is obtained by calculating the ratio of years of critical periods to the total number of cases within those periods to obtain the central tendency.

<sup>d</sup> The index is obtained by calculating the central tendency of the ratio of years of noncritical periods to the number of cases declaring federal legislation to be unconstitutional within those periods.

<sup>e</sup> The coefficient is obtained by dividing the index for critical periods by the index for noncritical periods, thus arriving at a single number.

answer that question, all that is necessary is to calculate the arithmetic mean for the total years of critical and noncritical periods.<sup>66</sup> The two indexes for all Supreme Court cases declaring acts of Congress unconstitutional are shown in Table 2. As expected, the index for the critical periods is somewhat larger than the index for the noncritical periods, but the difference is not particularly startling. On that basis, one might conclude that during critical periods the Court was slightly more likely to behave in a counter-majoritarian manner—but not very much more likely.<sup>67</sup>

<sup>66</sup> More sophisticated statistical techniques such as scatter-diagrams could, of course, be employed to make the same point. At this very initial level of analysis, however, I have opted to keep the statistics as simple as possible. Refinements can, of course, always be introduced later.

<sup>67</sup> Candor compels the admission that there is a general lack of dispersion of cases across the four critical periods. As an examination of the appendices demonstrates, the majority of them occurred during the 1928-1936 realignment. While the New Deal's difficulties with the Court were extraordinary, however, one must remember that the Court's opportunities to exercise its power of judicial review were enhanced by a burgeoning population, the vast increase in congressional legislation, and the Court's expanding docket. See Alexander Bickel, *The Caseload of the*

By using *all* cases in which federal legislation has been held unconstitutional in whole or in part, however, we are not really measuring what we set out to measure. For example, in *Myers v. U. S.*, decided in 1926, the Court declared unconstitutional an act which had been passed fifty years earlier.<sup>68</sup> In other words, here we have a Court on the eve of the realignment of 1928–36 declaring void a statute passed many years before the realignment of 1888–1896. What can one conclude from this case about the relationship between the *Myers* Court and the law-making majority? Obviously, nothing. Therefore, as did Dahl,<sup>69</sup> let us confine ourselves to cases which held provisions of federal legislation to be unconstitutional within four years of their enactment. We may then assume with a fair degree of confidence that the law-making majority which passed the legislation is not a dead one and that, if the Court acts against that sort of majority it is in fact acting counter to prevailing majority sentiment.

Within the 168 years with which we are concerned, there have been 38 cases in which the Court has declared provisions of federal legislation unconstitutional within four years of enactment.<sup>70</sup> (See Table 3.) Sixteen have occurred during the 32 years of partisan realignment identified by Chambers and Burnham as critical periods. Twenty-two have occurred during the other 136 years. If one now calculates the central tendencies of the Court, one arrives at an index for critical

Table 3. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment

	Total Years	Number of Cases	Index
Critical periods	32	16	0.50
Noncritical periods	136	22	0.16
Coefficient of Counter-Majoritarianism			3.1

periods of 0.50 and an index for noncritical periods of only 0.16. Which is to say that, as hypothesized, there has been a significantly greater tendency on the part of the Court during realigning periods to utilize its power to declare federal legislation unconstitutional, i.e., to oppose the law-making majority will, than during periods of political stability, when the Court is part of the dominant political majority.

How much greater? To answer this question we need but to compute a measure of relative variability. The simplest statistical measure of relative variability is, of course, the coefficient of deviation. This coefficient, which for want of a better title, I shall label in this case the Coefficient of Counter-Majoritarianism, is obtained by dividing one index by the other. Calculating the Coefficient of Counter-Majoritarianism for Table 3, we arrive at a figure of 3.1. In other words, statistically-speaking the Court has been more than three times as likely to declare recently enacted federal legislation unconstitutional during the realignment periods identified by Chambers and Burnham than it has been during the vast majority of its history.

Calculation of the Coefficient of Counter-Majoritarianism allows another important comparison. Examining Tables 2 and 3, one finds that using Chambers and Burnham's classifications to plot all of the 38 cases which held federal legislation to be unconstitutional within four years of enactment produces an index for critical periods of 0.50; on the other hand, using Chambers's and Burnham's scheme to plot all of the 94 cases which have ever held federal legislation unconstitutional yields an index for critical periods of 0.66. Does this mean that it is preferable for purposes of the demonstration to use all 94 cases? Obviously, no; for by increasing the number of cases one not only slightly increases the index for the critical periods but also one greatly increases the index for the noncritical periods. Because the statistical bases are different, the two indexes for critical periods simply are not comparable. Coefficients of deviation, however, are, and a comparison of the

*Supreme Court: And What, If Anything, To Do About It* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973). If one discounts the number of judicial review cases by the total incidence of opportunities presented for the exercise of the power of judicial review, the New Deal era appears to be less unique, though it remains one of great judicial activity.

<sup>68</sup> 271 U.S. 52 (1926).

<sup>69</sup> Dahl, "Decision-Making," pp. 286–291. I have not, however, adopted Dahl's distinction between cases dealing with major policy issues and those dealing with minor, because I am persuaded by criticisms of his attempts in other areas of analysis to distinguish between important and unimportant policy matters as definitional sleight of hand subject to serious theoretical reservations. See Peter Bachrach and Morton Baratz, "Decisions and Nondecisions: An Analytical Framework," *American Political Science Review*, 57 (September, 1963), 641–652; and "Two Faces of Power," *American Political Science Review*, 56 (December, 1962), 947–952. But see Robert Dahl, *Who Governs?* (New Haven, Conn.: Yale University Press, 1961). For purposes of this study, rejection of such a distinction does sacrifice historical nuance. But, as my conclusions indicate, I do not mean to discourage systemic, historical analysis of the Court—indeed, quite the opposite—and I hope that scholars who undertake such studies will have better luck than has Dahl in operationalizing definitions of "major" and "minor" policies.

<sup>70</sup> See note 65.

Coefficients of Counter-Majoritarianism in Tables 2 and 3 suggests a significant consideration about Supreme Court behavior. The Coefficient of Counter-Majoritarianism in Table 2 suggests that the Court is slightly more likely to exercise its power of judicial review during realignment phases than during noncritical periods. The Coefficient of Counter-Majoritarianism in Table 3 means that during realignment periods the Court is 3.1 times as likely to declare recently enacted federal legislation unconstitutional than at other times. In relation to each other, however, the two coefficients suggest that during realignment periods the Court is more than two and one-half times as likely to declare recently enacted legislation unconstitutional than other legislation.<sup>71</sup> In other words, it is not merely that during critical periods of partisan realignment the Court is more likely to declare recently enacted federal legislation unconstitutional than at other times, but that during realigning periods this is the very sort of congressional legislation which is most likely to be nullified by the Court!

One might suppose that this finding would justify acceptance of the Dahl-Dooley hypothesis. But a further modification may elaborate the point. Just as the scope of cases which would be accepted as evidence that the Court was out of line with a prevailing majority was narrowed by limiting it to those cases in which legislation not more than four years old was held unconstitutional, the scope of the critical periods may be expanded. If the Court is out of line with the will of the law-making majority during realignment periods, it would be reasonable to expect that, as the realignment takes place and the newly dominant coalition begins to consolidate its position, the Court will gradually come back into line with that majority by virtue of the recruitment process; or, put in statistical terms, the Coefficient of Counter-Majoritarianism will decrease over time.

In order to examine this proposition, it is necessary to introduce the concept of a lag period.<sup>72</sup> We have been examining the behavior of the Court only during realigning periods when the Court, because of the life tenure of the Justices, is out of step with the dominant trends of the political times. During and immediately following these realigning periods, however, appointments are being made to the Court which serve to bring it back into line with the dominant political coalition. If we take as our definition of "lag period" that period of time which is required for a new majority to be appointed to the Court, we have

<sup>71</sup>  $3.1/1.2 = 2.58$

<sup>72</sup> For a more extensive discussion of the time lag between the coming of new party eras and the restructuring of Supreme Court policy making, see Dahl, *Pluralist Democracy*, pp. 154-170.

**Table 4. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment (Utilizing Modification No. 1)**

	Total Years <sup>a</sup>	Number of Cases	Index
Critical periods	45	16	0.36
Noncritical periods	124	22	0.18
Coefficient of Counter-Majoritarianism			2.0

<sup>a</sup> The expanded critical periods are obtained by using the total years of partisan realignment phases (see Table 2) plus the total number of years necessary for a fifth or majority appointment to be made to the Court after the beginning of each of the realignment phases.

the possibility of calculating two different lag periods. The first possibility is to calculate the lag periods as ending with the fifth or majority appointment to the Court after the *beginning* of the realigning period. Using this possibility, the lag periods end in 1835, 1864, 1896,<sup>73</sup> and 1938.<sup>74</sup> The second possibility is to calculate the lag periods as ending with the fifth or majority appointment to the Court after the *end* of the realignment period. Using this possibility, the lag periods end in 1836, 1864, 1909, and 1940.<sup>75</sup> If the hypothesis is valid, we would expect that the Coefficient of Counter-Majoritarianism would be greatest when one uses as the measure of critical periods only the periods of partisan realignment. The coefficient should be smallest when one uses as the measure of critical periods the expanded eras produced by modifying the periods with the second possible and longest lag period. And it should be somewhere in between these two using the first possible calculation of lag period. In other words, one would suspect that the greater the length of the critical periods, the more the Coefficient of Counter-Majoritarianism would decrease as the Court comes back into line with the dominant law-making majority.

In fact, this is exactly what we do find. Table 4 presents the findings when all cases in which congressional legislation was held unconstitutional within four years of its enactment are plotted against Chambers's and Burnham's critical periods of partisan realignment with the critical

<sup>73</sup> The fifth appointment to the Court made after the beginning of the realignment period, 1888, was made in 1894; thus there is no "lag period." Many students of the Court have observed that in its decision making during this period the Court actually seemed to be anticipating the election returns. This pattern of recruitment suggests why.

<sup>74</sup> See note 65.

<sup>75</sup> See note 65.

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**Table 5. Incidence of Supreme Court Cases Declaring Congressional Legislation Unconstitutional Within Four Years of Enactment (Utilizing Modification No. 2)**

	Total Years <sup>a</sup>	Number of Cases	Index
Critical periods	61	20	0.33
Noncritical periods	107	18	0.17
Coefficient of Counter-Majoritarianism			1.9

<sup>a</sup> The expanded critical periods are obtained by using the total years of partisan realignment phases (see Table 2) plus the total number of years necessary for a fifth or majority appointment to be made to the Court *after the end* of each of the realignment phases.

periods extended by using the first, shortest lag periods. Table 5 illustrates the results produced when the very same cases are plotted against the same classification scheme, with the exception that the critical periods have been extended utilizing the second possible definition of "lag period." In short, during periods of electoral and partisan realignment, the Court, as a result of its life tenure, is most likely to be out of line with the new, dominant law-making majority, which, following Dahl, I have accepted for operational purposes as the index of majority will. As time passes, the new majority is enabled to appoint its own adherents to the Supreme bench, and the Court increasingly returns to harmony with the new law-making majority. Not only is this true logically but also, as the decreasing Coefficients of Counter-Majoritarianism in Tables 3, 4, and 5 illustrate, it is empirically demonstrable. "It is, therefore, accurate to say that the Court will reflect, in an overall, general sense, the values of the dominant political coalition capable of electing the president."<sup>76</sup>

It must be admitted, however, that this study represents but a very tentative first step. Not only does it suggest ways in which concepts and research from other areas of the discipline of political science can be utilized to address one of the classic problems in public law, it also illustrates the methodological and conceptual problems involved in such borrowing. Much more work can and should be done in this area. Different and more sophisticated statistical techniques might be employed to analyze the data presented here; different definitions of realignment periods could be utilized; and Supreme Court behavior in relation

to different categories of cases might be correlated with the electoral realignment data.

Adopting different definitions of realignment phases might involve either the refinement of the periods identified here or the use of other scholars' election classifications.<sup>77</sup> Initial tests using some of these different classification schemes confirm that, while the numbers naturally vary, the overall pattern identified here and the conclusions thus suggested are the same. Unlike natural scientists, however, political scientists too seldom build directly on other scholars' work. Therefore, in the interests of comparability and replicability, I have confined this study to the periods already utilized by Chambers and Burnham in their study of party development. For purposes of this analysis, moreover, the critical periods of partisan realignment have been arbitrarily defined as being eight years in length simply for the sake of neatness and order and because eight is readily divisible by four, the Survey Research Center's electoral classification scheme being applicable only to presidential elections. There is, however, nothing sacrosanct about eight-year periods, and the realignment phases might be more rigorously defined. But, again, narrowing the time spans covered or juggling a few years about has not significantly altered the general pattern reported here. Indeed, the use here of some years before the elections which brought new coalitions to power as part of the realignment phases may very probably have artificially deflated the indexes for the critical periods and, thus, the Coefficients of Counter-Majoritarianism in Tables 3, 4, and 5. While it is reasonable to expect tension between a Court of old coalition Justices and a new coalition president and Congress, with the old coalition in power, as in 1928-1932, one would not expect the enactment of legislation which would provoke a Supreme Court declaration of unconstitutionality.

Perhaps future research might attempt to determine the year of ascendancy of the new coalition. One might, for example, define this as possession of the presidency and then use as the critical periods the time between the election of the new coalition president and his appointment of a presumably sympathetic majority of Justices. Using these as realignment years for the Court, one would find results not unlike those reported here. Reckoning ascendancy by control of Congress would not change this much. Such attempts to determine precise years of ascendancy, however, not only must contend with the scholarly contentiousness about which elections actually involved realignments but also will be liable to the charge that they have defined the critical periods so as to inflate the statistical results. Again, this

<sup>76</sup> Richard Cortner and Clifford Lytle, eds., *Modern Constitutional Law* (New York: Free Press, 1971), p. 12.

<sup>77</sup> See note 22.

study has used Dahl's cases and Chambers and Burnham's classifications in order to achieve a more integrated analysis. If the results reported here can be achieved by using realignment phases not inherently favorable to the thesis, they may perhaps be considered to be even more persuasive.

In a more substantive vein, judicial review decisions are only one of several possible indices of the relationship between Supreme Court value structures and philosophies predominant in the larger political processes.<sup>78</sup> Court decisions challenging presidential actions, for example, might be examined within the framework of the S.R.C. model. The late Mr. Justice Robert Jackson observed that

[The Court] has been in angry collision with most dynamic and popular Presidents in our history. Jefferson retaliated with impeachment; Jackson denied its authority; Lincoln disobeyed a writ of the Chief Justice; Theodore Roosevelt, after his Presidency, proposed recall of judicial decisions; Wilson tried to liberalize its membership; and Franklin D. Roosevelt proposed to "reorganize" it.<sup>79</sup>

But note that at least three of these presidents have been the products of realigning elections.<sup>80</sup> Similarly, despite the operational and conceptual difficulties involved,<sup>81</sup> some scheme might be devised for analyzing the Court's exercise of its power to annul state legislation. In this vein, *Worcester v. Georgia*,<sup>82</sup> the only case in which a president apparently ever refused outright to execute a Supreme Court decision and one which occurred immediately following a realignment period, coupled with recent efforts to develop impact typologies of Supreme Court decisions,<sup>83</sup> is highly suggestive of avenues for future research.

Ultimately, the subject calls for an extended, systematic, historical examination of the Court's decision making explicitly based upon the elec-

toral classification scheme devised by the S.R.C. and elaborated by others, a treatment of the Court similar to that which Chambers and Burnham have done for the parties. The politically dominant values of each period must be identified and compared with judicial opinions through in-depth content and dialectical analyses to determine if the statistically apparent "fit" is present in substance.<sup>84</sup> Any study such as this, based upon aggregate data, necessarily sacrifices situational focus for numerological standardization, but Supreme Court decisions are not fungible goods. Indeed, it is striking that some of those decisions which occasioned the loudest protest in their own day and which appear frequently in the casebooks today did not occur during critical periods of partisan realignment.<sup>85</sup> They are included in the casebooks, of course, for reasons other than the public furor which they provoked, and such popular reaction need not invalidate the view that the Court tends to reflect the dominant political values. For an unexpected reversal by a presumed ally would occasion an intense and bitter response, a sort of "stabbed-in-the-back" syndrome. But whether such a speculative interpretation is justified can be known only through careful, historical case analysis focusing upon the political facts of individual cases.

### Conclusion

The question remains, of course, apart from being an interesting statistical exercise, what significance this has for an understanding of the judicial function. First of all, it suggests that Professors Dahl and Charles Black have been correct in emphasizing the Court's function as a legiti-

<sup>78</sup> See, for example, Kurland, *Politics, the Constitution, and the Warren Court*, p. 26. This analysis has, of course, been limited to cases involving the constitutionality of federal statutes for purposes of comparability with Dahl.

<sup>79</sup> Robert Jackson, *The Struggle for Judicial Supremacy* (New York: Knopf, 1941), pp. ix-x.

<sup>80</sup> Indeed, if one uses Sellers's categorization, five of the six Presidents mentioned by Jackson have come to office on the crest of realigning elections. See Sellers, "The Equilibrium Cycle." In this regard, President Nixon's attacks upon the Court's policy making and his attempts to change it while in office are yet other intriguing data possibly indicative of contemporary electoral realignment. See also Duane Lockard, *The Perverted Priorities of American Politics* (New York: Macmillan, 1971), pp. 234-235.

<sup>81</sup> See note 15.

<sup>82</sup> 6 Peters 515 (1832).

<sup>83</sup> See Stephen Wasby, *The Impact of the United States Supreme Court: Some Perspectives* (Homewood, Ill.: Dorsey Press, 1970.)

<sup>84</sup> The forthcoming, multivolume Oliver Wendell Holmes Devise History of the Supreme Court of the United States, under the general editorship of Professor Paul Freund, may greatly advance this task. Given the lawyerly orientation of most of the authors, however, there is little reason for encouragement that what is here advocated will either be attempted or achieved. Indeed, the first two volumes to appear, while comprehensive, have received mixed reactions. See Julius Goebel, Jr., *Antecedents and Beginnings to 1801*, The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Vol. I (New York: Macmillan, 1971), reviewed by Morton Horowitz, *Harvard Law Review*, 85 (March, 1972), 1076-1082; Charles Fairman, *Reconstruction and Reunion 1864-88: Part One*, The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Vol. VI (New York: Macmillan, 1971), reviewed by Morton Keller, *Harvard Law Review*, 85 (March, 1972), 1082-1088.

<sup>85</sup> For example, *Hepburn v. Griswold*, 8 Wallace 603 (1870); *Hammer v. Dagenhart*, 247 U.S. 251 (1918); *Bailey v. Drexel Furniture Co.*, 259 U.S. 20 (1922). *Hammer* and *Bailey*, however, might be seen as coming within a critical period, depending upon how one views the Wilson era. See Sellers, "The Equilibrium Cycle."



mating agency.<sup>86</sup> The traditional concept of the Court as the champion of minority rights against majority demands is *largely* incorrect.<sup>87</sup> In order to bring theoretical concepts of the Court's function into line with empirical reality, emphasis should be placed upon the Court's "yea-saying" power rather than upon its "nay-saying" power. Put more cryptically, *McCulloch v. Maryland*<sup>88</sup> is the most important case in American constitutional law—not *Marbury v. Madison*.<sup>89</sup> Historically, of course, the "nay-saying" power, the power of judicial review, has attracted more scholarly attention because it is so unique. But in both quantitative and qualitative terms the "nay-saying" power is the less important aspect of Supreme Court policy making.

The "yea-saying" power, on the other hand, involves two aspects. The Court can either expand or it can confine or mitigate the operation of statutes. This means that students of judicial policy making should be paying more attention to the Court's statutory, as distinct from its constitutional, interpretation. In the areas of selective service,<sup>90</sup> welfare,<sup>91</sup> and civil rights,<sup>92</sup> to cite but a few recent examples, the Court has been making public policy of tremendous consequence, though without raising any issues of constitutional dimensions. This policy making, however, has tended to escape most political scientists, if not

attorneys.<sup>93</sup> Confining the attention to judicial findings of unconstitutionality ignores, as did Dahl, the decisions most pertinent in practice to the questions of whether and how much the Court has succeeded in imposing its own will in the place of Congress's. A systematic study of statutory interpretation might very well yield richer results.

This, in turn, suggests that students of the judicial process should stop equating "activism" with "judicial creativity." Several years ago, Mr. Justice Felix Frankfurter delivered an address in which he claimed that John Marshall had actually been a practitioner of judicial restraint.<sup>94</sup> The reaction of the legal profession was one of general hilarity. Everyone *knew* Marshall had been an activist. But, according to a more complex way of looking at the judicial function, was not Frankfurter correct? After all, in thirty-five years on the bench, Marshall declared only one portion of one fairly limited congressional statute to be unconstitutional. But he was a *very creative* judge.

Finally, however, it must be emphasized that this analysis suggests that the traditional concept of the Court as the protector of minorities is not entirely incorrect. The debate between the judicial activists and the advocates of judicial restraint is particularly relevant during transitional phases of partisan realignment. In fact, by focusing the debate on these periods of political instability, one actually heightens the saliency of that debate, making it more crucial for both the Court and the country. But most of the time that debate will be irrelevant to the actual configurations of political power within the American constitutional system. As Professor Philip B. Kurland has argued,

There is at least one lesson to be derived from the history of the [Court] . . . and that is that the equation cannot be drawn—as it so frequently is drawn—between "activist" and "liberal" or between "judicial restraint" and "conservative." An "activist" Court is essentially one that is out of step with the legislative or executive branches of the government. It will thus be "liberal" or "conservative" depending upon which role its prime antagonist has adopted. . . .

There is obviously little merit in rehearsing the arguments about whether it was intended to grant the Supreme Court the power of judicial review over national legislation. Even the question of the scope of that authority, which is still much mooted, puts undue emphasis on what has not been a central question.

<sup>86</sup> But see Martin Shapiro, *Law and Politics in the Supreme Court* (New York: Free Press, 1964), pp. 75–173, 253–327; and *The Supreme Court and Administrative Agencies* (New York: Free Press, 1967); Richard Funston, ed., *Judicial Crises: The Supreme Court in a Changing America* (Cambridge, Mass.: Schenkman, 1974), p. v.

<sup>94</sup> Felix Frankfurter, "John Marshall and the Judicial Function," *Harvard Law Review*, 69 (December, 1955), 217–238.

<sup>86</sup> Dahl, "Decision-Making"; Charles Black, *The People and the Court: Judicial Review in a Democracy* (New York: Macmillan, 1960). An excellent critique of Black's work is to be found in Kurland, *Politics, the Constitution, and the Warren Court*, pp. 34–39. In spite of his criticisms, however, Professor Kurland also endorses by implication the legitimization thesis. See Kurland, p. 56. But see Adamany, "Legitimacy, Realigning Elections, and the Supreme Court."

<sup>87</sup> It must be emphasized that many questions in American politics and certainly many which reach the Supreme Court do not resolve themselves into clear-cut issues of a majority versus a minority. Rather, in a system of "minorities rule," many political issues involve conflict between two, competing minorities. In such situations, however the Court strikes the balance, it will by definition advance the rights of a minority. See generally Robert Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956).

<sup>88</sup> 4 Wheaton 316 (1819).

<sup>89</sup> 1 Cranch 137 (1803).

<sup>90</sup> For example, *United States v. Seeger*, 380 U.S. 1 (1965); *Oestereich v. Selective Service*, 393 U.S. 233 (1968); *Gutknecht v. United States*, 396 U.S. 295 (1970); *Breen v. Selective Service*, 396 U.S. 460 (1970); *Welsh v. United States*, 398 U.S. 333 (1970).

<sup>91</sup> *Townsend v. Swank*, 404 U.S. 282 (1972); *Jefferson v. Hackney*, 406 U.S. 535 (1972); *Carleson v. Remillard*, 406 U.S. 598 (1972).

<sup>92</sup> For example, *Daniel v. Paul*, 395 U.S. 298 (1969); *Sullivan v. Little Hunting Park, Inc.*, 396 U.S. 229 (1969); *Moose Lodge v. Irvis*, 407 U.S. 163 (1972).

For the fact of the matter is—whatever the romance may be—that so far as the relations between Court and Congress are concerned, the invalidation of national legislation has proved historically to be neither so important nor so exacerbating of the differences between the two branches as law professors would make it.<sup>95</sup>

If, then, the Court is normally in line with popular or, at least, law-making majorities, what function could it possibly serve? Traditional theory had a role for the Court to play, the protector of minority rights against the potential tyranny of the majority of the moment. If the Court does not normally play that role, does it serve any function at all which is distinctive to it? Because, if it does not, it is an absurd example of governmental "featherbedding" and should be abolished. In fact, it may be argued that the Court does play a role that differs from the legislative and executive functions and is peculiarly suited to its capabilities. That argument rests on the distinction between expediency and principle and begins with the premise that all governmental actions have two kinds of effects. The first is their immediate, intended effects. For example, faced with a major national economic crisis, we pass legislation which infringes upon agreements entered into between private individuals, i.e., contracts. The second kind of effect of any public program or policy is its bearing upon principles or values which we believe to be fundamental and of general applicability; or, to take our example again, we believe in the right of the majority to determine the course of government and in the right of the nation to protect its existence in times of crisis, either military or economic.<sup>96</sup> We are all able to perceive the relation between our immediate needs or desires and a particular governmental action, and it is these perceptions which we transmit, by virtue of the electoral process, to our representatives. But we are not always able to perceive, because we do not always care about, the relation of a given policy, program, or action to our long-term values.

Assuming that we wish to have principled government, some agency must, in the normal operation of the policy process, be primarily concerned with society's fundamental, underlying values; and practical, if not philosophic, reasons dictate that this agency be one or another of the branches of the federal government. Which one? I submit not only that it should be but also that, in the American constitutional system, it can only be

the Supreme Court.<sup>97</sup> The Court, by virtue of its institutional position, is able to deal with matters of principle, whereas Congress and the president, because they are responsible to the electoral whims of the moment, cannot. The Justices "have, or should have, the leisure, the training, and the insulation to follow the ways of the scholar in pursuing the ends of government."<sup>98</sup> This situation and the marvelously functional mystique of the judicial process—those seemingly esoteric institutional customs—allow the Court to appeal to our principled good sense, which may be forgotten in the heat of the moment. It is true, as Dahl points out, that the Court cannot withstand us when we are determined, but through the exercise of its power of judicial review it may entreat us to take a sober second thought about the course which we have set for ourselves.

Perhaps the argument can be cast in comparative, even metaphorical, terms. In communist systems, it is the work of the theoretician to re-interpret Marx so as to bring his philosophy and the immediate acts of the government into accord. With respect to the Constitution, the Supreme Court is the theoretician of American democracy. Or, to cast another metaphor, Louis Heren, formerly Washington correspondent of the *London Times*, has compared the Court's functional relationship to the policy-making agencies of American government with that of the Church to the medieval monarch.<sup>99</sup> It is the Court's function to legitimate, if possible, contemporary public acts by demonstrating their consonance with fundamental constitutional principles. It was Mr. Justice Oliver Wendell Holmes who perhaps best expressed the significance of this function:

The past gives us our vocabulary and fixes the limits of our imagination; we cannot get away from it. There is, too, a peculiar logical pleasure in making manifest the continuity between what we are doing and what has been done before.<sup>100</sup>

The Court, then, may be conceived of as an educational institution. The Justices of the Supreme Court, as Professor Eugene Rostow has put it, "are inevitably teachers in a vital national seminar."<sup>101</sup> They are in a better position to conduct

<sup>97</sup> See also Bickel, *The Least Dangerous Branch*, pp. 23–28; Wechsler, "Neutral Principles."

<sup>98</sup> Bickel, *The Least Dangerous Branch*, pp. 25–26.

<sup>99</sup> Louis Heren, *The New American Commonwealth* (New York: Harper & Row, 1968), p. 18.

<sup>100</sup> Oliver Wendell Holmes, *Collected Legal Papers* (New York: Harcourt, Brace and Howe, 1920), p. 139.

<sup>101</sup> Eugene Rostow, *The Sovereign Prerogative: The Supreme Court and the Quest for Law* (New Haven, Conn.: Yale University Press, 1962), pp. 167–168. Compare Rostow's analogy with Lowi, *The End of Liberalism*, p. 314:

In the United States the history of political

<sup>95</sup> Kurland, *Politics, the Constitution, and the Warren Court*, pp. 17–18, 22. See also Schubert, *Judicial Policy-Making*, pp. 145–157.

<sup>96</sup> See *Home Building & Loan Assoc. v. Blaisdell*, 290 U.S. 398 (1934).

such a seminar than any other officials in American government.

Finally, in this respect, it may be noted that this concept of the Court as an institution which brings our short-run, expedient means into accord with our long-run ends or principles is also grounds for justifying the Court's power of judicial review. For what would be the good of a declaration of legitimacy from an institution which had no choice but to validate everything that was brought before it? It is only because the Court has the power to annul that its validation is worthwhile.<sup>102</sup> This argument, derived from Professor Black's legitimation thesis, has seldom received better empirical support than in the inordinate haste with which the Voting Right Acts Amendments of 1970, enfranchising eighteen-year-olds, were brought before the Court for constitutional validation.<sup>103</sup>

Holmes's assessment of Marshall, then, may be extended to the Court *qua* institution: Its significance is essentially symbolic.<sup>104</sup> Its most striking power, judicial review, is largely, if not solely, symbolic and is merely the logical corollary of its power to legitimate.<sup>105</sup> As Lord Bryce would have

it, the Court derives its authority by being "the living voice of the Constitution, the unfold of the mind of the people . . ."<sup>106</sup> This view only serves to re-emphasize the relation between the conditions of freedom and a people's "civic culture." As Mr. Justice Jackson so cogently put the matter,

It is not idle speculation to inquire which comes first, either in time or importance, an independent and enlightened judiciary or a free and tolerant society. Must we first maintain a system of free political government to assure a free judiciary, or can we rely on an aggressive, activist judiciary to guarantee free government? While each undoubtedly is a support for the other, and the two are frequently found together, it is my belief that the attitude of a society and of its organized political forces, rather than its legal machinery, is the controlling force in the character of free institutions.<sup>107</sup>

Some may discount the importance of this observation. They would do well, however, to recall another of Holmes's dicta:

At this time we need education in the obvious more than the investigation of the obscure.<sup>108</sup>

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theory since the founding of the Republic has resided in the Supreme Court. The future of political theory lies there too.

Thus, the legitimation thesis does not necessarily depend, as Professor Adamany claims, upon popular acceptance of the myth of a self-interpreting Constitution. See Adamany, "Legitimacy, Realignment Elections, and the Supreme Court," p. 797. It is for the Court to teach—to convince—that the policies of the dominant coalition are within the broad mainstream of American constitutional values.

<sup>102</sup> See Black, *The People and the Court*, p. 53.

<sup>103</sup> *Oregon v. Mitchell*, 400 U.S. 112 (1970).

<sup>104</sup> Holmes, *Legal Papers*, p. 270.

<sup>105</sup> See Thurman Arnold, *The Symbols of Government* (New Haven, Conn.: Yale University Press, 1935); Edward S. Corwin, "The Constitution As Instrument And As Symbol," *American Political Sci-*

*ence Review*, 30 (December, 1936), 1072-1085; Max Lerner, "Constitution and Court as Symbols," *Yale Law Journal*, 46 (June, 1937), 1291-1319. One of the more interesting aspects of these studies of the symbolic functions of the Court and the Constitution is their dates of publication, no doubt owing largely to the Court's travails with the New Deal. Since the 1930s the study of the symbolic uses of constitutional adjudication appears to have fallen into desuetude. For example, Murray Edelman's admirable analysis of *Politics as Symbolic Action* (Chicago: Markham, 1971) contains not even a passing reference to the judicial process.

<sup>106</sup> James Bryce, *The American Commonwealth*, 2nd ed., rev. (New York: Macmillan, 1891), I, 348.

<sup>107</sup> Robert Jackson, *The Supreme Court in the American System of Government* (New York: Harper & Row, 1955), p. 81.

<sup>108</sup> Holmes, *Legal Papers*, pp. 292-293.

# Determinants of the Outcomes of Midterm Congressional Elections\*

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The outcomes of midterm congressional elections appear as a mixture of the routine and the inexplicable. In every off-year congressional election but one since the Civil War, the political party of the incumbent President has lost seats in the House of Representatives. Yet the factors explaining the variation around the usual aggregate outcome of midterms are not well understood; indeed, in *Politics, Parties and Pressure Groups*, V. O. Key suggested that the nature of the midterm verdict lacked explanation in any theory of a rational electorate:

Since the electorate cannot change administrators at midterm elections, it can only express its approval or disapproval by returning or withdrawing legislative majorities. At least such would be the rational hypothesis about what the electorate might do. In fact, no such logical explanation can completely describe what it does at midterm elections. The Founding Fathers, by the provision for midterm elections, built into the constitutional system a procedure whose strange consequences lack explanation in any theory that personifies the electorate as a rational god of vengeance and of reward.<sup>1</sup>

Furthermore, the central facts of midterm elections—the almost invariable loss by the President's party combined with the great stability in partisan swings compared to on-year elections<sup>2</sup>—

both suggest an electorate returning to their normal partisan alignment after the more hectic presidential contest two years earlier, rather than an electorate responding to short-term national forces and acting as a "rational god of vengeance and of reward." In seeking to explain the sources of midterm loss, both Campbell and Key emphasized the differences in turnout in off-year compared to on-year elections—rather than short-run factors such as the electorate's evaluation of the performance of the President and his party.<sup>3</sup> Following up this approach, Hinckley assessed the administration's midterm loss with reference to the prior presidential election and concluded: "... the midterm 'referendum' appears quite derivative. It is, in part, a continuation of the verdict expressed in the preceding presidential elections and, in part, an adjustment of that verdict, an adjustment built into the midterm by the preceding presidential election."<sup>4</sup> Another recent analysis of midterms from 1954 to 1970 concludes that they are "non-events" and "non-elections," predictable solely from the preceding presidential election.<sup>5</sup> Finally, the Stokes-Miller study of the

<sup>3</sup> Angus Campbell, "Voters and Elections: Past and Present," *Journal of Politics*, 26 (November, 1964), 745-757; Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (Fall, 1960), 397-418; and Key, pp. 568-569.

<sup>4</sup> Barbara Hinckley, "Interpreting House Midterm Elections: Toward a Measurement of the In-Party's 'Expected' Loss of Seats," *American Political Science Review*, 61 (September, 1967), 700.

<sup>5</sup> Mark N. Franklin, "A 'Non-election' in America? Predicting the Results of the 1970 Mid-term Election for the U.S. House of Representatives," *British Journal of Political Science*, 1 (October, 1971), 508-513. See also Anthony King, "Why All Governments Lose By-Elections," *New Society*, March 21, 1968, pp. 413-415; Nigel Lawson, "A New Theory of By-Elections," *Spectator*, November, 1968, pp. 651-652; and John D. Lees, "Campaigns and Parties—The 1970 American Mid-Term Elections and Beyond," *Parliamentary Affairs*, 24 (Autumn, 1971), 312-320. The view that midterms represent, in large measure, the electoral swing of the pendulum (or electoral surge and decline) does not seem to be held by politicians. Sam Kernell has compiled convincing evidence that a central premise among American politicians is that "the president's popularity directly affects his congressional party candidates' chances for election" in midterms: Sam Kernell, "Presidential Popularity and Negative Voting: An Alternative Explanation of the Mid-Term Electoral Decline of the President's Party," paper delivered at the 1974 annual meeting of the American Political Science Association. See also the variety of interpretations of the outcome

\* I wish to thank Marge Cruise, Jan Juran, Alce Anne Navin, Susan Spock, Michael Stoto, and Richard Sun for their help in the collection and analysis of the data. John L. McCarthy, Richard A. Brody, Gerald H. Kramer, Duane Lockard, David Seidman, and Jack Walker provided advice and encouragement. Financial support came from the Woodrow Wilson School of Public and International Affairs at Princeton University and from a fellowship at the Center for Advanced Study in the Behavioral Sciences. Early drafts of the paper were presented in seminars at the Center (October, 1973); the Eay Area Political Behavior Seminar (January, 1974); and Princeton University (October, 1974). A partial, preliminary version of the model is reported in Edward R. Tufte, *Data Analysis for Politics and Policy* (Englewood Cliffs, New Jersey: Prentice-Hall, 1974), pp. 140-145. I wish also to thank several anonymous reviewers and Dr. Ellen Y. Siegelman of the *Review* for their helpful comments. These individuals and institutions do not, of course, bear responsibility for the faults of the study.

<sup>1</sup> V. O. Key, Jr., *Politics, Parties, and Pressure Groups*, 5th ed. (New York: Thomas Y. Crowell, 1964), pp. 567-568.

<sup>2</sup> Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly*, 26 (Winter, 1962), 531-546.

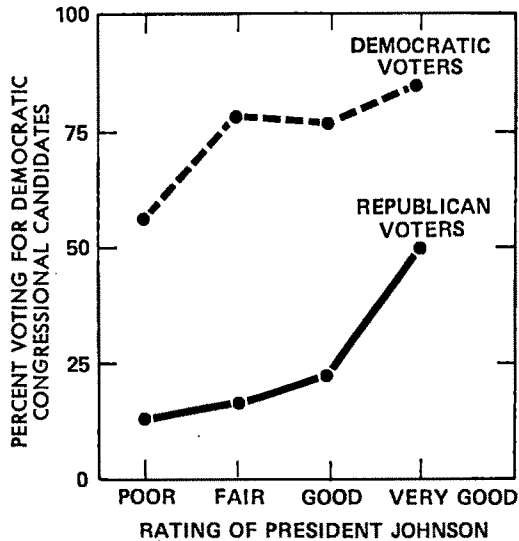
1958 midterm found that voters were simply not responding to the parties' legislative records in casting midterm ballots.

Nevertheless the prevailing view of midterm outcomes as an adjustment restoring the normal partisan equilibrium unrelated to objective events in the two years prior to the midterm is incomplete, for while it explains why the President's party should almost always be operating in the loss column, it does not account for the *number* of votes and seats lost by the President's party. In statistical parlance, the adjustment model of midterm congressional elections explains the location of the mean rather than variability about the mean. But, as Key indicated, "The significance of a specific midterm result comes not from the simple fact of losses by the President's party. Some loss is to be expected. It is the magnitude of the loss that is important."<sup>6</sup>

In this study, we seek to explain the *magnitude of the national midterm loss* by the President's party: why do some presidents lose fewer congressional seats at midterm than other presidents? Do the outcomes of midterm congressional elections represent the electorate's evaluation of the President's performance? Do such outcomes reflect the electorate's evaluation of the administration's management of the economy? If a relatively large proportion of the electorate approves the President's handling of his job or his management of the economy, then does his party lose less in the midterm congressional elections? Or, on the other hand, is the midterm "referendum" only "derivative" and the outcomes lacking in rational explanation? Since those citizens showing up at the polls in the midterm are probably somewhat more politically sophisticated and interested than those voting in on-year elections,<sup>7</sup> the assertion that midterm outcomes are "irrational" provides a substantial challenge to the view that the electorate behaves in rational ways, or at least in ways somewhat responsive to the political environment.

Because there are no other targets available at the midterm, it is not unreasonable to expect that some voters opposed to the President might take

Figure 1. Relationship Between Evaluations of the President and Vote For Congressional Candidate, 1968.



Source: Arseneau and Wolfinger, "Voting Behavior in Congressional Elections," p. 16.

out their dissatisfaction with the incumbent administration on the congressional candidates of the President's party. Arseneau and Wolfinger, using survey data, provide some evidence that "... the public image of Congress is rather undifferentiated and, moreover, assessments of the two parties' performance are likely to be determined predominantly by evaluations of the president rather than Congress. ... congressional candidates are likely to suffer or benefit from voters' estimates of how well the president has been doing his job."<sup>8</sup> Figure 1 shows their analysis of the Survey Research Center data for the 1968 election; for voters of both parties, support for Democratic congressional candidates increases by about thirty percentage points as the evaluations of President Johnson go from "poor" to "very good."

Thus our first link in the model explaining midterm outcomes is the relationship between the aggregate outcome and the electorate's evaluation of the President at the time of the election: at

of British by-elections in Chris Cook and John Ramsden, eds., *By-Elections in British Politics* (New York: St. Martin's Press, 1973). Alternative midterm models are discussed in Richard W. Boyd and James T. Murphy, "How Many Seats Will the Republicans Lose? Changes in the House: A Prediction," *New Republic*, October 24, 1970, pp. 12-14.

<sup>6</sup> Key, p. 569.

<sup>7</sup> This conventional description of the midterm electorate has been challenged by the evidence of Robert B. Arseneau and Raymond E. Wolfinger, "Voting Behavior in Congressional Elections," paper delivered at the 1973 annual meeting of the American Political Science Association.

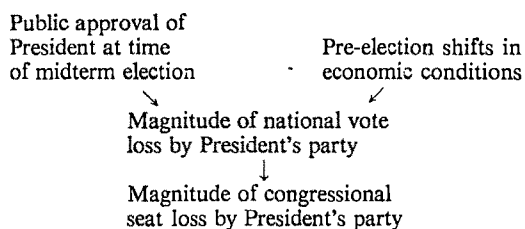
<sup>8</sup> Arseneau and Wolfinger, p. 3. Several other recent studies have greatly reinforced the evidence linking evaluations of the incumbent president to electoral choices in congressional races. See Kernell, "Presidential Popularity and Negative Voting," for an extensive analysis of Gallup poll data in six midterm elections; and James E. Pierson, "Presidential Popularity and Midterm Voting at Different Electoral Levels," *American Journal of Political Science*, forthcoming, November, 1975, which uses data from the 1970 election study conducted by the Center for Political Studies at the University of Michigan.

what rate fluctuations in presidential popularity are translated into fluctuations in votes and congressional seats in off-year elections? Two kinds of evidence help assess the relationship: aggregate data for the whole nation and individual interviews in surveys of the electorate. Although this study is largely based on aggregate data, fortunately some new detailed material at the individual level also bears on the issue.

The second explanatory variable in our analysis of midterm outcomes is the performance of the economy in the year prior to the midterm. There is already available a careful study linking prevailing economic conditions to aggregate electoral outcomes, including midterms. Kramer's model explains 56 per cent of the variation in the national partisan division of vote in the midterms from 1898 to 1962.<sup>9</sup> Although these results have been subjected to vigorous, but not convincing, critiques by Stigler and others, it seems clear from Kramer's analysis that midterm outcomes are responsive to changes in objective economic conditions taking place between the presidential election and the midterm itself.<sup>10</sup>

In summary, our data analysis here will estimate the impact on midterm congressional elections of the electorate's evaluations of presidential performance and of prevailing economic conditions prior to the election. Such estimates lead to predictions of the national partisan division of the congressional vote. That vote, of course, is not the ultimate measure of the midterm outcome—it is the resulting partisan distribution of *seats* in the House of Representatives that matters politically. As we will see, the translation of votes

into seats has changed considerably over the period covered in our study: comparable shifts in the midterm partisan division of the vote are now worth less than half as much in terms of congressional seats compared to 35 years ago<sup>11</sup>—thus significantly muting the impact of midterm elections on party alignment in the House. It is therefore not enough to explain and predict the partisan division of the vote; it is necessary also to take into account the changing political consequences of that vote resulting from the changing character of the translation of votes into congressional seats. Thus the model is:



#### Measuring the Variables in the Model

A substantial amount of recent research has contemplated the three variables in the first stage of the model: the measurement of economic performance in relation to electoral outcomes, the meaning of the long-run Gallup poll question on approval of the President, and the proper way to interpret midterm congressional outcomes with respect to the vote and the seat loss by the President's party. It is clear from Stigler's study that the results in the aggregate analysis of congressional elections are sensitive to the particular specification of variables in the model. The problem is further complicated by the difficulty of choosing among alternative specifications, given the relatively small number of data points, the high intercorrelation between alternative measures of the same general concept, and difficulty in handling idiosyncratic problems such as third party candidacies, elections that involve unusual factors, and the like. In the face of these problems, some special attention to the particular operationalization of each variable is necessary, even though each seemingly has rather obvious empirical referents.

The most important variable to measure well is the dependent or response variable, the magnitude of the midterm loss by the President's party. As most discussions evaluating midterm losses point out, the idea of "loss" implies the question "Relative to what?"<sup>12</sup> The relevant comparison, it

<sup>9</sup> Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964," *American Political Science Review*, 65 (March, 1971), 131-143. A data error in this paper is corrected in its Bobbs-Merrill reprint (PS-498); see also Saul Goodman and Gerald H. Kramer, "Commentary on Arcelus and Meltzer, 'The Effect of Aggregate Economic Conditions on Congressional Elections,'" *American Political Science Review*, forthcoming; Gerald H. Kramer and Susan J. Lepper, "Congressional Elections," in *The Dimensions of Quantitative Research in History*, ed. William O. Aydelotte, Allan G. Bogue, and Robert William Fogel (Princeton: Princeton University Press, 1972), 256-284; and Susan J. Lepper, "Voting Behavior and Aggregate Policy Targets," *Public Choice*, 18 (Summer, 1974), 67-81.

<sup>10</sup> George J. Stigler, "General Economic Conditions and National Elections," *American Economic Review*, 63 (May, 1973), 160-167; and further discussion by Paul W. McCracken, Arthur M. Okun, and others, pp. 169-180. Stigler's method might best be described as a "most squares" technique: find the specification that maximizes the error variance. But the outstanding work in discovering the pessimum most squares model is Francisco Arcelus and Allen H. Meltzer, "The Effect of Aggregate Economic Conditions on Congressional Elections," *American Political Science Review*, forthcoming; see the reply of Goodman and Kramer.

<sup>11</sup> On the translation of votes into seats, see Edward R. Tuft, "The Relationship Between Seats and Votes in Two-Party Systems," *American Political Science Review*, 67 (June, 1973), 540-554.

<sup>12</sup> Hinckley, "Interpreting House Midterm Elections;" Harvey Zeidenstein, "Measuring Congressional Seat Losses in Mid-Term Elections," *Journal of Poli-*

seems, is between the normal, long-run congressional vote for the political party of the current President and the outcome of the midterm election at hand—that is, a standardized vote loss that takes the long-run partisan trend into account:

$$\begin{array}{lcl} \text{standardized vote loss by} & \text{national congressional vote} & \text{average national congressional} \\ \text{President's party in the } i^{\text{th}} & \text{for President's party in the } i^{\text{th}} & \text{vote for party of current Presi-} \\ \text{midterm election} & \text{election} & \text{dent in previous elections} \end{array}$$

Thus the loss is measured with respect to how well the party of the current President has normally done, where the normal vote is computed by averaging that party's national vote over the eight preceding both on-year and off-year congressional elections.<sup>13</sup> This standardization is necessary because the Democrats have dominated postwar congressional elections; if the unstandardized vote won by the President's party is used as the response (dependent) variable, the Republican presidents would appear to do poorly. For example, when the Republicans win 48 per cent of the national congressional vote, it is, relatively, a substantial victory for that party and should be counted as such. The eight-election standardization takes this effect into account as well as yielding a model with a bit of dynamics to it.<sup>14</sup> It is

tics, 34 (February, 1972), 272–276; and A. H. Taylor, "The Proportional Decline Hypothesis in English Elections," *Journal of the Royal Statistical Society, Series A*, 135 (1972), 365–369.

<sup>13</sup> See the normalizations in William H. Flanigan and Nancy H. Zingale, "The Measurement of Electoral Change," *Political Methodology*, 1 (Summer, 1974), 49–82; also William H. Flanigan and Nancy H. Zingale, "Electoral Competition and Partisan Realignment," paper delivered at the 1973 annual meeting of the American Political Science Association. The standard discussion is, of course, Philip E. Converse, "The Concept of a Normal Vote," in Angus Campbell et al., *Elections and the Political Order* (New York: Wiley, 1966), pp. 9–39.

<sup>14</sup> The Democratic loss in the 1938 midterm election is measured relative to the share of votes received by Democratic congressional candidates in the previous three elections (1932, 1934, and 1936), rather than the eight used for estimating the normal vote in the other midterms. The eight election normalization fails for 1938 because of the rapid and extensive realignment from 1930 to 1932; thus the averaged results of congressional elections from 1932 on gives a more reasonable estimate of the 1938 normal vote than the inclusion of several Republican dominated years prior to the realignment. The percentage share of the congressional vote received by the Democrats during that period shows the problem:

1938	50.8%
1936	56.2%
1934	56.2%
1932	56.9%
<hr/>	
1930	45.9%
1928	42.8%
1926	41.6%
1924	42.1%
1922	46.4%

straightforward, furthermore, to reconstruct the actual or predicted outcome from the standardized vote, thereby permitting pre-election predictions of the partisan division of the vote. There are many alternative ways of quantitatively as-

sessing the midterm loss.<sup>15</sup> The elections used in the standardization could be weighted, with the heaviest weight given to the most recent elections. On-year congressional elections might be discarded altogether. A larger or smaller number of elections might be used. In general, most of the obvious alternatives are highly correlated; in addition, experiments with a variety of methods for computing the normal vote revealed that the model performed well under most reasonable alternatives. Table 1 shows the computations for the midterm elections from 1938 to 1970.

Let us now consider the explanatory variables, the public's approval of the President and the economic conditions prevailing at the time of midterm election.

The only long-run consistent measure of the public's evaluation of the President's general performance is the standard Gallup poll question asked in their monthly surveys: "Do you approve or disapprove of the way [the incumbent] is handling his job as President?"<sup>16</sup> While the Gallup

From this series, it seems clear that a reasonable estimate of the normal Democratic vote for the 1938 election should be based on the elections of 1932, 1934, and 1936, rather than earlier years.

<sup>15</sup> See Harvey M. Kabaker, "Estimating the Normal Vote in Congressional Elections," *Midwest Journal of Political Science*, 13 (February, 1969), 58–83.

<sup>16</sup> The presidential approval ratings from 1946 to 1970 are from *The Gallup Opinion Index*, 64 (October, 1970), 16; the 1938 approval rate, 57 per cent, was averaged (because of inconsistencies in question wording and survey dates) from two surveys: September, 1938—"Are you for or against Roosevelt today?" 55.2 per cent; and October, 1938—"In general do you approve or disapprove of Roosevelt as President?" 59.6 percent. The source for the 1938 data is George Gallup, *The Gallup Poll* (New York: Random House, 1972), pp. 118, 122. Similar, but not identical figures are reported in a fine study by Wesley C. Clark, "Economic Aspects of a President's Popularity" (Ph.D. dissertation, University of Pennsylvania, 1943), p. 47, which also contains an extensive discussion of the early years of the series. A flawed analysis of the factors affecting the ratings is given in John E. Mueller, "Presidential Popularity from Truman to Johnson," *American Political Science Review*, 64 (March, 1970), 18–34; and in John E. Mueller, *War, Presidents and Public Opinion* (New York: Wiley, 1973). The substantive and statistical difficulties in Mueller's analysis are discussed in Richard A. Brody and Benjamin I. Page, "The Impact of Events on Presidential Popularity: The Johnson and Nixon Administrations," paper delivered at the 1972 annual

Table 1. Data for Midterm Elections

Year	$V_i$ Nationwide Midterm Congressional Vote for Party of Incumbent President	$N_i^8$ Mean Congressional Vote for Party of In- cumbent President in 8 Prior Elections	$Y_i = V_i - N_i^8$ Standardized Vote Loss (-) or Gain (+) by President's Party in Midterm Election	$P_i$ Gallup Poll Rating of Presi- dent at Time of Election	$\Delta E_i$ Yearly Change in Real Dispos- able Income Per Capita
1938	50.82%	Democratic 57.18%*	-6.36%	57%	-\$82
1946	45.27%	Democratic 52.57%	-7.30%	32%	-\$36
1950	50.04%	Democratic 52.04%	-2.00%	43%	\$99
1954	47.46%	Republican 49.79%	-2.33%	65%	-\$12
1958	43.90%	Republican 49.83%	-5.93%	56%	-\$13
1962	52.42%	Democratic 51.63%	+0.79%	67%	\$60
1966	51.33%	Democratic 53.06%	-1.73%	48%	\$96
1970	45.68%	Republican 46.66%	-0.98%	56%	\$69

\* For 1938, mean is based on last three elections only. See note 14.

poll has asked for evaluations of the President since 1935 (thereby limiting our study to the midterms since 1938), the wording of the question has shifted and it was only in 1945 that the standard wording was adopted. The wording used immediately prior to the 1938 midterm, however, differs only slightly from the postwar surveys and consequently our analysis includes the 1938 survey results.<sup>17</sup>

Table 1 shows the approval ratings from the surveys taken prior to each midterm election. The simple correlation between the normalized midterm loss by the party of the President and the pre-election approval rating is .50, indicating that larger losses are associated with lower popularity.

Although we have only a single relatively consistent indicator over the years of the public's evaluation of the President, there are available, on the other hand, many different possible measures for the other dependent variable, the performance of the economy. Neither theory nor data strongly suggest a good choice. The discussions of the studies of Kramer and Stigler by McCracken, Okun, Riker and Ireland reveal many speculations and little theory—beyond the observation, agreed upon by all, that general economic conditions might somehow have an effect on some elections—that suggest specific hypotheses about which economic variables should be important or what kinds of time perspectives voters might use in evaluating the pre-election

performance of the economy.<sup>18</sup> Kramer's empirical work has shown the political importance of inter-election shifts in real income and of inflation; both seem to have more impact on congressional elections than do shifts of ordinary magnitude in unemployment.<sup>19</sup> The best measure of economic conditions for our model therefore appears to be pre-election changes in real disposable income per capita.<sup>20</sup> This measure may reflect the economic

<sup>18</sup> Stigler's article itself, as well as the commentary, makes clear the lack of theoretical specificity found in current models correlating economic time series to electoral outcomes; see Stigler, "General Economic Conditions and National Elections," pp. 160-167; and further comments, 168-180. Stigler uses a two-year difference to compute the change in economic conditions for his model, on the view that voters compare the change in the economy in the current election year with that at the time of the previous election two years before. That model seems doubtful, attributing an excessively long time perspective to voters, especially for moderate economic changes. Another way to look at the matter is to consider the voter's problem as the generation of an estimate of the rate of real economic change immediately prior to the election—in order to estimate what can be expected with respect to the performance of the economy under the incumbent administration. If those expectations are good, the party of the incumbent President receives a vote; otherwise the out-party gets the vote. If this is a reasonable description of the problem facing the voter, then the voter would probably not use Stigler's method—a two-year difference in economic changes—to estimate expected or immediately past economic performance. A one-year difference is at least slightly more realistic. Additional progress on this question can probably be made by examining survey evidence at the individual level rather than by still more specifications of aggregate models.

<sup>19</sup> Kramer, p. 139; see also Goodman and Kramer.

<sup>20</sup> The yearly change in real disposable personal income per capita (in 1958 dollars) was computed from data in *The Annual Report of the Council of Economic Advisers, 1973* (Washington, D.C.: U.S. Government Printing Office, 1973), p. 213; and *The Annual Report of the Council of Economic Advisers, 1971*, p. 215.

meeting of the American Political Science Association; and in Douglas A. Hibbs, Jr., "Problems of Statistical Estimation and Causal Inference in Time-Series Regression Models," in *Sociological Methodology, 1973-1974*, ed. Herbert Costner (San Francisco: Jossey-Bass, 1974), pp. 252-308.

<sup>17</sup> The details of the shifting wording are in Clark, *Economic Aspects of a President's Popularity*, pp. 47, 55-60.



concerns of many voters, for it assesses the short-run shift in average economic conditions, measured in terms of real purchasing power, prevailing at the individual level—a shift in conditions for which some voters might hold the incumbent administration and the political party of the incumbent administration responsible.

In summary, our model explaining the midterm vote received by the political party of the incumbent President is:

$$Y_i = \beta_0 + \beta_1 P_i + \beta_2 (\Delta E_i) + u_i \quad (1)$$

where

$Y_i$  = standardized midterm vote for the political party of the incumbent President in the  $i^{\text{th}}$  midterm congressional election,  
 $Y_i = V_i - N_i^8$ ;

$V_i$  = nationwide share of the two-party congressional vote received by political party of the incumbent President in the  $i^{\text{th}}$  midterm;

$N_i^8$  = normal congressional vote for the political party of the incumbent President at the time of the  $i^{\text{th}}$  midterm; computed as the average, over the preceding eight congressional elections (both on- and off-years) prior to the  $i^{\text{th}}$  midterm, of the na-

tionwide share of the two-party congressional vote received by the political party of the President in office at the time of the  $i^{\text{th}}$  midterm;

$P_i$  = percentage of sample in Gallup poll in September prior to the  $i^{\text{th}}$  midterm who approve of the job the incumbent President is doing;

$\Delta E_i$  = yearly change in real disposable personal income per capita between the year of the midterm and the previous year; and  
 $u_i$  = residual or error term.

Note that the model can be rewritten to estimate the nationwide congressional vote for the President's party:

$$V_i = \beta_0 + \beta_1 P_i + \beta_2 (\Delta E_i) + \beta_3 N_i^8 + u_i \quad (2)$$

Thus, while equation (1) leads to estimates of  $Y_i$ , the standardized vote loss by the President's party, equation (2) estimates  $V_i$ , the nationwide proportion of the congressional vote for the party of the President. The two models are identical, however, if  $\beta_3$  is unity—for then  $N_i^8$  can be moved, in equation (2), from the right-hand to the left-

hand side of the equation. The data were used to fit equation (2) and  $\beta_3$  was, in fact, very close to unity; and therefore equation (1) will be the model developed in the remainder of the analysis. Equation (1) estimates one less parameter than equation (2), an advantage since the model must be estimated on the basis of a very small number of cases. The small  $N$  and the potential fragility of the model also suggest that extraordinary tests of the model's explanatory and predictive capacity are necessary. Many such tests will be conducted, including an assessment of the model in predicting the outcome of the 1974 midterm congressional elections.

#### Fitting the Model and Confirming the Results

We now use the data of Table 1 to estimate the  $\beta$ 's, the regression coefficients, in the model.<sup>21</sup> The estimate,  $\beta_1$ , assesses the impact of presidential approval rating on the midterm vote; and  $\beta_2$ , the impact of the pre-election change in real disposable personal income per capita on the midterm vote.

$$\text{Standardized vote loss by President's party in the midterm} = \beta_0 + \beta_1 \left[ \begin{array}{c} \text{Presidential} \\ \text{popularity} \end{array} \right] + \beta_2 \left[ \begin{array}{c} \text{Yearly change} \\ \text{in economic} \\ \text{conditions} \end{array} \right]$$

The idea is, of course, that the lower the approval rating of the incumbent President and the less prosperous the economy, the greater the loss of support for the President's party in the midterm congressional elections.

Table 2 shows the estimates of the model's coefficients. The results are statistically secure since the coefficients are at least four times their standard errors. The fitted equation indicates:

—A change in presidential popularity of 10 percentage points in the Gallup poll is associated with a national change of 1.3 percentage points in national midterm vote for congressional candidates of the President's party.

—A change of \$100 in real disposable personal income per capita in the year prior to the midterm election is associated with a national change of 3.5 percentage points in the midterm vote for congressional candidates of the President's party.

The fitted equation explains statistically 91.2 per cent of the variance in national midterm out-

<sup>21</sup> The midterm of 1942 is omitted from the analysis because of the special effect of wartime controls on the economy and of wartime conditions on evaluations of the incumbent President. Kramer also dropped wartime years; see Kramer, p. 137.

Table 2. Multiple Regression Fitting Standardized Vote Loss by President's Party in Midterm Elections

$$Y = \beta_0 + \beta_1(P) + \beta_2(\Delta E)$$

	Regression Coefficient and (Standard Error)	Simple Correlation with Midterm Loss
Presidential approval rating ( $P$ )	$\hat{\beta}_1 = .133^*$ (.033)	.503
Yearly change in real disposable personal income per capita ( $\Delta E$ )	$\hat{\beta}_2 = .035^*$ (.006)	.795

Constant ( $\hat{\beta}_0$ ) = -11.083.

$R^2 = 0.912$ .

\* Statistically significant at the .01 level.

comes from 1938 to 1970;<sup>22</sup> or, to put it another way, the correlation between the actual election results and those predicted by the model is .955, as shown in Figure 2. Since the fitted equation uses two meaningful explanatory variables, it seems reasonable to believe that in this case a successful statistical explanation is also a successful substantive explanation.

Before turning to the substantive consequences of the fitted equation, it is necessary to test the soundness of the model. Such tests are important because the model is based on a relatively short series of elections (although more than many studies of midterms)—and also because the model is apparently so successful in terms of the variance explained. Are the findings the result of some artifact?

The overall equation and the estimates of the individual regression coefficients are statistically significant at the .01 level. Let us consider four additional tests of the model: the independent replication of the estimated regression coefficients, and tests assessing the stability, postdictive quality, and predictive quality of the regression equation.

Some independent studies are consistent with estimates of the regression coefficients in this model. Kramer finds that, in congressional elections from 1896 to 1964 (including both on- and off-year congressional elections), "a 10% decrease in per capita real personal income would cost the incumbent administration 4 to 5 per cent of the congressional vote, other things being

<sup>22</sup> In regressions of this sort, involving such a small number of degrees of freedom, some prefer to use a corrected  $R^2$  that takes into account the loss in degrees of freedom as the coefficients are estimated. In our case, the corrected  $R^2$  is 0.88. See Carl F. Christ, *Econometric Models and Methods* (New York: Wiley, 1966), pp. 509-510.

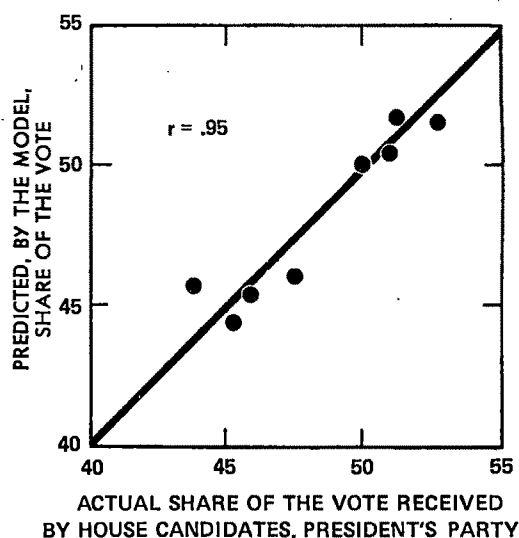


Figure 2. Actual and Predicted Share of the Two-Party Vote Received by Congressional Candidates of President's Party, Midterm Elections, 1938-1970

equal."<sup>23</sup> Since the average real disposable personal income per capita in the period under study here is around \$1800, Kramer's model estimates that a shift of approximately \$180 in real disposable income would produce a shift of 4 to 5 per cent in the congressional vote. Our regression indicates that a shift of \$180 in income would produce a shift of about 6 per cent in the congressional vote. Given the differences between the studies with respect to the period and types of elections covered, the results seem quite comparable. Our short-term (1938-1970) estimate approximately matches Kramer's long-term (1896-1964) estimate of the impact of economic conditions on congressional elections.

An independent confirmation of the estimated effect of the presidential approval level on the midterm outcome comes from a study based on survey interviews from national samples of individual voters. Kernell computed voter defection rates by analyzing responses to the interviews in the Gallup poll's samples prior to the midterms of 1946, 1950, 1954, 1958, and 1962. He finds that "for every nine point change in the percentage approving the president, his party's congressional vote will change 1.4 percentage points."<sup>24</sup> Our estimate, using aggregate data, is virtually identical. The two estimates—one based on individual interviews recording respondent's claimed vote choice in the midterm and the evaluation of the incumbent president, the other based on the aggregate approval rating and the actual vote result—were arrived at completely independently.

<sup>23</sup> Kramer, p. 141.

<sup>24</sup> Kernell, p. 32.

Table 3. After-the-Fact Predictive Error of the Model

Year	Actual Vote for House Candidates, President's Party	Gallup Poll Prediction*	Model Prediction	Gallup Absolute Error	Model Absolute Error
1938	50.8	54	50.8	3.2	0.0
1946	45.3	42	44.5	3.3	0.8
1950	50.0	51	50.1	1.0	0.1
1954	47.5	48.5	46.9	1.0	0.6
1958	43.9	43	45.7	0.9	1.8
1962	52.4	55.5	51.6	3.1	0.8
1966	51.3	52.5	51.7	1.2	0.4
1970	45.7	47	45.4	1.3	0.3

Average absolute error, Gallup = 1.9 percentage points.

Average absolute error, Model = 0.6 percentage points.

\* National survey taken 7 to 10 days prior to the election. The question asked is "If the elections for Congress were being held today, which party would you like to see win in this congressional district, the Democratic or the Republican party?"

In our small data set, based on single readings taken once every four years immediately prior to each midterm, there is no relationship between the presidential approval rating and the pre-election shift in real disposable income. (Note that the approval rating is measured in absolute terms, rather than as a pre-election shift.) For example, the Eisenhower midterms of 1954 and 1958 reflect a popular president and a mediocre short-run economic performance. More generally, it appears that presidential approval ratings are a function of many different factors, including performance in foreign affairs, the President's personality, scandals, and large downward shifts in economic conditions.

To check the stability of the fitted equation, the model was re-estimated after excluding one election at a time from the computations.<sup>25</sup> The regression coefficients remained very stable and statistically significant, and the  $R^2$  did not go below .89 in the re-estimates. It is clear that the estimates for the overall model are not dominated by a single set of outlying values for one election.

As another check of the adequacy of model, its after-the-fact predictions of midterm outcomes were compared with the pre-election predictions made by the Gallup poll in the national survey conducted a week to ten days before each elec-

tion.<sup>26</sup> As Table 3 shows, the model outperforms the pre-election predictions based on surveys directly asking voters how they intend to vote. Now all this is, of course, after the fact and it would be more useful to have a genuine prediction in hand prior to the election to test the model. This leads to our strongest test of the model—for we can examine its *predictive* powers in a series of historical experiments.

Suppose the model had been estimated prior to the 1970 election, using the data from the midterm elections from 1938 to 1966. The fitted equation prior to the 1970 election was:

$$\text{Standardized midterm loss} = -11.06 + .133(P) + .035(\Delta E).$$

Now let us use this model, generated from the experience from 1938 to 1966, to predict the outcome of the 1970 election. The September, 1970, level of presidential approval was 56 per cent; the 1969–1970 shift in real disposable personal income per capita was \$69. Plugging those values into the pre-1970 equation leads to a pre-election forecast of the 1970 outcome:

$$\begin{aligned} \text{Predicted 1970 normalized midterm loss} \\ &= -11.06 + .133(56) + .035(69) \\ &= -1.20 \text{ per cent.} \end{aligned}$$

Since the actual normalized loss was  $-0.98$  per cent, the model performed well in this predictive trial. To translate these results to the actual partisan division of the vote, the pre-1970 model predicted the 1970 outcome to be 45.4 per cent of the vote for the party of the President, the Republicans; in fact, they won 45.7 per cent of the vote in 1970.

Table 4 shows the outcome of this historical

<sup>25</sup> The discarding of observations one at a time coupled with re-estimation is the first step in producing a "jackknife" estimate of a complex statistic along with a confidence interval; see Frederick Mosteller and John W. Tukey, "Data Analysis, Including Statistics," in Gardner Lindzey and Elliot Aronson, eds., *The Handbook of Social Psychology*, 2nd ed. (Reading, Massachusetts: Addison-Wesley, 1968), pp. 133–163; and Rupert G. Miller, Jr., "The Jackknife—A Review," Technical Report No. 50 (August 28, 1973), Department of Statistics, Stanford University, Stanford, California.

<sup>26</sup> Polls reported in Gallup, *The Gallup Poll*.

Table 4. Before-the-Fact Predictions of the Model

Model Based on Years	$\hat{\beta}_1$	$\hat{\beta}_2$	$R^2$	Predicting Election of	Vote for President's Party		Model Absolute Error	Gallup Absolute Error*
					Predicted %	Actual %		
1938-1954	.12	.032	.98	1958	46.0	43.9	2.1	0.9
1938-1958	.11	.032	.85	1962	50.8	52.4	1.6	3.1
1938-1962	.13	.036	.90	1966	51.9	51.3	0.6	1.2
1938-1966	.13	.035	.90	1970	45.4	45.7	0.3	1.3
1938-1970	.13	.035	.91	1974	39.2	41.1	1.9	1.1

\* Average absolute error, Gallup = 1.5 percentage points.  
Average absolute error, Model = 1.3 percentage points.

experiment for three other midterm elections—1958, 1962, and 1968. In each case, the predictions are based on a model estimated *prior* to the election predicted—that is, they are honest predictions. Note how stable the model is over the years, even though it is based on fewer and fewer elections as we go backwards in time. Table 4 shows that the model performs very well indeed in its predictions, doing even somewhat better than pre-election polls directly asking voters what party's candidate they intend to support in the upcoming midterm election. Table 4 thus provides a very strong test of an explanatory model based on non-experimental data—a test of predictive success. The before-the-fact predictive trials of the model show the following: in the four midterm elections from 1958 to 1970, the pre-election forecast generated by the model deviated by an average of 1.1 percentage points from the actual partisan division of the vote. In these tests, based on genuine prediction, the model performs successfully.

The model for the years 1938-1970 has performed well in all our statistical tests:

- high explanatory power,  $R^2 = .91$
- statistical significance (.01 level) of all estimates

- independent replication, using other data or models, of parameter estimates
- no multicollinearity
- no outliers dominating estimates; stability of estimates when parts of data are discarded
- historical experiments: successful postdictions
- historical experiments: successful predictions.

This is all very nice, but how well did the model do in its predictions of the 1974 midterm congressional elections?

**The Model and the 1974 Elections:  
A Difficult Test in a Landslide Election**

In the fall of 1973, I constructed Table 5—showing what the model would predict for the 1974 midterm, given varying levels of presidential approval ratings and performance of the economy. It was possible, then, to track the vote estimates for the huge changes in approval ratings and economic conditions as both President Nixon and the economy collapsed. Using data for approval rating and economic conditions, a pre-election prediction for the 1974 midterm was generated from the model. The calculations and the prediction were made public two weeks before

Table 5. Predicted Republican Congressional Vote in 1974

Predicted national congressional vote for Republicans in 1974 = $35.04 + .133 P + .035 \Delta E$ , where $P$ = per cent approving the job the President is doing $\Delta E$ = yearly change in real disposable personal income per capita					
Percentage Approving the Job President is Doing ( $P$ )	Yearly Change in Real Disposable Personal Income Per Capita ( $\Delta E$ )				
	—\$100	—\$50	\$0	\$50	\$100
25%	34.9	36.6	38.4	40.1	41.9
30%	35.5	37.3	39.0	40.8	42.7
35%	36.2	37.9	39.7	41.5	43.2
40%	36.9	38.6	40.4	42.1	43.9
45%	37.5	39.3	41.0	42.8	44.5
50%	38.2	39.9	41.7	43.4	45.2
55%	38.9	40.6	42.4	44.1	45.9
60%	39.5	41.3	43.0	44.8	46.5

the election; in addition, several other people used the model on their own to generate pre-election predictions. All in all, the election of 1974 provided a particularly stern test—both explanatory variables had undergone very large short-term shifts and had reached historical extremes in the period before the election. And Mr. Ford had only been in office for three months.

The prediction for the 1974 midterm was constructed from the model:

Standardized midterm loss in 1974 (predicted)

$$= \text{predicted Republican vote in 1974} - \text{average Republican congressional vote in prior elections, 1958-1972}$$

$$= -11.083 + .133P + .035(\Delta E).$$

The average Republican vote over the last eight congressional elections (from 1958 to 1972) is 46.12 per cent. Substituting this value into the above equation yields the estimates shown in Table 5. The predicted Republican share of the congressional vote in 1974 is shown for a variety of combinations of popularity levels and economic conditions and the exact prediction is determined by the approval level and economic conditions prevailing immediately before election. In the months before the 1974 elections, President Ford's popularity shifted greatly:<sup>27</sup>

August 16	71%
September 6	66%
September 27	50%
October 22	55%

The condition of the economy also changed greatly prior to the election; real disposable income in March, 1974 had declined about \$45 over the preceding year, but by the time of the November elections the decline was \$90. Putting the pre-election values for the approval rating and economic conditions into the equation led to the following late-October prediction for the 1974 midterm vote:

Predicted Republican congressional vote

$$= 35.04 + .133(55) + .035(-90)$$

$$= 39.2\%$$

The final pre-election Gallup poll predicted 60 per cent Democratic, 40 per cent Republican; a national phone poll by Decision Making Information led to a 62-38 prediction; and our midterm model predicted 60.8 to 39.2.<sup>28</sup> The actual vote was 58.9 to 41.1 in 1974.<sup>29</sup> Thus the error of the midterm model was 1.9 percentage points; of the Gallup poll, 1.1 points; and of the DMI poll, 3.1 points. Although the predictive error of the model

for the 1974 election was slightly above its previous average, the model still performed very well in a most difficult election.

During 1973-74, both explanatory variables in the midterm model moved toward historical extremes, both high and low: presidential approval ratings ranged between 24 and 71 per cent; and  $\Delta E$  between +\$198 and -\$90; and as these variables changed, the projected midterm vote shifted. These shifts can be compared with a series of polls

taken during 1973-74 in which respondents were asked what party's candidate they intended to support in the upcoming congressional elections. Although the different polls themselves are not always consistent with one another, the projections of the midterm model follow rather well the shifts in the vote recorded in the polls (Table 6).<sup>30</sup> The dynamics of the midterm model show how economic conditions and the electorate's changing evaluations of Nixon and Ford shaped the Democratic landslide of 1974: while Ford's replacement of Nixon helped congressional Republicans by nearly six percentage points, most of that gain was offset by a declining economy and by the 15-point loss in Ford's approval rating following the pardon of Nixon.

Table 6 also indicates that the midterm model may over-respond to very extreme values in approval ratings and particularly in economic conditions; the model's projection computed in May 1973 deviates quite substantially from the Gallup poll (although less so from the Harris survey of the same month). Nevertheless the important point is not only that the model did survive the really difficult test of the 1974 midterm, but that it also helped assess the electoral effects of the

<sup>27</sup> "Gallup Says Poll Shows Ford Popularity on Rise," *The New York Times*, October 24, 1974.

<sup>28</sup> "Poll Confirms GOP Fears," *The Washington Post*, November 4, 1974, p. A5.

<sup>29</sup> Vote as reported in *The Gallup Opinion Index*, 118 (April, 1975), p. 27.

<sup>30</sup> Data sources: Gallup approval rating for October, 1974 from "Gallup Says Poll Shows Ford Popularity on Rise," *The New York Times*, October 24, 1974; other months from *The Gallup Opinion Index*, 103 (January, 1974), 3; 108 (June, 1974), 1; and 111 (September, 1974), 12. The change in real disposable income per capita is available only by quarters and the monthly values are interpolated; it should also be noted that the quarterly figures are quite unstable, with provisional and final estimates often differing substantially. The computations are based on data in Bureau of Economic Analysis, *Business Conditions Digest*, January, 1975 (Washington, D.C.: U.S. Government Printing Office, 1975), p. 69. The reports of polls asking people how they intended to vote in the 1974 congressional election are from "Poll Confirms GOP Fears," *The Washington Post*, November 4, 1974, p. A-5; "Poll: Democrats Will Sweep Into the House," *New York Post*, August 8, 1974, p. 24; and *The Gallup Opinion Index*, 110 (August, 1974), 1-4. Nonresponses have been divided equally between the two parties.

Table 6. 1974 Election Projections from the Midterm Model and Polls, 1973-1974

Date	Per Cent Approving President	Change in Real Income ( $\Delta E$ )	Projection of Midterm Model, % Democratic	Polls, % Democratic in Upcoming Election (G = Gallup, H = Harris, DMI = Decision-Making Information)
October, 1974	55%	-\$90	61%	60%, 62% (G, DMI)
July, 1974	24%	-\$91	65%	63% (H)
May, 1974	27%	-\$78	64%	60%, 64% (H, G)
March, 1974	26%	-\$44	62%	62% (H)
January, 1974	27%	+\$22	60%	59%, 65% (H, G)
October, 1973	28%	+\$168	55%	64% (G)
September, 1973	34%	+\$168	54%	58% (H)
May, 1973	44%	+\$198	51%	55%, 60% (H, G)

political and economic earthquakes in the months prior to the election. And, of course, the experience of 1974 is clearly contrary to the textbook view of midterms—that off-year congressional elections are not much more than the electoral swing of the pendulum, mainly the consequence of differences in off-year compared to on-year turnout.

In summary, the tests of the midterm model confirm that even though the fitted equation is based on a relatively short series of elections, the quantitative results are quite secure—as indicated by the independent replications of both regression coefficients, the postdictive and predictive trials, the conventional tests of statistical significance, the pre-election predictions for the 1974 midterm, and the model's ability to move with changing events.<sup>31</sup> The midterm model also explains most of the variance in midterm outcomes. Few models of political behavior have passed such tests, particularly those of fairly complete statistical explanation, replication, and honest prediction.

We now consider the political significance of the midterm.

#### From Votes to Seats

The political consequences of a midterm election flow from the resulting partisan distribution of seats in the House of Representatives, rather than from the partisan distribution of the na-

tional congressional vote. Since, as we shall see, the character of the translation of votes into seats has shifted greatly over the years, the political meaning of the midterm has itself shifted.

For midterms from 1938 to 1970, the relationship between seats and votes is a moderately strong one—variations in votes explain 76 per cent of the variation in seats. Much of the strength of that relationship comes from the more extreme outcomes of 1946 and 1958; omitting those years, the vote explains less than 4 per cent of the variance in seat shares. The lack of a really strong and consistent relationship between votes and seats in midterms is a reflection of changes in the swing ratio—the rates at which votes are translated into seats—over the years. The change in the value of the midterm vote in terms of seats can be seen by comparing the gain in the nationwide congressional vote made by the out-party with their gain in House seats. In the 1938 midterm, for example, the Republicans gained 7.7 percentage points in their national congressional vote compared to their 1936 congressional vote—and the result was an 18.1 per cent gain in their share of seats in the House. This yielded a swing ratio of:

Swing ratio for 1938 midterm

$$= \frac{\% \text{ change in seats, 1936 to 1938}}{\% \text{ change in votes, 1936 to 1938}} = \frac{18.1}{7.7} = 2.4.$$

In the 1970 midterm, the Democrats gained 3.4 percentage points in their share of the congressional vote over 1968, but only 2.8 percentage points in seats:

$$\text{Swing ratio in 1970 midterm} = \frac{2.8}{3.4} = 0.8.$$

Thus a comparable change in the midterm vote in

<sup>31</sup> The model appears to over-respond to very large short-run improvements in economic conditions. Such improvements have been more typical of on-years than off-years. The model performs acceptably for ups and downs of less than \$150 per capita in real disposable income, which has been the case for all midterms since 1938. In addition, the model might be examined comparatively—for example, in British by-elections. Finally, the model could be extended for on-year elections (although Gallup did not ask the presidential approval question for incumbent presidents in the six months prior to on-year elections until the 1960s). On-year elections are clearly a much more complex problem; and the midterm model is best seen as an alternative to "surge and decline" and the similar approaches cited in footnotes 1-5.

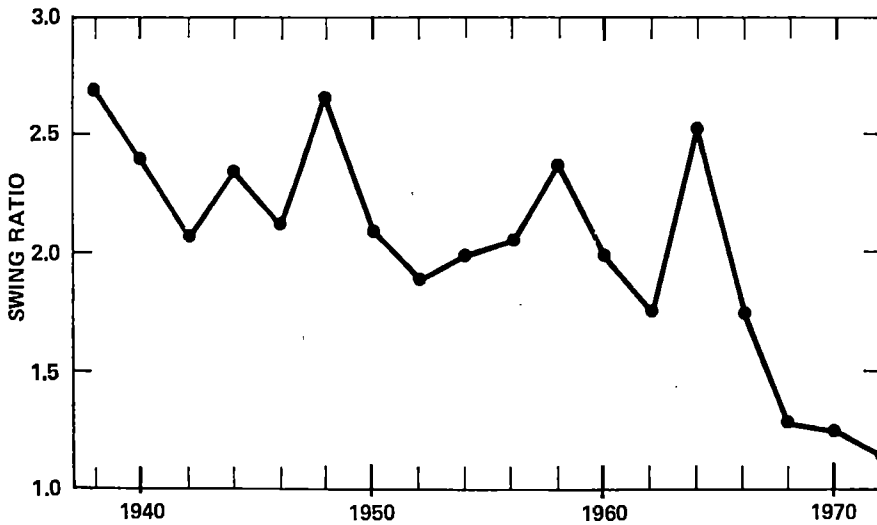


Figure 3. Swing Ratio in Congressional Elections, 1938-1972\*

\* The swing ratio is the percentage change in congressional seats associated with a one per cent change in the nationwide congressional vote.

the 1970 midterm was worth one-third what it was in 1938. The trend in the swing ratio over all the midterms has been:

1938	2.4
1942	2.0
1946	2.0
1950	2.1
1954	1.6
1958	2.2
1962	0.2
1966	1.8
1970	0.8
1974	1.5 (approximately)

Figure 3, recording the results of alternative estimates of the swing ratio, reveals the same pattern: a significant decline in the swing ratio over the years. The estimates of Figure 3 are computed as the least-squares slope calculated in the region within five percentage points of the actual nationwide congressional vote.<sup>32</sup> The estimates indicate that, in a normal election, shifts in the congressional vote are worth about half of what they were twenty years ago in terms of seats in the House of Representatives.

Figure 4 shows how the electoral systems of individual states have contributed to the nationwide change in the swing ratio in midterm congressional elections over the years. The graphs compare the translation of votes into seats for the midterm congressional elections of 1950 and 1970 in four large states. Note the difference between the seats-votes curves in the midterm of 1950 com-

pared to 1970: a flat spot in the middle of the 1970 seats-votes curve has developed and, on that plateau, changes in the congressional vote in that state yield no changes at all in the partisan distribution of seats. Some of those flat spots are rather large; a party can gain ten or twelve per cent of the vote in every congressional district in a state and still not gain a single additional congressional seat. The swing ratio in such states is, for all practical purposes, zero: any change of ordinary magnitude in the vote results in no change in seats. The plateaus in the 1970 seats-votes curves are found in the region between 40 and 60 per cent of the vote for each party—right where the statewide congressional vote falls in most relatively competitive states. That is, elections are taking place on the section of the seats-votes curve that has the lowest swing ratio. The effect of all this is to secure the tenure of incumbent representatives, since they are invulnerable to vote swings occurring in typical congressional elections.<sup>33</sup>

<sup>32</sup> The causes of recent increases in congressional tenure are not yet clear; see David R. Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity*, 6 (Spring, 1974), 295-317; Walter Dean Burnham, "Communication," and Edward R. Tufte, "Communication," *American Political Science Review*, 68 (March, 1974), 207-213; and Robert S. Erikson, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review*, 66 (December, 1972), 1234-1255. Some consequences of seat changes are described in David W. Brady and Naomi B. Lynn, "Switched-Seat Congressional Districts: Their Effect on Party Voting and Public Policy," *American Journal of Political Science*, 17 (August, 1973), 528-543.

<sup>33</sup> This technique is described in Tufte, "Seats and Votes," pp. 549-551.

Thus the electoral system—the arrangements for the aggregation of the votes of citizens into seats in the House—does not respond consistently (and hardly responds at all in some states) to changes in the aggregate preferences of voters, even though the voters themselves are casting their ballots in a systematic and focused way in midterm congressional elections. No wonder midterm outcomes—especially when they were evaluated in terms of changes in seats rather than votes—appeared, in V.O. Key’s words, as “a procedure whose strange consequences lack explanation in any theory that personifies the electorate as a rational god of vengeance and reward.”<sup>34</sup>

<sup>34</sup> Key, p. 568.

Conclusion

Our fundamental finding is that the vote cast in midterm congressional elections is a referendum on the performance of the President and his administration’s management of the economy. Although the in-party’s share of the nationwide congressional vote almost invariably declines in the midterm compared to the previous on-year election, the magnitude of that loss is substantially smaller if the President has a high level of popular approval, or if the economy is performing well, or both. The fitted model indicates that the aggregate midterm outcomes from 1938 to 1970 (omitting the wartime election of 1942) are explained by—and are predictable from—the economic condi-

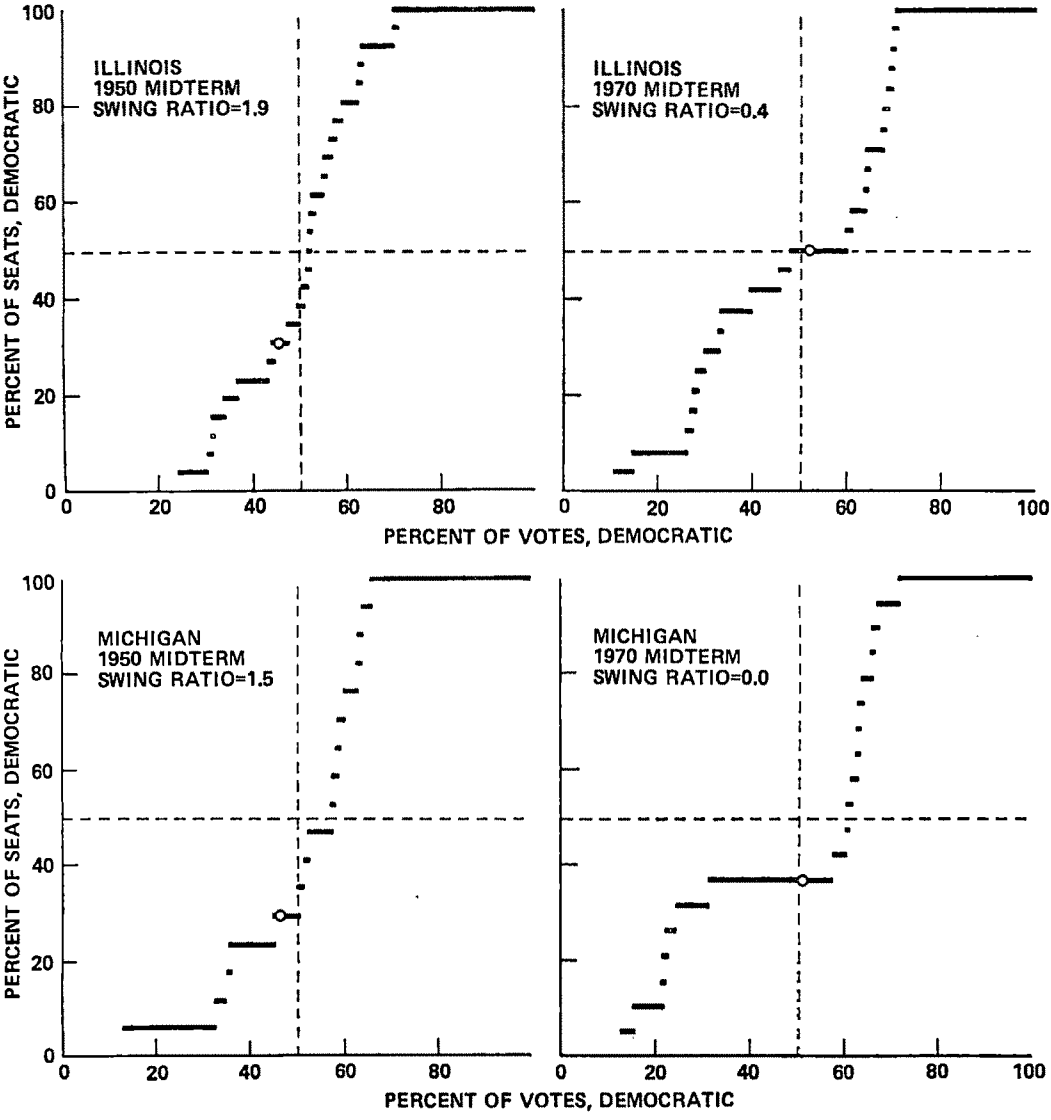


Figure 4. Votes-Seats Curves for the 1950 and 1970 Midterm Congressional Elections: Illinois, Michigan, Ohio, and Pennsylvania



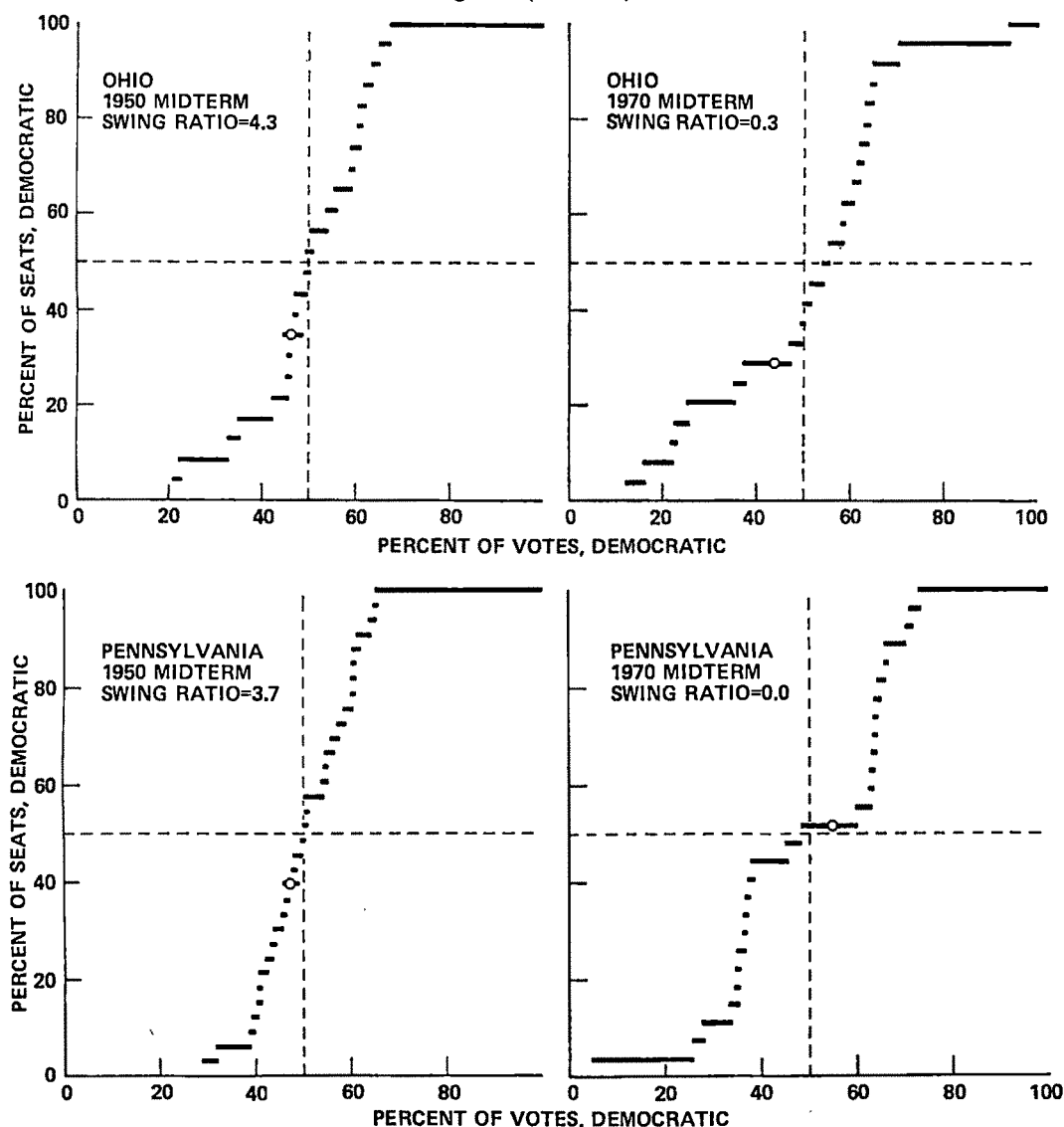
tions and the level of approval of the President prevailing at the time of the election. To be specific: a change of ten percentage points in the President's approval rating in the Gallup poll is related to a change of 1.3 percentage points in the national midterm congressional vote for the President's political party; and a change of \$50 in real disposable personal income per capita in the year of the election is related to a change of 1.8 percentage points in the vote. These estimates, although based on a relatively short series of elections, appear to be very stable and are confirmed by independent replication and by genuine predictive tests. From a statistical point of view, the model constitutes a virtually complete explanation of the aggregate vote in midterm elections: the

model explains 91 per cent of the variation in the partisan division of the vote in midterms from 1938 to 1970; the model performed successfully in predicting the outcome of the 1974 congressional election.

Our second main finding is that the midterm referendum of the nationwide congressional *vote* is often poorly reflected in the resulting partisan distribution of *seats* in the House of Representatives. Thus even though the voters, in aggregate at least, have done their best to make the midterm a referendum on the performance of the administration, their efforts are greatly muted by the structure of the electoral system.

The finding that the midterm vote does, in fact, constitute a referendum—albeit a sometimes hid-

Figure 4. (Continued)



den referendum because of shifts in the votes-seats translation—on the performance of the President and his administration's management of the economy is especially significant when compared to the political science textbook view of midterms. The standard view is, of course, that the midterm outcome derives from the prior on-year election, mostly a residual product of an electorate from which the short-term forces prevailing in the prior on-year have been subtracted. But our evidence indicates that the midterm is neither a mystery nor an automatic swing of the pendulum; the midterm vote is a referendum.

Along much more speculative lines, the midterm model also suggests a partial explanation of the fundamental fact of midterm elections, the loss of votes by the President's party. The model indicates that the loss occurs because the electorate's approval of the President has declined since the prior on-year election and because the economy is performing less well at the time of the midterm than it was two years earlier during the presidential election. Mueller has estimated the yearly decline of presidential approval at about six percentage points per year in office.<sup>35</sup> And, in general, the economy—measured here in terms of the yearly increase in real disposable income per capita—has historically performed better in on-years than in off-years.<sup>36</sup> At present, such an application of the midterm model is very speculative; I suspect that a satisfactory explanation of why the President's party always operates in the loss column in off-years will grow from a combination of the midterm model and a revised version of Campbell's "surge and decline" model (which, in revision, might place more emphasis on the surge and decline of coattail effects and less on turnout effects).

Let us conclude by considering the relevance of the findings for the eternal issue of voter rationality.

Stokes and Miller demonstrated that the midterm election could hardly be regarded as the electorate's evaluation of the legislative record of the two parties in Congress because an embarrassing number of voters lacked the minimal information required to cast a ballot informed by a judgment of a party's legislative performance. For example, a majority of those surveyed failed to recall which party controlled Congress. Less political information is demanded of voters, however, if they are to cast their midterm ballots as a referendum on the performance of the administration—only knowledge of what political party

the President belongs to. And the link between the President's party and the congressional candidate is easy, because each congressional candidate's party is printed on the ballot along with the name of the candidate.<sup>37</sup> Thus the information necessary to cast an off-year congressional vote for or against the party of the President is at hand for most voters. If the information demands on voters are minimal enough (in this case, knowing the name of the President's political party), then the aggregate performance of the electorate can be consistent with objective factors prevailing at the time of the election.

The basic idea behind the model—that, in midterm congressional elections, at least some voters reward or punish the party of President by casting their votes for representatives in line with their perceptions and evaluations of the President and economy—is a theory about the behavior of individual voters. It is important to realize, however, that all we observe in these data is the totally aggregated outcome of the individual performances of the forty million voters who turn out in midterm elections. Many different models of the underlying electorate are consistent with electoral outcomes that are collectively rational; and the observation of aggregate rationality clearly does not imply a unique specification or description of individual voters or of groups of voters making up the electorate. Aggregate studies provide evidence about aggregates. And surely for many citizens of voting age the midterm is not a referendum on the performance of the incumbent administration: some do not have the opportunity for a referendum vote since they find only one party's candidate on their ballots; others rely entirely on party affiliation, name recognition, and incumbency to guide their decisions; and a majority do not show up at the polls in the off-year.

Consequently, our highly aggregated evidence speaks only most indirectly to the central political questions concerning the rationality of voters as individuals:

- What kinds of decision rules do individual voters use?<sup>38</sup> Which voters use what decision rules?
- What conditions encourage voter rationality?
- How may these conditions be nurtured?

These concerns once again emphasize the theoretical interest (as well as the continuing practical interest) in ticket splitters, swing voters, and citizens who fail to vote in some elections—for it is, after all, the aggregate combination of these individual effects that leads to the striking collective rationality apparent in our findings here.

<sup>35</sup> Mueller, "Presidential Popularity," p. 25.

<sup>36</sup> Edward R. Tufte, "The Political Manipulation of the Economy: Influence of the Electoral Cycle on Macroeconomic Performance and Policy," manuscript, Princeton, 1974.

<sup>37</sup> Stokes and Miller, pp. 544–546.

<sup>38</sup> See Stanley Kelley, Jr. and Thad Mirer, "The Simple Act of Voting," *American Political Science Review*, 68 (June, 1974), 572–591.

# Underdevelopment and the "Gap" Theory of International Conflict\*

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The diffusion of Western culture and economic patterns to other areas of the world has proceeded unabated for more than three centuries. Its forms, often violent and oppressive, have always been tempered by a faith in the ultimate benefits that the underdeveloped world would receive through the processes of cultural and economic interaction. While apologists for colonialism did not deny the strategic, prestige, and economic advantages accruing to the metropolitan states, they would point out simultaneously that the natives were also receiving benefits, including employment, stable government, Christianity, an end to local slave trade, and the introduction of education, democracy, and modern health facilities. Nevertheless, the objectives of most colonial regimes were limited to securing advantages for planters, traders, the military, and the church.<sup>1</sup>

During the past thirty years a greatly expanded view of the possibilities for the former colonial territories has developed. The ultimate fate of the underdeveloped societies is to become fully developed. A common view is that these territories will have to become replicas of the highly industrialized states of Western Europe, North America, and the Communist countries. There is no halfway station where a society can stop on the road of modernization. The "gap" between the rich and the poor is growing, and the only solution is presumably for the poor to catch up and to become basically what the rich are. The end state of development is not the African or Asian version of late nineteenth-century Sweden (economically speaking), but the fully modern industrial urban society of 1975 or 1995. To suggest anything less is to deny the value of equal welfare and to perpetuate two classes of international citizenship.

The rationale for the egalitarian view of world development is to some extent humanitarian:

\* This is a revised version of a paper presented at the IX World Congress, International Political Science Association, Montreal, August 23, 1973. Barbara Haskel, Hal Sarf, and Janice Stein, all of the Department of Political Science, McGill University, and Ole R. Holsti, Duke University, made many useful comments on a draft of this essay. I appreciate their kindness.

<sup>1</sup> See, for examples, Richard D. Wolff, *The Economics of Colonialism: Britain and Kenya, 1870-1930* (New Haven: Yale University Press, 1974), and Ngo Vinh Long, *Before the Revolution* (Cambridge, Mass.: Massachusetts Institute of Technology Press, 1973).

everyone in the world should enjoy the blessings of the advanced countries. In other cases, economic considerations—the growing interdependence of the world—underlie the imperative for "closing the gap." Another important consideration is international peace and security. Many statesmen, politicians, and academics have argued that neither domestic turmoil nor international wars can be avoided in a world of states many of whose populations suffer from poverty. The unrequited goals of the "revolution of rising expectations" in the underdeveloped countries will lead to frustration, domestic revolutions, political instability, and messianic politics. These are likely to have spillover effects into the international system, causing regional rivalries at best and the intervention of the great powers at worst.<sup>2</sup>

Another view emphasizes the "gap" between the rich and poor nations as a source of international conflict.<sup>3</sup> The powerful industrialized states constitute a model which the underdeveloped seek to emulate. Industrialization, mass consumption economies, modern military forces, high levels of technology, and welfare-oriented politics stand as the indicators of worth in today's world. To the extent that the underdeveloped countries are not achieving anything resembling parity with those who have established these standards, or, put in another way, to the extent that they do not receive adequate proportions of the "rewards" of the international system, frustration expressed through aggressive nationalism or adventurous foreign policies will result. Though few agree on the sources of the widening "gap" (neocolonialism and imperialism are the causes according to writers of the "New Left"; economic, social, and cultural "barriers" within the underdeveloped societies are the sources, according to more orthodox interpretations), there is a consensus

<sup>2</sup> This common view is proposed, for example, in *Partners for Development: Report of the Commission on International Development*, Chairman: Lester B. Pearson (New York: Praeger, 1969), p. 7; see also Robert S. McNamara, *The Essence of Security* (New York: Harper and Row, 1968), pp. 145-146.

<sup>3</sup> See, for example, A. F. K. Organski, *World Politics*, 2nd ed. (New York: Knopf, 1968), and McNamara, p. 146; Angelos Angelopoulos, *The Third World and the Rich Countries* (New York: Praeger, 1972), p. 7. For a critique of the statistical biases and fallacies in measuring the "gap," see P. T. Bauer, *Dissent on Development* (London: Weidenfeld and Nicolson, 1971), chap. 1.

that the condition left unresolved must lead inevitably to international conflict.

I shall subject this latter thesis to a critical scrutiny, not from the point of view of international conflict theory or a theory of the causes of underdevelopment, but from a perspective which emphasizes the impact of Western, including socialist, economic solutions on indigenous cultures and social structures.<sup>4</sup> From the perspective of future international relations, we can also inquire: If some underdeveloped countries adopt more nationalist economic policies, reject some forms of Western "penetration," and isolate themselves more from international cooperative ventures, will the Western response be hostility or accommodation? The phenomenon of isolation or quasi autarchy is seldom noted in the development literature, primarily, I suspect, because it challenges long-standing normative commitments to a world of increased international integration and communication. International relations scholars have also neglected the phenomenon because our ruling paradigm—the notion of an international system—does not lend itself to analysis of such normative problems as what *sorts* of influences (and what sorts of social consequences) permeate the "nodes," "networks," and "linkages" of the system. In brief, while not rejecting the "gap" theory of international conflict, I would like to explore an alternative scenario in which the international system becomes more fragmented and some underdeveloped countries selectively reduce their "interdependence."

The scenario derives from my critique of three components of the common Western image of underdevelopment. The first is the view that once having been influenced by modern communications, the people in the underdeveloped countries universally desire to change their life-styles and to adopt most, if not all, of the economic, cultural, technological advantages of the industrialized state. Thus, the main thrust for development comes from the grass-roots level, and local elites commit themselves to modernization in order to accommodate this "revolution of rising expectations." Second, our image of underdevelopment is based on considerable pictorial or written evi-

dence of widespread poverty, despair, disease, lack of opportunity, and cultural deprivation. If the picture of underdevelopment in our heads includes these characteristics as typical, then of course the "revolution of rising expectations" seems a perfectly appropriate response. Third, given the first two components of the image, we often imagine that people in underdeveloped countries will quickly grasp the means by which their expectations can be met, readily accept the techniques necessary to lift themselves out of misery, and suffer few costs in doing so.

Many academic treatments of underdevelopment are more sophisticated than this description of these three components implies. Nevertheless, many fail to acknowledge the harmonious and satisfying aspects of the lives of nonindustrialized peoples, the costs involved in many development schemes, and the inappropriateness of some aspects of Western-style economic activity to other value systems. This shortcoming becomes apparent when one compares the writings of economists with those of many anthropologists. Economists, accustomed to thinking in terms of aggregates and quantitative indicators, necessarily make inferences about individuals from aggregate data. Most economists believe that people earning \$200 annually must suffer from extreme deprivation in all social, physical, economic, and cultural dimensions. An anthropologist who has lived among people with such incomes, however, would see that that way of life typically includes close family ties, reasonable dietary standards (except in some countries such as Bangladesh, and during periods of natural catastrophe), cooperative work habits, effective, and often humane, social controls, and intellectual stimulation through acquiring knowledge of the natural environment and skills relating to that environment.

I will distort the picture in favor of the view of many anthropologists. This is done deliberately in order to highlight corresponding distortions in the more common view of underdevelopment, and so that we can begin to think critically about those conceptions of the future international system which are based on more "interdependence," more international trade, more private investment, and more urbanization and industrialization.

#### Sources of the Commitment to Development

Given the strong resistance of many indigenous cultures to Western penetration<sup>5</sup> throughout the

<sup>4</sup> Throughout this paper, the term "Western" includes most of the developed socialist states. The literature on the causes of underdevelopment is expanding rapidly, largely fostered by the pioneering work of André Gunder Frank. See particularly his essay "The Development of Underdevelopment," in *Latin America: Underdevelopment or Revolution?* ed. A. G. Frank (New York: Monthly Review Press, 1969). A summary and critique of the underdevelopment-dependency literature is in Benjamin J. Cohen, *The Question of Imperialism: The Political Economy of Dominance and Dependence* (New York: Basic Books, 1973).

<sup>5</sup> H. A. C. Cairns, *Prelude to Imperialism* (London: Routledge and Kegan Paul, 1965); Edmund Stillman and William Pfaff, *The Politics of Hysteria* (New York: Harper and Row, 1964); Ignacy Sachs, *La découverte du tiers monde* (Paris: Flammarion, 1971), chap. 4.

past three centuries, it is somewhat surprising that today we should accept uncritically the thesis which portrays tribal peoples and peasants as grasping uncritically for the benefits of modern Western society and technology. Most political leaders in the world have referred to the "revolution of rising expectations" as if this phenomenon were the sole source of the drive toward development. To be sure, few people reject all the benefits of modern technology, science, and economic activity. With a modicum of demonstration, most people can easily see the benefits of better health or of decreasing dependency upon the vagaries of nature. No longer does everyone accept misfortune, poor housing, clothing, and diet as the will of God, and many aspire to improve themselves and their children through individual and collective action. But we have often confused these relatively simple aspirations for a better life with the assumption that everyone wants to adopt *all* Western institutions through Western-type economic activity. We have confused development with outright emulation; a reasonable standard of living with large cities, heavy industry, mass consumption, personal mobility, rejection of agriculture as an appropriate "way of life," high per-capita income, personal ownership of an automobile, and the like.

Evidence that many people in underdeveloped countries do not accept all aspects of the "developed" society as an end state of human activity is not overwhelming but appears regularly. In Kenya, South Africa, Zambia, Sicily, and Thailand, for example, many go to cities and towns to work for wages, but a high proportion return eventually to the rural areas.<sup>6</sup> They want the wages that offer them some increased economic opportunities, but the major social, psychological, and emotional supports of their lives re-

main in rural areas.<sup>7</sup> Migration back to the country occurs despite the notable bias of economic development plans (Cuba, Taiwan, and Tanzania excepted) favoring urban over rural programs.<sup>8</sup>

This is not to deny strong grass-roots impetus for the betterment of life. But a case can also be made that the strongest thrust for modernization conceived as industrialization, urbanization, and consumerism derives from the aspirations of a relatively small, Westernized elite whose members have been educated and trained in the West or have pursued career opportunities which are closely linked to Western economic institutions and international organizations.

Another important source of the development thrust is national security. Nayar<sup>9</sup> shows that rapid industrialization in Japan, the Soviet Union, China, and India has related less to public welfare considerations than to the requirements of military security and national independence. The leaders of these countries, through their speeches and actions, have argued that modernization is a necessary condition for securing and maintaining independence in an international or regional system of hostile states. Even in some of the smaller underdeveloped societies, such as Thailand, Western economic techniques were borrowed as a means of reducing Western economic penetration.<sup>10</sup>

Closely related to the perception of external threat and the desire to secure national independence are considerations of prestige. Once the leaders of underdeveloped states become integrated into the international system, they adopt

<sup>6</sup> The transfer of tribal customs and social relationships into an urban environment is possible. See Peter C. W. Gutkind, "African Urban Family Life and the Urban System," in *Urbanism, Urbanization, and Change*, ed. Paul Meadows and Ephraim Mazruchi (Reading, Mass.: Addison-Wesley, 1969), pp. 215-222.

<sup>8</sup> This bias is acknowledged in "International Bank for Reconstruction and Development," in *World Bank Operations: Sectoral Policies and Programs* (Baltimore: The Johns Hopkins University Press, 1972), p. 418. For a study of the antirural bias of development planners in India, see M. Lipton, "Urban Bias and Rural Planning in India" in *Development and Underdevelopment: The Third World Today*, ed. Henry Bernstein (Hammondsworth: Penguin, 1973), pp. 235-253; for Africa, see Gerald K. Heleiner, "Structural Change in Africa," in *The Widening Gap*, ed. Barbara Ward, J. D. Runnals and Lenore D'Anjou (New York: Columbia University Press, 1971), p. 92.

<sup>9</sup> Baldev Raj Nayar, "The Political Mainsprings of Economic Planning in the New Nations: The Modernization Imperative Versus Social Mobilization," paper presented at the conference "Asia in the Seventies," Carleton University, Ottawa, November 1971.

<sup>10</sup> Jacobs, *Modernization without Development*, pp. 131-133.

<sup>6</sup> See, respectively, Colin Leys, "Politics in Kenya: The Development of a Peasant Society," *British Journal of Political Science*, 1 (July, 1971), 307-337; Sheila T. Van der Horst, "The Effects of Industrialization on Race Relations in South Africa," in *Industrialization and Race Relations: A Symposium*, ed. Guy Hunter (London: Oxford University Press, 1965), p. 112; Norman Long, *Social Change and the Individual* (Manchester: Manchester University Press, 1968), chap. 9; Johan Galtung, *Members of Two Worlds: A Development Study of Three Villages in Western Sicily* (New York: Columbia University Press, 1971); Norman Jacobs, *Modernization Without Development* (New York: Praeger, 1971), p. 178. Nevertheless, the growth of urban population in Africa, even if unstable, has been phenomenal. Most cities doubled their populations during the 1960s. For statistics, see Donald G. Morrison, Robert C. Mitchell, John N. Paden, and Hugh M. Stevenson, *Black Africa: A Comparative Handbook* (New York: The Free Press, 1972), part 2.

the measures of worth of that system.<sup>11</sup> Jacobs, for example, points out that a prime stimulus for economic modernization among the Thais is not the felt needs of the people, but the needs of the patrimonial leadership, which include the desire to be "politically respected in the halls of the developed Western powers."<sup>12</sup> Hence, as has been noted often in the literature, many visible indicators of modernity—commercial urban centers, national airlines, heavy industries, middle-class suburbs, and a plethora of private vehicles and consumer goods—are promoted less to assuage popular economic expectations than to bring diplomatic influence and prestige to the nation.

It is difficult to assess the relative weights of these sources of the drive for modernization. Undoubtedly they vary greatly from country to country, and in many instances large proportions of the population support their leaders' quest for international recognition and securing independence. Taken together, however, socialization, national security, and international prestige probably provide as plausible an explanation for the commitment to create a replica of the industrial states as does the hypothesis of a grass-roots revolution of rising expectations.

Some leaders, of course, do not see their nations as potential miniatures of Britain, the United States, or the Soviet Union. They have deliberately created alternative welfare models and pursued development strategies reasonably consistent with indigenous social and economic patterns. Julius Nyerere of Tanzania, for example, sees the goal of development as a reasonably independent economy based on local ownership, little direct dependence upon the major powers for capital or technical assistance, and a more or less egalitarian social structure in which the major segment, the peasants, work communally and cooperatively in accordance with, not contrary to, traditional social patterns.<sup>13</sup> Thai political leaders, despite their rhetoric in favor of economic rationalization and industrialization, maintain a firm commitment to political and economic forms which are based on patrimonial personal relations and which incorporate uniquely Thai ideas of moral goodness.<sup>14</sup> Other exceptions could be cited; but most leaders of underdeveloped states have adopted the Western (including variations of socialism) developed

state as their goal or model, as well as Western analyses of their social and economic problems, and Western strategies for overcoming them.

Their goal is *economic growth*. United Nations reports indicate that although most governments also talk about social progress, equality, and advancement of culture, their budget allocations and administrative performances are geared primarily to a particular type of economic development.<sup>15</sup> These and other reports make it clear that the philosophy of Western-style economic development has spawned strategies favoring industry over agriculture, urban over rural areas, the modern over the traditional sector, and large capital-intensive projects over smaller labor-intensive initiatives. Income is distributed primarily toward urban middle classes, thus encouraging migration to cities and discouraging agricultural development.<sup>16</sup> The priorities of development policies thus seem to reflect short-run considerations of security and prestige, as well as a rather uncritical acceptance of Western definitions of development.

#### Western Images of Underdevelopment: The Sources of Bias

There are frequently contradictions between popular Western images of underdevelopment (including assumptions about the pervasiveness of the revolution of rising expectations), and the social and economic realities some investigators find in underdeveloped societies.<sup>17</sup> Most striking is the strange conservatism among vast populations: many simply will not act according to Western conceptions of rational economic behavior. The resistance to urbanization, mechanization, capital savings, regular work schedules, achievement criteria, production of surplus, and to many other facets of Western commercial behavior is often noted in the literature, but less often taken seriously as evidence that some Western development goals and strategies are simply inappropriate for other value systems. The literature, moreover, makes no effort to link these resistances to foreign policy or to the overall character of relations be-

<sup>11</sup> Goulet, p. 70.

<sup>12</sup> *World Bank Operations*, pp. 96-97. Until the late 1960s, India's priorities lay clearly with heavy industry. Although 80 per cent of the population was employed in agriculture, only 20 per cent of public development expenditures went to this sector. See *Partners in Development*, pp. 287-288.

<sup>13</sup> Dan Usher describes finding that an early conception of living conditions among the Thais, based on economic studies, was greatly at odds with the reality he experienced living among them. The low per-capita income figures indicated virtually nothing about material welfare or sense of well-being. See his *The Price Mechanism and the Meaning of National Income Statistics* (Oxford: Oxford University Press, 1968), intro.

<sup>14</sup> Denis A. Goulet, *The Cruel Choice: A New Concept in the Study of Development* (New York: Atheneum, 1971), p. 80.

<sup>15</sup> Jacobs, p. 126.

<sup>16</sup> See the paper, "Ujamaa—The Basis of African Socialism," in Julius K. Nyerere, *Freedom and Unity: A Selection of Writings and Speeches* (London: Oxford University Press, 1967), pp. 162-171 and 183-188. The Arusha Declaration of 1967 explicitly rejects a Western consumption-oriented society for Tanzania.

<sup>17</sup> Jacobs, chap. 5, and pp. 314-316.

tween developed and underdeveloped countries. The "gap" theory of international conflict rests squarely on the notion of economic growth as the primary goal of underdeveloped societies and on the hypothesis that failure to reach the economic levels of the developed states will lead to frustration and aggressive nationalism. It is less often acknowledged that development itself may create strains and turmoil that will spill over into the international system.

A number of reasons could be put forth to explain inconsistencies between images and realities. Given graphic details of the worst aspects of lives of people in underdeveloped societies—the street people of Calcutta or the *favelas* of Latin America—we assume that it is only natural for others to want what we have. We are seldom told, however, that often the worst social blights in the underdeveloped world are the *results of development policies* which place highest priority on urbanization, heavy industry, and "growth" in economic indicators, while simultaneously neglecting agricultural and rival problems.<sup>18</sup>

Others have looked at the problem of underdevelopment primarily in terms of the Cold War and have seen only two paths or models of development—socialism and free enterprise. The possibility of some entirely new strategies for development, or goals which realistically fall far below a per capita income of \$800, are simply not considered; an economy based on farming, fishing, handicrafts, and some light industry and commerce just does not add up to a socialist or Western image of being "developed."

The main reason for failure to understand indigenous resistance to development may be, however, common Marxist and liberal assumptions about the worth of economic activity. Development analysts for the most part carry these assumptions into their images of the underdeveloped world, into the goals they project, and into the strategies they propose for achieving those goals. We need to go back at least to Bentham to find the intellectual source of our contemporary preoccupation with economic growth. His simple assumption was that the maximization of wealth led to the maximization of happiness ("each portion of wealth has a corresponding portion of happiness . . ."). Not only does Bentham describe the basically appetitive nature of man in economic terms, but he is among the first to argue that acquisitiveness is both rational and morally

commendable in its consequences for private and public welfare. His view of the economic mainsprings of human activity still characterizes Western images of development. It is his version of "economic man" which most developers are trying to mold or create; they cannot see how people who do not share Bentham's view of human motivations can be rational or happy. The prime prerequisite for aid programs is thus to instill a respect for materialism and to overcome all those aspects of a culture which impede economic activity.<sup>19</sup> Economic sectors composed of producers who meet only immediate needs are labelled "stagnant." If there is no economic "growth," there can be no cultural progress.<sup>20</sup>

Take one example. Norman Jacobs presents an interesting analysis of modernization and development in Thailand in which he laments the fact that values other than economic maximization are rampant in the society. While Jacobs cannot entirely hide an admiration for the Thais' proverbial propensity to enjoy life—even to the point where they would rather go to a party or ceremony than show up for work—his overall conclusion rests squarely in the tradition of Western writing on underdevelopment: no matter how one may enjoy life through noneconomic activities, it is better to work hard and create institutions which encourage savings, long-run investment, production beyond immediate needs, strict work schedules, and the like.

Now if development (defined by Jacobs as "maximizing the economic potential of the environment")<sup>21</sup> is the most important personal and national goal, then of course conventional analyses of the problem of underdevelopment may be valuable. But many discussions of development fail to demonstrate that economic maximization is a superior value for the individual, even if it is important at the aggregate level. In some societies, of course, overpopulation forces governments to act *as if* production were the first national and individual priority. In those areas where there is still considerable land available and where some people prefer not to adopt Western-style value structures, should they be condemned

<sup>19</sup> Among the many examples in the literature, see the attitudes expressed implicitly and explicitly in George M. Guthrie, *The Psychology of Modernization in the Rural Philippines* (Quezon City: Ateneo de Manila University Press, 1970).

<sup>20</sup> Virtually all commentators characterize nonindustrializing and non-high-growth economies as "stagnant." For one example, see E. K. Fisk, *The Political Economy of Fiji* (Canberra: Australian National University Press, 1970). Fisk praises the Indian community in Fiji for its aggressive economic advancement, while criticizing native Fijians for their lack of interest in large-scale economic pursuits.

<sup>21</sup> Jacobs, *Modernization Without Development*, chap. 5.

<sup>18</sup> For an account of the depressing effects of unbalanced urban industrialization on a rural population, and the distortion of government expenditures in favor of the modern sectors, see Robert H. Bates, "The Policy Origins of Migration in Zambia," paper presented at the meeting of the American Political Science Association, New Orleans, September, 1973.

or urged to act to the contrary? The unwillingness to save, to increase output, or come to work according to a rigid schedule should indicate something about the preferences of people about their life-styles, rather than being portrayed as "irrational" behavior, the consequences of superstition, or plain stubbornness.

But, it can be argued, most of the people we are discussing live in such pitiful conditions that they must be shown how to become economic maximizers. If we go to the extreme of cultural relativism and accept *all* traditional practices as "functional," we are overlooking the problem of some objective minimum standard of general welfare. Growing populations, urbanization, and the requirements of international politics do not allow us the luxury of respecting traditional values and pursuits. Modernization can only come about through breaking up traditional social and economic patterns.

There is merit in this reply, particularly if we agree that in all cases population pressures allow no alternatives, and uncritically accept the assumption that a precarious economic existence must cause intellectual and cultural poverty as well. These positions may be substantially correct in countries such as India, Egypt, Pakistan, and in regions subject to natural disasters. Even in moderately developed countries such as Mexico, recent studies describe rural life in a manner to substantiate Hobbes's characterization of man in the state of nature.<sup>22</sup> Oscar Lewis's scenarios show a life with no reprieve from economic insecurity, a life of constant fear and distrust of friends and neighbors and of virtually no opportunities for self-betterment.<sup>23</sup> In southern Italy and Sicily, too, peasant life is known for "La Miseria." Edward Banfield's classic study, *The Moral Basis of a Backward Society*,<sup>24</sup> leaves little doubt that some sort of economic development is essential if the people of the region are to enjoy even a modicum of opportunity and a diminution of distrust, suspicion, and miserliness.

Yet when we realize that many of these studies portray societies undergoing rapid economic and social change, or describe characteristics that are also prevalent in developed societies (gossip, insecurity, and the like), then other portrayals take on more credibility. Robert Redfield, after years of field research in Mexican peasant communities, came to appreciate many aspects of peoples' lives

in an underdeveloped community.<sup>25</sup> He not only pointed to the integrative and satisfying aspects of small community life, but implied that mental balance cannot be sustained in a society undergoing rapid change. While Redfield was not opposed to change, he saw that raising expectations which cannot be fulfilled only creates frustration, and that a great deal of so-called development creates wants "whose satisfaction brings no satisfaction."<sup>26</sup> More important, "[the peasants'] way of life, the persisting order and depth of their simple experiences, continue to make something humanly and intellectually acceptable of the world around them."<sup>27</sup> More contemporary observers of peasant communities are similarly impressed by, and respectful of, traditional cultures, and sensitive to the problems created by technological innovation, industrial development schemes, and the general penetration of relatively isolated communities by Western values and mores.<sup>28</sup> Redfield's observations are by no means exceptional. Thus, the wretchedness of most underdeveloped peoples' lives should be viewed as a hypothesis, not as a universal fact. At the very least, the first component of the common Western image—widespread economic and intellectual poverty—needs to be spelled out in much greater detail. The urban slum-dweller of Bogotá cannot be equated with the South Pacific islander or the Thai subsistence farmer. Per capita income figures reveal little about happiness or misery among such diverse people, much less about the importance of economic activity to their welfare. Indeed, the whole notion of "development" needs to be broadened to include more profound objectives such as esteem, self-actualization,<sup>29</sup> justice, and the maintenance of emotional communities. Economic growth may have only slight impact on these forms of development, and in some cases it is actually inconsistent with them. A brief review of some of the nonfinancial costs of economic development policies will help explain why resistance to development exists, and why the "revolu-

<sup>22</sup> Chan Kom, *A Maya Village* (Washington: Carnegie Institution of Washington, 1934); Tepoztlán: *A Mexican Village* (Chicago: University of Chicago Press, 1930); and *The Little Community, and Peasant Society and Culture* (Chicago: University of Chicago Press, 1960).

<sup>23</sup> *The Little Community*, p. 63.

<sup>24</sup> Redfield, *Peasant Society and Culture* (Chicago: University of Chicago Press, 1956), pp. 132–133.

<sup>25</sup> For others, see John Greenway, *Down Among the Wild Men* (Boston: Little, Brown, 1972); Aliczia Iwanska, *Purgatory and Utopia* (Cambridge, Mass.: Shenkman, 1971); Lisa Peattie, *The View from the Barrio* (Ann Arbor: University of Michigan Press, 1968); Colin M. Turnbull, *Tradition and Change in African Tribal Life* (Cleveland: World Publishing Co., 1966).

<sup>26</sup> Goulet, *The Cruel Choice*, chap. 3.

<sup>22</sup> For a summary of the debate on characteristics of peasant societies, see George Foster, "Interpersonal Relations in Peasant Society," in *The Human Factor in Political Development*, ed. Monte Palmer (Boston: Ginn, 1970).

<sup>23</sup> Oscar Lewis, *Five Families: Mexican Case Studies in the Culture of Poverty* (New York: Basic Books, 1959).

<sup>24</sup> (Glencoe, Ill.: The Free Press, 1958).



tion of rising expectations" provides a questionable basis for the theory that international conflict will result from the "gap" between developed and underdeveloped countries.

### The Costs of Development

George Foster's analysis of underdevelopment and his solutions to it are reasonably typical of the literature. The requirements for inducing development include reducing the strength of conservative forces or neutralizing their results, while simultaneously strengthening the forces of change.<sup>30</sup> How is this to be done? Among other things, all cultural and social "barriers" to development must be overcome. These include "the basic values of [a] group, its conception of right and wrong . . . [and] the 'fundamental fit' or integration of its parts. . . ."<sup>31</sup> Barriers such as the social structure of groups and the prevailing type of family relationships must also be amended so that economically maximizing behavior can result. In short, social and cultural patterns which are incompatible with industrialization, "community development," and many other favored Western notions of what constitutes a good society, must be eradicated. While Foster does not endorse the "stern measures" advocated and practiced by developers in the socialist countries,<sup>32</sup> it is not hard to see why people who have any pride in or attachment to their culture, folkways, and social patterns might react negatively to such prescriptions for "growth."

One of the foremost analysts of underdevelopment, Gunnar Myrdal, appears no less impatient of those "barriers" to systematic national planning and Western-style economic activity and institutions. His major work in the field, *Asian Drama: An Inquiry into the Poverty of Nations* (1968), is replete with disparaging references to language and ethnic differences among peoples, to religious traditions and customs which go back several thousand years, and to other social institutions which seem incongruent or incompatible with drastic land reform, heavy industrialization, and above all, with nationwide planning programs that move people in the same manner that one moves nonhuman resources. Myrdal is not reticent in urging compulsion where growth is in-

hibited by noneconomically maximizing behavior—in short, he believes man should be forced to become more wealthy.<sup>33</sup>

Most economists and anthropologists note that there are costs to be paid in programs which involve breaking down traditional social patterns and practices. But they seldom dwell on these costs, and very few indeed have acknowledged that certain forms of development are not worth the price they extract.

Consider some of the development costs listed by Foster and others.

- (1) Destruction of the "extended family." By breaking down traditional family patterns which emphasize mutual obligation, a productive man is liberated so that he can become part of a mobile labor force. He casts off his family obligations (presumably to be taken over by the state) in order to maximize his economic and, perhaps, career possibilities. The destruction of the extended family is directly attributable to the demands and opportunities of modern economic organization and of technology.<sup>34</sup>
- (2) Destruction of traditional rural cooperative work patterns. Although communal work may not be as productive as individual enterprise, it fulfills social needs that cannot be met through private labor. The introduction of cash crops and money are directly linked to the demise of communal work in rural areas.
- (3) As peasants become integrated into a commercial economy, dietary standards often decline. According to various studies cited by Foster,<sup>35</sup> a significant proportion of malnutrition in the world can be attributed to the substitution of packaged foods for traditional dietary sources. People purchase packaged foods primarily for their prestige value, the efficacy of advertising, or aesthetic rather than dietary reasons. (This phenomenon raises the question of measuring primary need satisfaction. Most development planners would identify increased cash spent on food as indicating growth and progress, whereas from a dietary point of view, needs might be met less adequately than previously. The availability of packaged foods, like many other commodities, is also linked to escalating definitions of primary needs among populations; once obtained, they tend to become necessities.) Another study discloses that many mothers in agrarian societies, imitating middle-class patterns in the West, have given up breast feeding their babies, with resulting undernourishment and protein deficiency.<sup>36</sup>

<sup>30</sup> George Foster, *Traditional Cultures and the Impact of Technological Change* (New York: Harper, 1962), p. 59.

<sup>31</sup> *Ibid.*

<sup>32</sup> The ethnocentrism of analysts of underdevelopment in the socialist countries is remarkably explicit. All local customs which inhibit industrialization and the development of an urban proletariat must be destroyed, no matter at what cost. There is only one route to modernization, namely heavy industry and collective agriculture, based on the Soviet pattern. V. G. Solodovnikov, *Afrika vybirat put* (Moscow: Hayka, 1970).

<sup>33</sup> For a critique of Myrdal's approach to economic development, see Bauer, *Dissent on Development*, chap. 5.

<sup>34</sup> Foster, *Traditional Cultures . . .*, pp. 30-40; Hafeez M. Said, *The Village Culture in Transition* (Honolulu: East-West Center Press, 1970), pp. 140-143.

<sup>35</sup> Foster, *Traditional Cultures . . .*, pp. 33-37.

<sup>36</sup> An FAO study cited in Conrad M. Arensberg and Arthur H. Niehoff, *Introducing Social Change* (Chicago: Aldine, 1971), p. 126.

Many other costs could be cited. Perhaps chief among them is the rise of class distinction. In villages which are patterned on communal labor and mutual help, introduction of larger incomes and the availability of Western-style consumer goods creates status distinctions that are disruptive to cooperation, political decision making, and to inter- and intrafamily harmony.<sup>37</sup>

But what is most deficient about the notion of economic "growth" as measured by increased per capita income is that it reflects little about the sense of well-being. Various studies show that there is no apparent connection between the massing of consumer goods—even home comforts—and happiness. Development breeds new wants and the perception of new needs, but when these have been met, the people do not feel they are better off.<sup>38</sup> At the same time, one investigator, reflecting his American values, was surprised to find a great amount of contentment among rural Filipinos living at subsistence or near subsistence levels.<sup>39</sup> Such contentment did not in the least suggest to the researcher that perhaps the people in question should be left "underdeveloped."

Finally, there are the indicators of social stress resulting from rapid cultural change. These do not appear everywhere, of course, but the question of people's ability to undergo change involving destruction of traditional economic and social patterns is not yet understood well enough. (Even in the United States, government officials have resisted the idea of an annual "social state of the union" report. The traditional "State of the Union message" is concerned mostly with economics, as if all other "states" depended upon the rise and fall of economic indicators.) Consider the following report upon the consequences of a well-intentioned Danish program to offer training, economic opportunities, and the cultural benefits of town life to native Greenlanders: According to a Greenland member of the Danish parliament, many benefits have resulted from these programs, "but we young Greenlanders find that the creation of a modern social pattern has not been accompanied by human growth, mental stability, psychological balance—in short, human well-being—in the new environment. Families are breaking

up, crime is increasing, alcoholism is spreading, even to the very young. The number of psychological disturbances is increasing, also among school children. Many people discern an inner confusion and disillusion. The social privation of the past is being replaced by human disintegration."<sup>40</sup> Similar observations have been made about Fiji, the Philippines, and the aborigines of Australia.<sup>41</sup>

Recognition of these and other costs have not, for the most part, reduced the zeal with which many governments and some populations pursue the road to modernization. Apparently most have agreed that on balance the costs are worth the advantages, or, without looking around for alternative images or strategies of development, they have assumed that the degree of need allows no other course of action. Most, however, simply do not make an assessment of the relative balance between the costs and advantages. The latter are assumed to be obvious and preponderant. Yet, we would expect that as some of the costs become more obvious and painful, some governments, supported by people facing cultural stress, will begin to acknowledge the political benefits of resisting further westernization. It is then that we could expect to see more "buffers" erected by the governments of underdeveloped states against the penetration of foreign influences.

### The "Maldeveloped" Societies

Most of the literature on development assumes that the paths we have trod must serve for others as well, give or take a few national peculiarities.<sup>42</sup> But it is becoming increasingly apparent that as our own growth continues at astonishing speed, increases in indicators of social stress are occurring with disturbing regularity. The British government's recent "official social commentary" on the state of England shows impressive economic gains over the past decade but reveals that in the

<sup>37</sup> George Boulton, "Danish Plan for Greenland Too Fast," *The Montreal Star*, October 12, 1972.

<sup>38</sup> Statement of Fiji minister of tourism to a meeting in Djakarta, reported in *The Sun* (Vancouver), Sept. 4, 1974, p. 34; "Civilization Brings Grief to Aborigines," *The Sun* (Vancouver), Sept. 19, 1974, p. 42; McKendry et al., p. 18.

<sup>39</sup> Possibly this assumption is being questioned more frequently today. See, for example, the testimony of U. Alexis Johnson, former Under-Secretary of State in the United States, before the House Subcommittee on National Security Policy and Scientific Development. Johnson claimed that the concept of development needed to be redefined to eliminate the worst effects of industrialization as experienced in the advanced countries. U.S., House of Representatives, Committee on Foreign Affairs, Subcommittee on National Security Policy and Scientific Development, *National Security and Changing World Power Alignment*, 92d Cong., 2d Sess., 1972, pp. 362-394.

<sup>37</sup> For other costs, see Herbert Blumer, "Industrialization and Race Relations," in *Industrialization and Race Relations*, ed. Hunter, p. 226; V. H. Joshi, *Economic Development and Social Change in a South Gujarat Village* (Baroda: University of Baroda Press, 1966), pp. 101-113; and James M. McKendry et al., *The Psychological Impact of Social Change in the Philippines* (Science Park, Penn.: H. R. B. Singer, Technical Report 857-R-2, n.d.), pp. 66, 72.

<sup>38</sup> Goulet, *The Cruel Choice*, p. 80; Redfield, *The Little Community*, p. 62. Quantitative evidence supporting this statement is found in Guthrie, *The Psychology of Modernization* . . . , pp. 90, 96.

<sup>39</sup> McKendry et al., p. xii.

same period violent crimes have almost trebled and that many other indicators of serious social problems are rising at a rate much faster than population growth. The number of women employed part- or full-time in the world's oldest profession has increased at an alarming rate in Italy and Israel, while in the United States, murder, assault, and burglary rates have doubled in most cities during the past decade. The number of people hospitalized for mental illness is growing proportionately much more rapidly than is the general population—one-half of all beds in American non-Federal hospitals are occupied by people suffering from mental disorders<sup>43</sup>—while child beatings are reported to be increasing dramatically. Alarmed physicians note that child beating usually serves the needs of the attacking adult seeking relief from uncontrollable anger and stress.<sup>44</sup> For their part, the young in many countries are contributing to an ever-swelling juvenile delinquency rate. While all these phenomena cannot be attributed to a single cause such as social change or urban density, it is significant that their highest incidence is commonly found in urban areas.<sup>45</sup>

It may be that the worst *physical* aspects of rapid industrialization, so precisely described by Dickens and Zola, are behind us. What may lie ahead of us is social stress and possible breakdown. The other manifestations of the maldeveloped society—pollution, destruction of wilderness, waste, depletion of resources, and the like—are too well-known to require further comment. In the face of these physical and social consequences of rapid change, we must begin to wonder if our prescriptions for others are really well-advised. The model of the “developed” state seems increasingly tarnished. When elites or others already subject to the strains caused by development begin to learn of our difficulties, they may well question whether they are willing to undergo similar consequences. Disillusionment with the “developed” model may thus become another source of policies which attempt to isolate underdeveloped areas from certain forms of Western penetration.

<sup>43</sup> Noted in Robert C. Wood, “The Future of Modernization,” in *Modernization*, ed. Myron Weiner (New York: Basic Books, 1966), p. 49.

<sup>44</sup> *The Globe and Mail* (Toronto), August 18, 1971, p. 8. The rising figures on child abuse may be accounted for, on the other hand, by new legislation requiring physicians to report incidents to police authorities.

<sup>45</sup> A study by W.M.S. and Claire Russell demonstrates the positive correlation between urban density and crime rates, reported in *The New York Times*, August 16, 1970, sec. 1, p. 53. Care must be taken in interpreting crime and mental health statistics, as these may be manipulated for political purposes, or they may reflect better detection methods.

#### Avoiding Conflict between Underdeveloped and “Maldeveloped” States: Some Solutions to the “Gap” Problem

The Pearson Report, government statements in support of private investment and foreign aid, and academic treatises as well agree basically with the view expressed by former Under-Secretary of State U. Alexis Johnson in 1972 that the growing gap between the rich and poor is a “fundamentally unacceptable situation [which is] explosive in a political sense.”<sup>46</sup> The solutions offered to this problem have been mostly continuations and expansions of past approaches: more trade, lower tariffs in the industrial countries, more private investment, and more international cooperation. For Mr. Johnson, the solution is greater international cooperation and increased interdependence. The “foundation bricks” of his image of a more egalitarian world order are being laid by private citizens, and particularly by private businesses and multinational corporations. The gap will be closed to the extent that these private networks of association grow. While Mr. Johnson is expressing a private opinion, his view is consistent in its fundamentals with the solutions proposed by many governments and international agencies.

Another view, expanded at length by Denis Goulet, argues that the gap can never be closed until the developed states allow the other areas of the world to share in their decision-making power and wealth. All the exploitative aspects of contemporary international economic relations must be ended, and the small states suffering from underdevelopment, status inconsistency, or whatever, must be made into active partners in the formulation of world monetary, trade, and aid policies. But the main ingredient of the solution must be a willingness by the “maldeveloped” states to *decrease* their levels of consumption in order that others may have more. There must be a fundamental redistribution of political power and economic resources in the world.

Mr. Johnson's solution contains several immediate problems. It assumes, first, the availability of infinite resources in the future. While Johnson may recognize some of the shortcomings of advanced societies, his plea for more interdependence—more trade, private investment, tourism, and extraction of natural resources—does not reveal how it attacks such fundamental problems as overpopulation and the present distribution of energy use. Edward Woodhouse has argued recently that given finite resources, we can no longer adhere to the view that economic development should be the primary goal of all states, or that “unlimited urbanization, indus-

<sup>46</sup> *National Security and Changing World Power Alignment*, Johnson's testimony on August 7.

trialization, and GNP growth . . . are in fact possible or worthy any longer."<sup>47</sup> In his opinion, development as it has been defined by most economists is a chimera. The dwindling availability of resources, in addition to the pollution problem, simply will not allow the world to be composed of 140 or more highly industrialized collectivities. More "interdependence" as proposed by Mr. Johnson sounds as if there should simply be more "growth" in all economic dimensions.

There is little reason to believe, moreover, that the governments of underdeveloped countries will continue to accept uncritically those relationships which Western governments and multinational enterprises define as "interdependent." To an increasing number of observers, these relationships are being defined not as "cooperation," but as dependence (at best) or imperialism (at worst). The continuation of present economic trends in the world is not likely to allay these fears; quite the contrary. More trade, more aid, more private investment, more tourism, and more private contacts—the traditional ingredients of the liberal view of internationalism—are all implied in Johnson's formula, but he fails to take into account that the consequences of these types of interactions often create feelings of resentment, racial antagonism, and perceptions of exploitation.

The solution offered by Goulet seems attractive. It acknowledges the problems of dwindling resources, and unequal decision-making power and economic rewards in the international system. But there is little evidence that any government in a "maldeveloped" state is even beginning to think in his terms. While many governments have begun to organize anti-pollution programs, none has yet instituted a regime of rationing in order to reduce pollution or to save resources . . . even for its own population. That it would do so to redistribute the world's resources in favor of the underdeveloped states seems highly unlikely in the foreseeable future.

#### **The Underdeveloped States Reject the "Maldeveloped" States: An Alternative View of the Source of International Conflict**

Although academic researchers show little agreement on the correlation between the state of the underdevelopment and the propensity to engage in aggressive foreign policy,<sup>48</sup> we cannot dis-

miss entirely the "gap" theory of international conflict. There might well be more military crises triggered by domestic turmoil in the underdeveloped countries, and the "have nots" may, as revealed in the Arab oil embargo of 1973, undertake aggressive policies toward the "haves."

Many governments, of course, have probably gone too far down the path of industrialization, urbanization, and consumerism to change their course significantly. Many will continue to pursue Western-type development for reasons of national security or international prestige, or because their westernized elites (including Marxists) remain cut off from their roots. Most of the Latin American countries are already heavily urbanized and, despite large population increases, have shown impressive economic growth rates. India, Egypt, and some of the other more overpopulated countries will probably continue industrialization programs enabling them to maintain regional leadership roles, prevent overdependence on great powers for military supplies, or avoid the consequences of not having adequate arable land to sustain rapidly growing populations in rural areas. The alternative scenario, then, would refer primarily to some of the smaller states in Africa, Southeast Asia, the South Pacific, and possibly the Middle East. These are the states already on the periphery of the international system, least touched by Western influences and thus perhaps most vulnerable to cultural dislocation caused by increased penetration from outside.

These states, rather than accepting greater interdependence, would seek to reduce their penetration of external societies. Tourism would be discouraged; foreign aid turned down; aid experts expelled; foreign corporations nationalized; and new multinational enterprises denied opportunities for exploitation of natural resources or development of markets. While not all forms of contact would cease—trade, normal diplomatic contacts, and some international cooperative programs would remain—those which were identified as "endangering" the local culture or creating undue dependency on outside states would be curtailed or terminated. The models of Burma under General Ne Win and Sikkim until recently come to mind.

What sorts of evidence might be put forth to support even a minimal confidence in the scenario as a possibility? There is no systematic evidence yet, much less an identifiable trend, but some indicators of the rejection of liberal nostrums for the "gap" problem are beginning to appear. First, at the grass-roots level, there is the persistence of millennial movements in many African and some South Pacific societies. Lanternari<sup>49</sup> shows that

<sup>47</sup> Edward J. Woodhouse, "Re-visioning the Future of the Third World: An Ecological Perspective on Development," *World Politics*, 25 (October, 1972), 9.

<sup>48</sup> For example, Rudolph J. Rummel, "The Relationship Between National Attributes and Foreign Conflict Behavior," in *Quantitative International Politics: Insights and Evidence*, ed. J. David Singer (New York: The Free Press, 1968), pp. 187-214. See, however, the chapter by Michael Haas in the same volume.

<sup>49</sup> Vittorio Lanternari, *The Religions of the Oppressed: A Study of Modern Messianic Cults* (New York: Knopf, 1963).

almost all of these movements and their early twentieth-century predecessors are a response to extreme external disturbances to indigenous ways of life. The people seek relief from their frustrations and sufferings in religious ways, usually before they turn to political means. It is thus possible to speculate that many of the adherents of millennial movements today might someday constitute a fertile ground for strongly anti-Western political leaders. The religious prophets of these movements could also adopt political roles.<sup>50</sup>

Second, most of the current generation of leaders in the underdeveloped countries are highly "westernized." But what of a new generation of leaders, some of whom will be less personally familiar with the West and whose careers might be founded on a populist base that contains anti-Western attitudes? Is President Amin of Uganda—a man whose career includes few of the experiences his African counterparts had in Western societies—an exception, or might others of his persuasion and tactics follow? If nativist political groups begin to flourish, will the external sources of the commitment to development—prestige, security, and socialization patterns—remain so potent?

It has been argued, third, that part of Allende's appeal in the 1970 elections in Chile was based on his handling of the issue of foreign influence in the Chilean economy. Aside from the well-known American corporations operating in the country, there was a rapid growth of foreign ownership of Chilean manufacturing in the late 1960s.<sup>51</sup> While the Chileans were by no means rejecting Western patterns of economic organization in general, the popularity of appeals against foreign economic penetration was undeniable. Chile will probably not be the last country which decides to accept the costs of decreasing foreign economic influence in order to enhance governmental control over the direction of development.

Fourth, the notion of mutual benefits accruing to underdeveloped countries through capital transfers and the operations of foreign-based corporations is being increasingly scrutinized. Except among Marxists, it has been commonly assumed until recently that private investment from the industrialized countries brings significant economic gains to the host underdeveloped countries. The transfer of technology and of managerial skills, along with the training of a skilled work

force, is a positive consequence of this activity. And a good case can be made that foreign investment and production which lead to import substitution help the host country's balance of payments position.<sup>52</sup> Yet there are also tangible and intangible costs to the host country. Some studies show, for example, that over the long run, the flow of capital tends to reverse itself; remitted profits, royalties, and management fees become greater than the amount of new capital flowing into the host country. In the period 1950–1965, remittances of Latin American subsidiaries of American parent companies exceeded new private investment by \$7.5 billion, and for the period 1965–1969 the gap was an additional \$3 billion.<sup>53</sup> When we consider that many subsidiaries of multinational corporations provide their own managerial talent, that they import components from parent companies (rather than purchase them locally), and that in many ways they reduce the possibilities for local firms to survive,<sup>54</sup> the benefits accruing to the host country begin to pale. Even employment benefits may be much less than is often assumed. Capital-intensive projects simply do not provide the opportunities for employment that, let us say, investment in agriculture would.<sup>55</sup>

Moreover, most foreign-owned manufacturing enterprises in underdeveloped countries must work assiduously to shape Western-style consumption habits among people becoming part of the market economy. Yet it is precisely these habits which are inimical to many preferred development strategies. People are encouraged to acquire tastes which, while perhaps meeting prestige needs, make little contribution to the general welfare or to community development. As Illich has pointed out, every automobile manufactured and sold in Latin America implies that more rational mass-transportation policies are less likely to be developed. As cars are purchased, national resources will have to be (and have been) devoted to the construction of mass highway systems, benefiting primarily the modern urban middle class. Correspondingly less will be spent, we can assume, on high capacity systems of urban and rural transportation. Every private refrigerator

<sup>52</sup> Raymond Vernon, *Sovereignty at Bay: The Multinational Spread of U.S. Enterprises* (New York: Basic Books, 1971).

<sup>53</sup> Peter B. Evans, "National Autonomy and Economic Development: Critical Perspectives on Multinational Corporations in Underdeveloped Countries," *International Organization*, 25 (Summer, 1971), 678.

<sup>54</sup> For a discussion of some of the negative consequences of American firms operating in the Canadian economy, see Kari Levitt, *Silent Surrender: The Multinational Corporation in Canada* (Toronto: Macmillan, 1970). See also Fouad Ajami, "Corporate Giants: Some Global Social Costs," *International Studies Quarterly*, 16 (December, 1972), 511–529.

<sup>55</sup> *World Bank Operations*, p. 418.

<sup>50</sup> Other forms of protest that have been linked to the disruptive effects of modernization are social banditry and some guerrilla movements. See Eric J. Hobsbawm, *Primitive Rebels: Studies in Archaic Forms of Social Movements in the Nineteenth and Twentieth Centuries* (Manchester: Manchester University Press, 1959).

<sup>51</sup> Richard N. Cooper, "Economic Interdependence and Foreign Policy in the Seventies," *World Politics*, 24 (January, 1972), 172–173.

sold makes it less likely that villages will want to organize community freezers.<sup>56</sup>

The presence of foreign-owned firms whose existence and expansion depend upon fostering Western-style consumption patterns can seriously limit the ability of a government to shape development strategies with a maximum social impact. The pattern of saving and the pattern of consumption, to name only two aspects of economic behavior, can be significantly distorted by the advertising and blandishments of foreign manufacturers and their local emulators.<sup>57</sup> Yet most observers predict a dramatic rise in the numbers and size of multinational firms within the next several decades. The majority of contemporary multinational enterprises focus their productive and marketing activities in the industrialized countries. But in the future they can be expected to seek many new ventures in the Third World. The evidence about their impact on underdeveloped societies is by no means entirely negative, but the traditional view that their activities bring substantial benefits, with no attending costs to host countries, has come under increasing attack.<sup>58</sup> Given the increasing awareness of the negative consequences of their activities, it would not be rash to predict that some governments will adopt more stringent and, in some cases, exclusive policies against Western economic interests.

The growth of tourism by Westerners in underdeveloped countries has had a number of consequences not originally anticipated by those who see travel and personal interaction as important sources of building "international understanding." While American- and European-built and -owned tourist facilities provide some local employment, taxes, and beneficial effects to the host

country's balance of payments,<sup>59</sup> they also tend to create ghettos for the rich, places of amusement which display ostentatious wealth, and which alienate prime land. The behavior of many tourists is no less conducive to bad impressions and to social tensions: at its worst, it can be vulgar and racist; at its best it tends toward the paternalistic. According to L. E. Braithwaite<sup>60</sup> the situation in the Caribbean is already dangerous: the peculiar characteristics of North American tourists simply breed resentment and antagonisms. Pollution, the rapid growth of prostitution and many other menial "service" trades to comfort the northerners, and alarming increases in delinquency involving tourists as victims are all byproducts of this developing industry. While one would not want to argue in all cases that the disadvantages outweigh the costs, this aspect of "interdependence" may also come to face limitations and restrictions imposed by some governments in the underdeveloped countries.

Finally, in a few countries, receptivity to bilateral aid programs is diminishing. The main objective of the American aid program in Brazil has been to promote a good "climate" for American investment. One byproduct of this objective is that today the more dynamic American firms dominate most of the modern parts of Brazilian industry. Both Brazilian intellectuals and government officials are becoming increasingly suspicious of the consequences of American aid.<sup>61</sup> No government is likely to look with favor upon a situation where important sectors of their country's economy become dominated by foreign interests. To the extent that foreign aid programs are used as a vehicle to promote private investment, we could expect some diminution of enthusiasm to participate in them. Moreover, there is some evidence that economic aid does not promote economic growth and, indeed, in the case of Latin America, that aid may retard growth.<sup>62</sup> Trade and private investment rates between the United States and Latin America, according to another study, also correlate negatively with economic development: the greater the economic depen-

<sup>56</sup> Ivan Illich, "Outwitting the 'Developed' Countries," *New York Review of Books*, 13 (November 6, 1969), 20. For a more general discussion of the negative cultural consequences of multinational firms operating in underdeveloped countries, see David Osterberg and Fouad Ajami, "The Multinational Corporation: Expanding the Frontiers of World Politics," *Journal of Conflict Resolution*, 25 (December, 1971), especially 461-468. Concerned with the problem of distorted consumption, the government of Tanzania has prohibited the importation of automobiles for private use. See Woodhouse, "Re-visioning the Future of the Third World," p. 28. In February 1973, Tanzania began prohibitions against effectuation of certain Western dress habits, such as long hair for males and short skirts for females.

<sup>57</sup> For a discussion of the negative cultural impact of American television advertising in Latin America, see Jack N. Barkenbus, "Communication and Cultural Change: Experience in Latin America," paper presented to the meeting of the International Studies Association, New York, March 1973.

<sup>58</sup> Disenchantment among some non-Marxist economists with many of the liberal solutions to underdevelopment is discussed briefly in Cohen, *The Question of Imperialism*, pp. 169, 218.

<sup>59</sup> The net foreign exchange earnings brought by tourism vary greatly from country to country. But in many of the underdeveloped countries, the foreign-owned tourist industry must bring in management, building supplies, and many operating items, including food and beverage. In these countries net foreign exchange earnings are usually from 45 per cent to 60 per cent of gross receipts.

<sup>60</sup> "Race Relations and Industrialization in the Caribbean," in *Industrialization and Race Relations*, ed. Hunter, pp. 39-45.

<sup>61</sup> Samuel P. Huntington, "Foreign Aid for What and for Whom?" *Foreign Policy*, No. 1 (Winter, 1970-71), 186-188.

<sup>62</sup> Keith Griffin, *Underdevelopment in Spanish America* (London: Allen and Unwin, 1969), p. 124.

dency of South American countries, the lower their growth rates.<sup>63</sup>

Bits and pieces of evidence such as those listed above do not constitute in any sense verification of a hypothesis. They do suggest that many of the traditional liberal assumptions about the development of a world order through increased interdependence are being questioned.

The responses of the developed countries to autarchic or isolationist policies are difficult to predict, but it is tempting to assume that in the short run they will be more hostile than indifferent or supportive. Governments of developed states have usually retaliated in instances where important economic interests were expelled, nationalized, or even restricted in their activities. Moreover, the economic charitableness typical of the great powers in the postwar years may be waning as concentration on economic problems and safeguarding economic interests replaces fixation on the Cold War and issues of national security conceived in military terms. Underdeveloped states which undertake to restrict investment opportunities or the marketing activities of multinational firms thus may invoke not only the attention of Western governments and populations, but hostile responses as well. (Of course when a relatively noninvolved state such as Burma acts individually, few international repercussions will ensue.) But if the scenario described in the paper involves a reasonably large number of states within a fairly short period of time, the consequences to the interests of industrialized countries might be much more severe.

An alternative response, perhaps more likely in the long run, is indifference. The governments of many industrialized states, deeply concerned about social trends in their own countries, may understand that all of the orthodox solutions to the problem of underdevelopment have to be re-examined. In the meantime, aid policies would be reformulated by both donors and hosts to concentrate more on social development, agriculture, and population control and less on heavy industrialization and activities which bring advantages pri-

marily to the modernized urban middle classes.

Whatever the hypothetical responses, there is little in the present literature on international conflict to suggest probabilities. We know a great deal about decision-making behavior in crises, the relationship between national attributes and conflict behavior, and the actions and reactions of governments in an arms race. Our conflict studies are based for the most part on the interaction of parties who are locked into a system involving high densities of verbal and symbolic communication. We know a great deal less about isolationist impulses, about situations where one state is attempting to break away from dependence upon others, or in other ways to decrease the degree of integration with the outside world.

Our visions of future international systems generally assume greater transaction flows, an increase in nongovernmental associations, and a burgeoning "international culture."<sup>64</sup> But before we can accept these models as possible norms for the future, we should ask many empirical and normative questions: How are the benefits of increased transactions being distributed? What are the sources of values in the "international culture?" Is the international system tending toward cultural and material homogeneity or toward acceptance and promotion of pluralism? Is homogeneity of life-styles, economics, and views of man and human activity something to be promoted or deplored? Is increased international economic integration or interdependence consistent with the preservation of the most meaningful parts of indigenous cultures? Are the networks of communication and economic activity that surround the globe capable of promoting and sustaining two-way movement, or have the networks developed in such a manner as to assure the predominance of Western and socialist states over the Third World? These and many other questions have not yet been raised by systems theorists of international relations or by those who are concerned with development problems. If the scenario in this paper has validity, then questions such as these should begin to command our attention.

<sup>63</sup> Lawrence R. Alschuler, "Satellization and Stagnation in Latin America," paper presented at the European Consortium for Political Research, Mannheim, West Germany, April 1973.

<sup>64</sup> For example, John Burton, *Systems, States, Diplomacy, and Rules* (London: Cambridge University Press, 1968); George Modelski, *Principles of World Politics* (New York: The Free Press, 1972).

# The Primary Goals of Political Parties: A Clarification of Positive Theory

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Positive theorists, notably Anthony Downs, William Riker, and their followers, have made a major contribution to the theory of political parties.<sup>1</sup> Apart from the validity of their specific conclusions, a signal virtue is their method which requires clarity and precision about the fuzziest aspects of political parties. Nothing is more obscure, for example, particularly with respect to American parties, than who is included within the party. Most writers have treated the concept of membership with considerable flexibility, expanding or contracting the concept to suit their immediate purpose.<sup>2</sup> But the positive theorist has been more exact. Viewing the party as a calculating instrument, he explicitly decides on who is doing the calculating. Thus, whereas most analysts of political parties at one time or another, for one purpose or another, include voters within the party, Anthony Downs never does. He consistently views parties as "teams" of office-seekers.<sup>3</sup>

Or take the thorny problem of party goals. Parties attract a great variety of people: opportunists, power brokers, idealists, ideologues, patronage seekers, dilettantes, each with his own goals. The thorough descriptive analysis reflects this complexity.<sup>4</sup> It thereby provides convenient alternative explanations of a party's past behavior.

<sup>1</sup> I use the term "positive theory" following the example of William H. Riker and Peter C. Ordeshook, who have synthesized much of this literature in their book, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973).

<sup>2</sup> A thorough review of the varying scholarly meanings for the term "party" is presented in Austin Ranney's "The Concept of 'Party,'" in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge: Harvard University Press, 1968), pp. 143-162. The solution of V. O. Key, Jr., who probably ranks as the most influential single writer on American parties, to the problem of membership was to speak of the "party-in-the electorate," "party-in-government," and "party of organized workers"; see *Politics, Parties and Pressure Groups*, 5th ed. (New York: Crowell, 1964), pp. 163-165. An alternative solution grades members or participants according to degree of activism from leaders to occasional supporters. See Austin Ranney and Willmoore Kendall, *Democracy and the American Party System* (New York: Harcourt, Brace and Company, 1956), pp. 199-212.

<sup>3</sup> Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), pp. 25-26.

<sup>4</sup> Many studies attest to the complexity of attitudes of participants in parties. One of the most thorough is Samuel Eldersveld's *Political Parties, A Behavioral Analysis* (Chicago: Rand McNally, 1964).

Traditional descriptive analysis arrives at an impasse, however, in trying to predict the direction of party activity. The positive theorist, on the other hand, acknowledges that a multiplicity of goals provides too many variables. Typically he isolates one goal (for Riker and Downs it is "winning") and subsumes or ignores the alternatives. Such simplification makes feasible speculation about a party's future behavior. Because his goals are held only by individuals, the positive theorist also avoids the sin of reification, common in party literature, or the imputation of goals to a collectivity such as a political party.

In effect the positive theorist allows us to hold constant a complex reality, particularly those difficult aspects whose definitions depend on the observer. By being explicit and restrictive about such problems as party membership and party goals, positive theorists allow us the hope that we can accumulate knowledge about these problems, knowledge which is lacking despite the prodigious efforts of many competent scholars. It is not accidental that those aspects of party behavior about which we have been able to accumulate knowledge are precisely those which do not depend upon each observer's definition. In the last thirty years we have made considerable progress in our understanding of voting behavior. This has been possible because, despite differences in theories and techniques, no one argues about what a vote or a voter is. There now exists, therefore, an accumulation of data on voting behavior which can reliably serve theoretical speculation. But where is a similar body of data on the boundaries of the Republican and Democratic parties or on their goals? Perceptive writers such as Key, Chambers, Epstein, Sorauf, and Burnham<sup>5</sup> have discussed such matters in important ways, but the cumulative results of their writings on parties are nothing like the advances made in our knowledge of voting. Positive theorists, not only because they must be explicit, but also because they consciously seek to

<sup>5</sup> Key, *Politics, Parties and Pressure Groups*, chap. 8, pp. 199-227; William N. Chambers, *Political Parties in a New Nation* (New York: Oxford University Press, 1963), pp. 45-51; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton and Company, 1970), pp. 91-134; Frank J. Sorauf, *Party Politics in America*, 2nd ed. (Boston: Little, Brown and Company, 1972), pp. 7-27; Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger Publishers, 1967), pp. 3-18.



build upon each other's work, have opened the way for the accumulation of data and conclusions about the vexing problems central to the study of political parties.

My purpose in this paper is not to praise positive theorists. It is, rather, to demonstrate that their works, which deal almost exclusively with strategies, also can provide us with insights into the broader problem of party organization. In order to do this, however, we must first recognize that in their efforts to build a cumulative theory positive theorists have obscured vital differences in the types of parties they were examining. I shall start with the basic works of Riker and Downs, demonstrating that although the former sought to build upon the latter's theory, both were talking about different goals and different boundaries and therefore different types of party organization.<sup>6</sup> I shall also argue that in the discrepancies between them we can find a satisfactory explanation of significant tensions inherent in all democratic parties.

The broad outline of Downs's and Riker's theories is quite familiar. Downs's book, published in 1957, offers a model of the democratic system in which the key element is competition between rational parties for the votes of equally rational citizens. He is clear in defining a political party as "... a team of men seeking to control the governing apparatus by gaining office in a duly constituted election."<sup>7</sup> All members of the team share the same goal which "... is to reap the rewards of holding office *per se*."<sup>8</sup> Riker's theory, appearing in 1962, is less explicitly a theory of parties than it is of coalitions, of which he considers parties to be an important subspecies. The nub of his theory is the "size principle," or the proposition that under certain conditions "participants create coalitions just as large as they believe will ensure winning and no larger."<sup>9</sup> Proofs of the theorems of each theorist rest upon specific limiting conditions, such as perfect information, not likely to be found in the real world. Nevertheless, both Downs and Riker imply that their theories should be predictive. Thus Riker analyzes specific instances in American political history when one party came close to monopolizing the vote, as in 1820 or 1872, and argues that the party then acted in accordance with the size principle to approximate the minimal winning coalition. At no point does Riker state explicitly what he means by a political party or even what the goal of a party is, other than "winning." But since he discusses

Downs's party and applies his own principles to it, we assume that he believes they are talking about the same thing.

There is, however, an obvious point at issue between them: the size of the electoral margin which the party seeks. Downs states it as axiomatic that a party seeks to maximize its vote. Riker's size principle, on the other hand, leads him to conclude that a party seeks to win by the smallest margin possible, or to achieve the "minimal winning coalition." Riker argues that had Downs adopted the minimal winning strategy he would have avoided his crisis in rationality. The rationality crisis in Downs's theory emerges because parties, in their effort to gather as many votes as possible, become ambiguous in their policies, thus making it impossible for voters to behave rationally. What is rational for the party leads to irrationality in the voter. Riker points out that if parties properly follow the minimal winning strategy they no longer need to be ambiguous. Thus the rationality crisis is resolved.<sup>10</sup>

For some time Riker's criticism of Downs appeared reasonable to me, and, so far I can discern, to others for whom this was a matter of interest. After all, the proposition that parties try to win by as narrow a margin as possible had been made very effectively by E. E. Schattschneider twenty years earlier in his enormously influential book, *Party Government*.<sup>11</sup> To those of us who prefer to see politics as the interaction of rational beings it was a relief to see any rationality crisis resolved.<sup>12</sup> Yet in doing so we overlooked the possibility that Riker and Downs were not really talking about the same thing; that what each meant by "party" was different and that from these differences there followed quite distinct yet equally rational strategies.

My own realization that Riker's size principle

<sup>10</sup> *Ibid.*, pp. 98-101.

<sup>11</sup> E. E. Schattschneider clearly anticipated the reasoning of positive theorists, including Downs and Riker. Anticipating the concept of the minimal winning coalition, he wrote that a party would not want to win 100 per cent of the vote because "it is unnecessary and wasteful. Fifty-one per cent of the vote will give any party all there is of the power to govern. . . . From the point of view of the interests participating in the political venture, it is more profitable to share a victory with a narrow majority than it is to partake of the spoils of victory with a larger number, for the smaller the number of participants the greater will be the share of each . . . the perfect party victory is to be won by accumulating a relatively narrow majority, the mark of the skillful conduct of politics." *Party Government* (New York: Holt, Rinehart and Winston, 1942), pp. 95-96.

<sup>12</sup> Kenneth A. Shepsle, "The Strategy of Ambiguity: Uncertainty and Electoral Competition," *The American Political Science Review*, 66 (June, 1972), 555-568, presents a different resolution to Downs's rationality crisis.

<sup>6</sup> William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962) and Anthony Downs, *Economic Theory of Democracy*.

<sup>7</sup> Downs, p. 25.

<sup>8</sup> *Ibid.*, p. 28.

<sup>9</sup> Riker, pp. 32-33.

was not a general theoretical principle applicable to all parties came from a practical experience which is worth recounting briefly. In the fall of 1971 I served as campaign manager for a candidate running for city council. The candidate was an incumbent seeking re-election in a difficult situation. The extension of the vote to eighteen-year-olds had more than doubled the electorate in a university town. A well-organized, radical anti-incumbent slate was fully prepared to use the untested youth vote to oust my sixty-year old incumbent, despite his liberal credentials. As a liberal councilman, however, he had antagonized many of the older town voters who were supporting a conservative slate. As the middleman between two opposing forces, our candidate was able to squeak through by the narrow margin of forty votes out of some 14,000 cast.

In the postelection euphoria we naturally expounded on the brilliance of our strategy. Any more effort, one more sign, one more mailing, or one more promise would have been wasted; any less effort would have spelled defeat. Although the brilliance of the strategy was evident only in retrospect, the fact remains that had we known what we were doing (i.e., enjoyed Riker's condition of perfect information), we would have followed precisely the same strategy; that is, if we accept the logic of the minimal winning coalition.

Yet a strange thing began to occur which our reading of Riker had not led us to expect. When talking to campaign workers and voters it soon became apparent that every one of those voters was one of the last forty and that each worker had brought in the last forty votes. Each appeared to feel, therefore, that he had a special claim. What had happened, clearly enough, was that by achieving a minimum winning coalition we had maximized the number of marginal voters. And marginal voters, precisely because their votes are the ones needed to win, have a greater claim on the officeholder. Thus the minimal winning coalition, rather than reducing the number of payoffs which the officeholder had to make, actually appeared to heighten the expectations of payoffs. Perhaps, after all, Downs really was right and we should have maximized the vote.

Upon closer examination, it seemed to me that both Riker and Downs were right, depending on whether one considered the primary interests of the candidate or of the voter. The strategy of the minimum winning coalition certainly made sense for the voters. By reducing their numbers to the minimum needed to win, they could maximize their claims on the successful candidate. But for the same reason, the candidate was well advised to maximize the number of his voters, if only to weaken the claims that any one voter could make

upon him. There was then a conflict of strategies.<sup>13</sup> From that conflict Riker emerged as the theorist of the voters, Downs the theorist of the candidate.<sup>14</sup>

To go one step further, if Riker and Downs have provided us with two equally rational but distinct political strategies, does their work not also imply two distinct answers to the problems of party goals and party boundaries? For Downs a party consists only of office-seekers, and its goals are confined to their goals, namely the winning of office. For Riker a party must encompass the voters as well, for they are part of his coalition. Confusion arises because Riker wants his party to be the same as Downs's. He fails to recognize that by admitting voters and their goals to the party, he must change almost all of Downs's assumptions. In particular he must reverse the critical proposition that "... parties formulate policies in order to win elections, rather than win elections in order to formulate policies."<sup>15</sup> Voters find no advantage in winning elections per se; office is a payoff limited strictly to candidates. The party which encompasses the voter must derive its interest in winning elections from the hope that it will thereby be able to implement policies or other nonofficeholding benefits for its members.

It would be unfair not to note here that Riker is quite aware of the distinction between the payoff to leaders and the payoff to followers in electoral coalitions. But in trying to resolve the conflict he resorts to psychoanalytic explanations of the motives of leaders, a most uncharacteristic lapse from his rationalist assumptions. For example, he relates Woodrow Wilson's extraordinary efforts to gain office to his need to overcome feelings of inadequacy instilled by his father. In so doing, Riker arrives at a rationality crisis of his own, for he concludes that "... leaders may pay out more to win than a victory is objectively worth."<sup>16</sup> Greater clarity will be achieved, I believe, if we recognize that we are dealing with two potentially

<sup>13</sup> Even though a minimizing strategy makes sense for voters, they are hardly capable of carrying one out. To pursue such a collective strategy requires organization, a point to which I return below. Candidates, on the other hand, can seek a maximizing strategy, as has been demonstrated by Marjorie R. Hershey, "Incumbency and the Minimum Winning Coalition," *American Journal of Political Science*, 17 (August, 1973), 631-637.

<sup>14</sup> The economist, George J. Stigler, in "Economic Competition and Political Competition," *Public Choice*, 13 (Fall, 1972), 91-106, has sought to resolve the conflict between party objectives found in Downs and Riker. While his argument is interesting, it rests upon the complete elimination of winning office as a party goal, a most apolitical assumption.

<sup>15</sup> Downs, *Economic Theory of Democracy*, p. 28.

<sup>16</sup> Riker, *Theory of Political Coalitions*, p. 209. The discussion of special payments to leaders is in pp. 203-210.

conflicting goals each of which implies not only its own strategy but also its own type of party organization.

### The Two Primary Goals

There are two kinds of people who will seek to participate in political parties to achieve two distinct goals: (a) *office-seekers* whose goal is *office* and (b) *benefit-seekers* who want *benefits* derivable either from the control of office or in some way dependent upon the existence of the party. Downs's theory is about the former, Riker's about the latter. The two goals must not be confused because they lead to distinctive strategies and conflicting ways of organizing parties. Riker has obscured the differences by subsuming both goals under the term "winning." Winning, however, is not a primary goal; it is a means of gaining something else. True, there may be some people for whom politics is like amateur competitive sports; for them there is adequate satisfaction in playing the game in the hope of winning. But I would suspect that for most participants winning in politics means gaining either office or the benefits derivable from the control of office.

It is the inherent value of *office* which is most obscured by Riker's argument. Yet its value is certainly a commonplace assumption. The widely held view that most if not all politicians are "opportunists" rests on the notion that it is the desire for office and its powers that guide officeholders rather than any concern for the content of public policy. Most political systems go to great lengths to give office independent value. Terms of deference such as "your honor," the lifetime use of titles such as "Mr. President" or "Governor," graded seating arrangements at state functions, and the privilege of state funerals all remove those who hold political office from the ordinary and give office holding a value well beyond the material benefits which may accrue to office holders. We watch wealthy men spend vast sums of money in the quest for offices which could not possibly provide them with a commensurate material return. History does not allow us to ignore the power of ambition for office as an incentive in its own right. Ambition is also a distinctly private or individual incentive which such positive theorists as Mancur Olson have taught us is essential to the development and maintenance of any organization as large as a political party.<sup>17</sup> True, only a few people can ever reasonably hope to hold office, but this does not reduce the central importance of the goal of office for the development of political parties.

<sup>17</sup> Mancur Olson, *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965).

By *benefits*, on the other hand, we mean all the nonoffice goals which people bring to politics. Thus benefit-seekers include anyone who hopes to affect government activity. Many benefits sought are private in character: jobs, contracts, or individual favors. Others are collective, such as the national defense or control of atmospheric pollution. Hence we include under benefits policies which provide the most selfish and private of advantages and those which derive from a completely selfless concern for the common welfare. We bring both types of concerns together under the general rubric of benefits because we wish to demonstrate that the strategies of all benefit-seekers have more in common with each other than they do with the strategies of office-seekers.

We know, of course, that a political party is only one means by which people seek political benefits. Commonly we call organizations which seek benefits without seeking elective office interest groups. Such groups may employ a wide range of tactics. When, however, benefit-seekers form a party or enter an existing one, they commit themselves to some use of the tactic of electoral politics. Since winning office or being able to affect someone else's chances of winning is central to electoral politics, benefit-seekers must decide that such a tactic is useful before they enter party politics. American labor unions, for example, debated this issue for many years before deciding during the 1930s that working for the election of Democratic candidates was a suitable strategy.<sup>18</sup>

We should note also that for many benefit-seekers, even those within political parties, winning office is not essential for the achievement of all of their objectives. Merely by entering competitive politics benefit-seekers force the competition to pay attention to their demands. If, in turn, by acceding to these demands the opposition wins the victory, the benefit-seekers, while having lost the office, have gained the concessions at least.

Nevertheless, winning elections, while not essential to benefit-seekers, is a better strategy than not winning. In 1968 George Wallace's American Independent party failed to win the election, but the size of its vote and the threat of its re-emergence in 1972 forced the two major parties to make policy concessions to its supporters. These concessions, however, were only partial. Without doubt, Wallace and his supporters would have gained more of their desired benefits had they won. As for office-seekers, winning elections is

<sup>18</sup> On the debate within the labor movement as well as the impact of direct involvement on the unions' own political flexibility, see J. David Greenstone, *Labor in American Politics* (New York: Alfred A. Knopf, 1969).

essential, the *sine qua non*, which forces them to form or enter political parties. The fact that winning is the mandatory strategy for one goal and only the optimal strategy for the other is an important distinction.<sup>19</sup> But here I wish to emphasize that winning elections is the best strategy for both primary goals.

If winning is the best strategy for both office-seeker and benefit-seeker, is it useful, after all, to distinguish between the two primary goals? Surely benefit-seekers and office-seekers need each other. Office-seekers must be concerned with benefits; otherwise no one would have any reason to work for them. At the same time, as I have pointed out, benefit-seekers realize their maximum gains either by winning office or by supporting those who do. True, one's ends may well be the other's means. But is it possible to disentangle means from ends in the real political world? On the basis of the experience which I recounted earlier I would contend that it is possible because there are distinct types of behavior. In other words, while both office-seekers and benefit-seekers seek to win elections, they do so in different ways. It is in trying to distinguish among such strategies that the theories of Riker and Downs can give us guidance.

First, however, it is important to include a refinement which has already been added to Riker's and Downs's work. As I pointed out earlier, the distinction between Riker and Downs rests on the size of the victory which each writer says a party should seek. But the size of a party's victory is not as simple a matter as it would appear from the early writings of the positive theorists. As Hinich and Ordeshook have pointed out, Downs and others failed to distinguish clearly between maximizing one's vote and maximizing one's plurality.<sup>20</sup> While at first glance, these may appear

to be the same, they in fact can differ considerably if voters abstain. The ability of voters to decide not to participate makes it possible for a party to try to maximize its plurality, or the distance between it and its nearest competitor, while at the same time minimizing its total vote. Or a party might opt for maximizing its vote while minimizing its plurality. In any event, only if all citizens vote can we say that vote maximizing and plurality maximizing are the same thing.

Recent political history provides us with ready evidence that the maximization of a party's plurality and the maximization of its vote do not necessarily go together. In 1960 Richard Nixon barely lost to John Kennedy in the popular vote; Kennedy's plurality margin was only 0.2 per cent. In 1960 Nixon's percentage of the *potential* vote was 31.6 per cent. In 1972 Nixon achieved a landslide plurality over McGovern of 23.2 per cent. Yet because the turnout rate was well below that of 1960, Nixon's percentage of the potential vote had risen only a little over two points to 33.7 per cent.<sup>21</sup>

If we add to the maximizing objectives of parties the refinement of the size principle, we can see four distinct strategies which political parties can follow, depending on how they combine the two components of victory. Two factors make up the size of any victory: (a) the size of the vote received, and (b) the size of the plurality. Logically they are independent of each other. A party can win all of the votes cast and gain a plurality of 100 per cent while winning hardly any of the potential vote, a situation characteristic of many an uncontested election. Any combination of the two variables is at least possible. I have chosen, however, to concentrate upon the extremes in which the parties seek either to maximize or minimize the two variables.

### The Four Strategies

(1) The first strategy requires the party to *minimize its plurality, while minimizing its vote*. This is the benefit-seeker's best strategy. It is identical with Riker's minimum winning coalition, for it seeks to reduce the size of one's support to the bare minimum. The benefit-seeker wishes to minimize the vote or seeks a low turnout election in order to minimize the number of claimants to the

<sup>19</sup> The fact that office-seekers must win while benefit-seekers in parties find winning the election only the best alternative is one reason that the former tend to dominate parties. Lacking an alternative, the office-seeker must either keep pressing his strategy, give up his goal, or move on to another party. The benefit-seeker, because his objectives are likely to be satisfied in some measure, is under less pressure to insist upon his strategy. He may even be able to withdraw from direct partisan activity and remain a benefit-seeker, something an office-seeker cannot do with respect to his goal.

In addition, the office-seeker and everyone else know when his goals have been achieved. The achievement of benefits, however, is not always so clear. The fact that one goal—office—is simple, observable, and easily measured in its achievement, while the other—benefits—is usually ambiguous and always subject to argument over its achievement reinforces the tendency for the office-seeking goal to dominate political parties, as well as the attitudes of candidates themselves.

<sup>20</sup> Melvin J. Hinich and Peter C. Ordeshook, "Plurality Maximization vs. Vote Maximization. A Spatial

Analysis with Variable Participation," *The American Political Science Review*, 64 (September, 1970), 772-791.

<sup>21</sup> One positive theorist has addressed the problem of relating turnout to plurality. See Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in *Mathematical Applications in Political Science II*, ed. Joseph L. Bernd (Dallas: Arnold Foundation, Southern Methodist University Press, 1966), pp. 137-160.

spoils. At the same time, the benefit-seeker wants to minimize the plurality of the victory for the reason I pointed out earlier: the narrower the victory, the greater the claim of each supporter upon the successful candidate. Reducing the number of voters reduces the number of people among whom the benefits must be shared; reducing the plurality strengthens the hold of each supporter upon the officeholder. Again we must emphasize that the argument holds, regardless of the type of benefit being sought. Patronage seekers hanging around the party clubhouse are chary of newcomers. Ideologues watch their candidate closely for signs of a broadening appeal, or in their words, "selling out." Both recognize the value of minimizing support in order to maximize their benefits.<sup>22</sup>

(2) The second strategy consists of *maximizing both the number of votes and the plurality*. It is the most advantageous method of winning for the office-seeker. Large victories enhance the inherent values of office. The leader of a coalition maximizes his independence from any individual or group. If the winner using the minimizing strategy emerges as a delegate, the winner who maximizes his victory comes forth as the trustee, the true leader. Yet it is more than the value of a particular victory which leads an office-seeker to use a maximizing strategy. It also improves his chances at continuing or advancing in office. The freedom he gains from past supporters permits him to maneuver for advantage in future elections. Since a political career is based on the continuous search for office, office-seekers must be future oriented, concerned with being right in future elections and therefore relatively free of the burdens imposed by elections in the past. Thus a politician who has successfully maximized his victory can more readily shift his stance as the flow of political issues requires it.

Maximum victories also serve office-seekers as a kind of political currency. The political community—including party officials, newspaper columnists, and financial contributors as well as other office-seekers—is visibly impressed by large victories. Politicians who get themselves talked about as potential presidents, governors, or senators are usually those who have been able to demonstrate broad popular support. Political parties which bring together the office-holding ambitions of many politicians are thus drawn even more

than individual candidates to the maximizing strategy.<sup>23</sup>

(3) The remaining strategies are mixed strategies or compromises. One consists of *maximizing one's plurality while minimizing the vote*. If he must compromise, this is probably the compromise the office-seeker would prefer. Davis, Hinich, and Ordeshook present plurality maximization as the best strategy for candidates.<sup>24</sup> My argument is that they are also served by maximizing the vote. But if an office-seeker cannot have both dimensions of victory it is likely that he would sacrifice the size of the vote. My guess, for example, is that John Kennedy would have preferred to win as Nixon did in 1972 rather than as he himself did in 1960, that is, by a large plurality in a low-turnout election rather than by a narrow plurality in a high-turnout election. Narrow pluralities not only reduce the maneuverability of the winner, they pose a threat to winning the office itself. Of course under conditions of perfect information the threat would not exist; but given the normal conditions of uncertainty we can expect the reasonable office-seeker to worry first about his plurality. Furthermore, by not seeking to expand the vote, the plurality-maximizing office-seeker can assuage his benefit-seeking supporters who, as we have pointed out, are not interested in seeing their claims diluted any more than necessary.

The Nixon campaign of 1972 is an example of what appears to have been a plurality-maximizing, vote-minimizing strategy. Nixon's large plurality was produced by sizable shifts of traditionally Democratic voters, southern whites, blue-collar workers, and ethnic minorities, all of whom were disaffected by the direction which the Democratic party had taken since 1964. Nixon, however, made few appeals to new voters, particularly to those who characteristically turn out in low numbers: blacks, poor whites, and the young. Indeed, Nixon himself did hardly any campaigning at all. In the southern states, where he was to register enormous pluralities, he followed for the most part a strategy of noninvolvement in lesser races. A brisk campaign would undoubtedly have relieved much of the tedium many observers noted among the electorate and would surely have produced a larger turnout. In retrospect, at least, it was a strategy well calculated to maximize Nixon's plurality, while keeping the overall vote low.

It is important at this point to recognize that the two elements, plurality and size of vote, are

<sup>22</sup> The proposition that benefit-seekers want a minimal victory rests on the assumption that control of the office can produce the benefit. The American system, combining separation of powers with bicameralism and federalism, means that control of one element does not necessarily produce the desired benefits. The need, therefore, to control more offices may lead the rational benefit-seeker to adopt a more than minimum winning strategy.

<sup>23</sup> On political parties as coalitions of ambitious office-seekers, see Joseph A. Schlesinger, *Ambition and Politics* (Chicago: Rand McNally, 1966).

<sup>24</sup> Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *The American Political Science Review*, 64 (June, 1970), 438.

independent of each other only in a technical sense. A candidate such as Richard Nixon might reasonably conclude that his chances for a sizable plurality rested on a low turnout of voters. John Kennedy in 1960, on the other hand, explicitly waged a get-out-the-vote campaign because he reasoned that this would enlarge his plurality. Depending on the reasons for abstention, we know that variations in turnout do not affect each party equally. Thus depending upon the candidate and the issue, each party must decide what combination of strategies it should follow. This in no way vitiates our central point, however, that the preferred strategy of the benefit-seeker is to minimize both turnout and plurality, that of the office-seeker to maximize both.

(4) The second compromise strategy, *minimizing one's plurality while maximizing the vote*, fits the needs of benefit-seekers more than of office-seekers. The latter would find much of the value of the large vote wiped out by the small plurality. A large vote alone would mean that the number of marginal voters had been maximized, placing an office-holder in the weakest position. The type of benefit-seekers, however, who might find this to be a desirable compromise are those who have a commitment to maximum voter participation, groups such as the League of Women Voters, many political scientists, and advocates of greater participatory democracy.

The two primary goals of political organization, as well as the two types of people we have described as participants in party politics, are, of course, abstractions. For that reason the strategies of compromise may indeed be the more realistic. Our ambitious office-seeker is not likely to be concerned solely with office. Inevitably he is a citizen concerned with the policies of government. To that degree, he too is a benefit-seeker. As office-seeker he wants to free himself through maximizing his victories. Only a sizable victory frees him to pursue any policy and join any coalition in his quest for advancement in office. As a benefit-seeker, however, he may well wish to reduce his maneuverability; thus he is drawn to a minimizing strategy. Like the southern congressman who could do pretty much as he wished as long as he voted the southern position on racial legislation, the benefit-seeker in office wants to be held fast by his supporters to those policies which will produce the benefits he wants too. The claims of his supporters provide him with the best defense against the vote-trading demands of other legislators.<sup>25</sup> And yet, the office-seeker within him

wants to be able to make those trades when they can help further his career.

In the real political world, therefore, both the individual and the party are inevitably subject to the pressures of both goals. The tensions within individuals and parties between the goals of office and benefits, as well as the confusion over whether offices and benefits are ends or means, make it nearly impossible to determine which goal is dominant through observation. Yet in practice we do it all the time, because we discern differences in strategies. On what basis, for example, do most observers conclude that the major American parties are predominantly concerned with winning office? I submit that this conclusion rests far less on any overt confession in party platforms or in the public utterances of candidates, than in our observation that the parties follow vote- and plurality-maximizing strategies, which make sense only if the quest for office is uppermost. We conclude, on the other hand, that some candidates are primarily ideological or oriented toward benefits because they take actions which appear exclusionary. That is, our test of ideological or issue orientation is a candidate's willingness to adopt what we see as a minimizing strategy. Thus we watched Eugene McCarthy cling to his "smarter" voters, Barry Goldwater drive away all but his preferred "extremists," and George McGovern's supporters reject as unacceptable practitioners of the "new politics" large segments of the traditional Democratic coalition. While we can never know with certainty the motives of politicians, our assumptions about their motives rest upon the strategies they employ.

The distinction among different strategies of electoral competition has certainly not been ignored by positive theorists. We are indebted to Hinich and Ordeshook<sup>26</sup> for their clarification of the consequences of vote- and plurality-maximization. Hinich and Ordeshook treat these strategies, however, as if these were the primary goals of candidates and parties rather than their means. The authors do so because they are concerned with the fact of winning, without asking what it is that the candidate wins. As I have sought to demonstrate, these are partial strategies which may be combined in various ways with vote- and plurality-minimizing as well. A complete theory of electoral competition has to take these alternatives into account also.<sup>27</sup>

<sup>26</sup> Hinich and Ordeshook, pp. 772-791.

<sup>25</sup> The benefit-seeking officeholder's problem is not unlike that analyzed by Thomas C. Schelling in which a bargainer increases his strength by proving his inability to change his position. *The Strategy of Conflict* (New York: Oxford University Press, 1962), pp. 22-28.

<sup>27</sup> Positive theorists have examined possible goals for parties and candidates other than plurality and vote maximization. Riker and Ordeshook, "Plurality Maximization vs. Vote Maximization," p. 336, mention briefly such goals as maximizing the proportion of the vote, the probability of winning, and the probability of securing a fixed percentage of the vote. None of these, however, is identical with the minimal winning coalition.

The failure of the positive theorists to probe further and to define the goals of parties as office and benefits accounts for their inability to integrate the principle of the minimum winning coalition into their theory of electoral competition. There can be no doubt that Riker means his size principle to be a statement about the objectives of candidates and parties. In the work, however, in which he and Ordeshook seek to synthesize positive political theory, the size principle and the discussion of electoral competition are handled as though they were entirely unrelated.<sup>28</sup> If parties do seek to attain a minimum winning coalition, as they argue in chapter 7, then one might expect the coalition to play a major part in their discussion of candidates' objectives which appears in chapter 11. Instead it is relegated to a footnote which asserts that under certain conditions the minimum winning coalition leads to results identical to those of plurality maximization.<sup>29</sup> While it is true that the results can be identical, Riker and Ordeshook fail to point out that a candidate who is trying to maximize his plurality can hardly be trying to minimize his coalition. In order to incorporate the principle of the minimum coalition into competitive electoral theory we must recognize that parties and candidates can follow both minimizing and maximizing strategies.<sup>30</sup>

<sup>28</sup> Their discussion of the size principle is found on pp. 176-201, assumptions about the objectives of candidates on pp. 333-337, in *Introduction to Positive Political Theory*.

<sup>29</sup> *Ibid.*, p. 335, fn. 33.

<sup>30</sup> Positive theorists have examined the possibility that candidates and parties are subject to internal restraints, i.e., their ability to respond to electoral competition may be affected by the need to respond to activists either in obtaining nominations or campaign resources. Riker and Ordeshook, raise the problem briefly on pp. 361-362. It is also raised in Davis, Hinich, and Ordeshook, "An Expository Development," pp. 426-448 and in James S. Coleman, "Internal Processes Governing Party Positions in Elections," *Public Choice*, 11 (Fall, 1971), 35-60. The argument of these articles is quite distinct from the one I am making since they treat activists as people who have *preferences*, while only candidates or parties have *strategies*. Thus their analysis is similar to James Q. Wilson's distinction between amateurs and professionals. My contention is that the activist is capable of strategic judgments; a primary voter can be concerned with choosing the candidate likely to win in the general election as much as with choosing one who comes closest to his preferences.

Two articles which do treat the activist as a potential strategist are Peter H. Aranson and Peter C. Ordeshook, "Spatial Strategies for Sequential Election," in *Probability Models of Collective Decision-Making*, ed. Richard G. Niemi and Herbert F. Weisberg (Columbus: Charles E. Merrill, 1972), pp. 298-331, and James S. Coleman, "The Positions of Political Parties in Elections," in Niemi and Weisberg, pp. 332-357. These do not, however, develop my point that the activist as benefit-seeker attempts to win with a strategy different from that of the office-seeker.

It is one thing, of course, to develop a theory of strategies and another to perceive differences in party behavior in the face of competition. In a two-party system where both parties accept winning as the optimal mean of achieving their goals, under conditions of uncertainty about what will influence the outcome, it probably makes little difference whether the goal is office or benefits. Competition forces both office- and benefit-seekers to woo a similar electorate; uncertainty forces them to broaden their appeals. As a result anyone's conclusions about "real" goals is subject to dispute. I wish only to point out that conclusions about goals are drawn not from direct knowledge about a candidate's motives, but rather from what we perceive to be his strategy.

### The Implication of the Two Primary Goals for Party Organization

The importance of the distinction between the goals of office and benefits rests, therefore, not in its usefulness in predicting how parties will behave but in determining how parties are organized. It is precisely because positive theorists have failed to distinguish between the two goals that their impact upon the study of political parties has been limited. Just as Riker believed that his party was the same as Downs's, students of parties have generally concluded that positive, or rational calculating theories apply only to office-seeking parties. For example, William Wright has classified parties and party theorists along a continuum ranging, in his words, from "rational-efficient" to "party democracy."<sup>31</sup> At one end of the continuum he places the major American parties and theorists such as Downs; at the other end of the continuum are the European Socialist parties and such theorists as Duverger. Clearly Wright assumes that only office-seekers are either capable of facing or willing to face the electorate in a rational, calculating manner. Were he to drop that assumption, his own continuum would become more reasonable, for the distinctions between parties would be reduced to the distinction between parties organized to gain office and those organized to maximize benefits.

The assumption that only office-seekers are rational calculators is, however, a bias which runs deeply through the study of politics. For example, James Q. Wilson's influential distinction between amateurs and professionals rests on the notion that only the latter are capable of treating issues and votes in a detached manner.<sup>32</sup> Interested in winning office, they can compromise and bargain. The amateur, on the other hand, holds policies as

<sup>31</sup> William E. Wright, *A Comparative Study of Party Organization* (Columbus: Charles E. Merrill, 1971), pp. 17-54.

<sup>32</sup> James Q. Wilson, *The Amateur Democrat* (Chicago: The University of Chicago Press, 1962).

pre-eminent. He feels that decisions should always be based on principle and thus is driven toward extremist positions. The difference is one of style and personality: the professional is the shrewd, rational, calculating politician, the amateur, the hot-blooded, do-or-die man of principle.

There is no need, however, to reject rationalist assumptions in interpreting the behavior of policy-oriented participants. No *a priori* reason exists for assuming them to be less capable than office-seekers of treating issues and voters in a detached, calculating manner. Indeed, Robert Putnam, in his careful study of British and Italian parliamentarians, found no barrier at all between ideological politicians and willingness to compromise.<sup>33</sup> We must recognize, however, that each goal implies its own strategy. Behavior that is rational for an office-seeker can be irrational for a benefit-seeker. We must, therefore, be careful not to confuse their distinct objectives under the rubric of "winning."

Since winning an election means something quite different to the office-seeker than it does to the benefit-seeker, each goal requires a distinct approach to party organization.<sup>34</sup> The difference in organizational effort made by the benefit-seeker and the office-seeker derives primarily from the different value which winning has for each. For office-seekers, winning provides immediate satisfaction. The state according to its rules hands over the prize. The benefit-seeker, however, must await the decisions of the officeholder. Thus the benefit-seeker needs some mechanism to assert his claim. Indeed, before he can make any claim at all, he must be able to prove his support. This is not as easy as it might appear under a voting arrangement with secret ballots. Indeed, one of the principal arguments for the secret ballot is that it makes it difficult for some types of benefit-seekers to prove their support and be paid for their vote. What good, then, is it for voters to forge a minimal winning coalition if there is no way they can prove to the successful candidate that they indeed supported him? For all he knows they might have voted for his opponent.<sup>35</sup> Benefit-seekers thus

need proof that they did support the candidate.

There are, of course, ways in which benefit-seekers can prove their support. Giving money has been institutionalized in the United States to the extent that the office-seeker must officially report each contribution, thereby giving legal assurance to benefit-seekers that their support will be recorded. Working for a candidate is another way, and only a foolish benefit-seeker would do so anonymously. Interest groups such as labor unions are careful to identify their support and even devise ways to keep their activities distinct so that their contribution to the coalition is clear. All of these methods are available to individuals and groups willing to give up the anonymity of the secret ballot. For the ordinary voter, however, there is only one way to exert claims through a party, and that is to become an explicit party member.

Hence we arrive at the rationale for the membership party. It rests on the proposition that a party is, or should be, concerned with distributing benefits. The people for whom the party seeks benefits, therefore, must be identifiable. Voters must be turned into members. Only in this way can the minimizing strategy become effective. Thus ideological movements which become political parties must carefully define their boundaries and develop techniques such as the membership card. Similarly a party theorist such as Duverger, who conceives of party as the representative of interests, i.e., benefit-seekers, has to conclude that the mass-membership party is the wave of the future.<sup>36</sup> Without a membership to make claims and a clearly identified structure to enforce them, what can keep the officeholder in line?

The office-seeker, on the other hand, will naturally prefer not to be burdened with mechanisms designed to keep him in line once he has achieved the goal of office. Hence his preference for light organizational baggage, for a loose structure with

<sup>33</sup> Robert D. Putnam, "Studying Elite Political Culture: The Case of 'Ideology'" *The American Political Science Review*, 65 (September, 1971), 669.

<sup>34</sup> Peter C. Ordeshook, in "Extensions to a Model of the Electoral Process and Implications for the Theory of Responsible Parties," *Midwest Journal of Political Science*, 14 (February 1970), 43-70, applies positive theory to one concept usually associated with problems of internal party organization. His discussion of the responsible party concept, however, deals entirely with the question of whether or not parties present clear and distinct electoral choices.

<sup>35</sup> In many political situations open voting does exist, thus making the type of organization required by voters with a secret ballot unnecessary. It is in such situations, quite naturally, therefore, that the concept

of the minimal winning coalition has been applied most successfully. Thus it has been applied to presidential nominating conventions by Steven T. Brams and William H. Riker, "Models of Coalition Formation in Voting Bodies," in *Mathematical Applications in Political Science VI*, ed. James F. Herndon and Joseph L. Bernd (Charlottesville: University Press of Virginia, 1972), pp. 79-124. Legislative voting is also open so that benefit-seekers need no special structure to demonstrate their support, hence the strategy of the benefit-seeking coalition is most readily observed there. The volume edited by Sven Groennings, E. W. Kelley, and Michael Leiserson, *The Study of Coalition Behavior* (New York: Holt, Rinehart and Winston, 1970) contains a number of such studies in various countries, the common feature of which is that the coalitions are formed by individuals overtly.

<sup>36</sup> Maurice Duverger, *Political Parties* (New York: John Wiley and Sons, 1963).



vaguely defined members. He wants a large body of voters who are identified with his party and can generally be counted on for support. But there are costs to him in putting names to those voters. Obviously he needs help. Inevitably there will be people upon whom he will become dependent and who will therefore be able to make claims. But the office-seeker wants to keep such people at a minimum and easy to replace. Only in this way can he achieve the maneuverability which is one of his most valuable assets.

The maneuverability which the office-seeker gains from a loosely organized party does not free him from all controls; but the controls to which he is subjected do differ from those which are exerted over the leaders of benefit-seeking organizations. Albert O. Hirschman has distinguished between voice and exit as types of organizational controls.<sup>37</sup> In an office-seeking organization, exit, that is the willingness of supporters, including candidates, potential candidates, and voters, to leave, controls behavior. In the benefit-seeking organization, voice or the internal expression of dissatisfaction is the means of control.

The existence of distinctive controls confirms the distinctions in organization which I have already described. Control through exit requires no elaborate structure. The departure of voters, candidates, and other contributors can become

painfully obvious. Office-seekers in the Republican party after 1964 and the Democratic party after 1972 needed no referenda, polls, or conventions to determine that the party had done something wrong and must alter its course. Control through voice, on the other hand, requires an apparatus through which party members can express their sentiments and exert their claims. Continuous forums must be provided, whether through branches, clubs, or conventions. Once these forums exist, it follows that the party's officeholders must carry out in a disciplined fashion the demands which these forums make explicit.

In conclusion I have sought to expand our use of positive theory by pointing out that there are in fact two positive theories, one for benefit-seekers, the other for office-seekers. In so doing I hope I have shown how we can explain the distinction between office-oriented and issue-oriented political organizations which is so often made. But I hope also to have offered a plausible explanation for the tensions which plague all political parties. In the real political world no party consists solely of office- or benefit-seekers. Conflicts within parties arise, therefore, not simply because of ideological or policy differences, but because two goals impose conflicting strategies for winning elections and for organizing parties. Hence the distinctions in positive theory which we have sought to make help explain the never ending struggle over party reform in democracies.

<sup>37</sup> Albert O. Hirschman, *Exit, Voice, and Loyalty* (Cambridge: Harvard University Press, 1970).

# Variations in Elite Perceptions of American States as Referents for Public Policy Making\*

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## Introduction

Analysis of American state expenditures emerged during the 1960s as a major focus for political research. Various public policy analysis or output analysis, these studies attempted to relate state spending to several socioeconomic and political characteristics of the American states.<sup>1</sup> The

\* Constructive criticisms of earlier drafts of this paper were provided by Professors Jack Walker and Richard I. Hofferbert, University of Michigan, and by Professor Stephen Zwerling, University of Connecticut. We are grateful for their comments. In addition, we are indebted to Professors William F. Lott and Stephen M. Miller, Department of Economics, University of Connecticut, for their assistance in calculating Gini ratios.

The research was supported in part by the Graduate Council, Louisiana State University, and the University of Connecticut Research Foundation. The computer analysis in this study was performed on the University of Connecticut's IBM 360-65, which is supported in part by the National Science Foundation, Grant Number GJ-9.

<sup>1</sup> The first published account of the interrelationships between political party competition, socioeconomic characteristics, and state public policy outputs was Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265-289. Other studies which emphasize the role of economic variables in determining state spending include Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review*, 60 (March, 1966), 73-82; and Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966). Publications placing greater emphasis on the impact of political variables on state spending include Ira Sharkansky, "Economic Development, Regionalism, and State Political Systems," *Midwest Journal of Political Science*, 12 (February, 1968), 41-61; Ira Sharkansky, *Spending in the American States* (Chicago: Rand McNally, 1968); and Andrew T. Cowart, "Anti-Poverty Expenditures in the American States: A Comparative Analysis," *Midwest Journal of Political Science*, 13 (May, 1969), 219-226.

For a review of these and many other related works, see John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing With the Relationships Between Political Processes, Socioeconomic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity*, 1 (Spring, 1969), 388-404; and James W. Dyson and Douglas St. Angelo, "Persistent Methodological and Theoretical Problems in Analysis of Policy Outputs," paper presented at the

results of this research generally cast doubt on the importance of political factors in the determination of state policy; social and economic indicators accounted for most of the attributable variance in the levels of public expenditures in the states. This "seemingly outrageous description of state politics"<sup>2</sup> led to more elaborate conceptualization and increasingly sophisticated data analysis. Even with improved methodologies, however, much of the variance in state spending remains unexplained.<sup>3</sup>

Missing from the state spending studies is the contribution made by elected and appointed officials to the formation of public policy in those states. One effort to provide an elite and organizational behavior context for the study of state

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Annual Meeting of the Southern Political Science Association, 1969.

<sup>2</sup> Herbert Jacob and Kenneth N. Vines, "Epilogue," in *Politics in the American States*, 2nd ed., ed. Jacob and Vines (Boston: Little, Brown, 1971), pp. 556-562. The authors point to inadequate measurement of concepts, inappropriate analysis techniques, and overly restrictive definitions of policy outputs as major sources of difficulty in many of the state expenditure studies. Supportive of this critique is Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858-866.

More recent attempts to expand the universe of relevant variables include Ronald E. Weber and William R. Shaffer, "Public Opinion and American State Policy-Making," *Midwest Journal of Political Science*, 16 (November, 1972), 683-699; and Sarah McCally Morehouse, "The State Political Party and the Policy-Making Process," *American Political Science Review*, 67 (March, 1973), 55-72.

<sup>3</sup> Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), 867-879. See also Sharkansky and Hofferbert, "Dimensions of State Policy," in Jacob and Vines, pp. 315-353. Sharkansky and Hofferbert's research is afforded separate notation because their conclusion that different "mixes" of socioeconomic and political factors are influential in state policy making dependent upon the policy area being considered is important to our presentation. In particular, their finding that political factors are more influential with regard to the welfare and education dimension of state policy formation than with the highway and natural resources dimension is one to which we shall return.

public policy formation is Jack Walker's research on the diffusion of innovations.<sup>4</sup>

Walker, who examined the dates of adoption of 88 programs in 12 subject-matter areas in each of the contiguous American states, was able to rank the states in terms of the relative speed of adoption of those programs. Our intent here is to expand on his research *and* to relate the findings to the state expenditure studies.

Walker contends that "state officials make most of their decisions by analogy."<sup>5</sup> This assertion rests on studies of organizational decision making which stress that administrators have neither the time nor the energy to examine thoroughly the alternatives to complex problems. Rather, the administrator looks for the successful resolution of a similar problem elsewhere. State officials, therefore, may take many of their cues for program development from other states. Walker provides an extended discussion in support of this basic premise—i.e., policy is often made by emulation and analogy—and little would be served by replicating it here.

If administrators frequently make policy by analogy, it is important to know from whom they take their policy cues. In attempting to discover patterns of emulation, Walker reported the responses of 235 upper-level administrators in ten states to the following questions: which states are the best sources of information; which states usually seek information; and which states the administrator felt had the best departmental programs. The findings suggest the existence of a "national league," composed of California, New York, Wisconsin, Michigan, Illinois, and Minnesota, from which most other states seek information.<sup>6</sup> In addition, Walker reported extensive regional interaction and exchange; administrators tend to contact and be contacted by officials in neighboring states.

One aspect of Walker's research requires further discussion. He found the speed of diffusion of innovation among the states to be increasing: i.e., the elapsed time before an innovative program was adopted by at least twenty states has decreased in recent years.<sup>7</sup> His explanation for this quickening of diffusion is the development of specialized communication networks among public officials. In addition to the better known organizations, such as the Council of State Govern-

ments and the Federal Commission on Intergovernmental Relations, there exist more than 80 national, professional associations of state officials—e.g., the National Association of State Highway Officials; the National Association of State Supervisors and Directors of Secondary Education; and the Association of State and Territorial Health Officers. Most of these specialized groups meet regularly and publish journals or newsletters. They facilitate the exchange of policy and program development information among the administrators in the fifty states.<sup>8</sup>

Thus, Walker's discussion implies that decision making by emulation or analogy occurs primarily within policy areas. The head of a state health agency may be expected to interact with and read about the activities of health officers in other states rather than to focus, for example, on the activities of highway department officials. In reporting his data, however, Walker did not distinguish the responses of the state administrators he interviewed within specific policy areas.

Our aim is to introduce into this discussion the differences in assessments of state programs *within policy areas*. It is less important, after all, to know that executives in Utah generally consider California and New York to be worthy of esteem than it is to know how much consensus there is among, for example, health officials about the best state health department in the country.

#### Variation in Assessments of State Programs by Policy Area

The data were gathered by mail questionnaire in 1971–1972 from appointed, upper-level administrators in ten American states.<sup>9</sup> Included

<sup>8</sup> The development of communications networks may lead to increased decision making by emulation. This, in turn, may produce increased demand for additional channels of communication, further stimulating the process.

<sup>9</sup> We are engaged in a longitudinal study of state executives. The initial research, conducted in 1970, elicited 1,870 returns of a four-page mail questionnaire which is a response rate of 80 per cent. This research is described more fully in Fred W. Grupp, Jr. and Alan R. Richards, "Partisan Political Activity among American State Executives," paper delivered at the Annual Meeting of the American Political Science Association, 1971.

The research reported here is based on a second questionnaire mailed to the original 1,870 respondents. This mailing produced 907 usable returns. The reduced response rate is due, in part, to occupational mobility; some of the executives had retired or changed jobs. Perhaps even more relevant was the inclusion of a Thematic Apperception Test in the second questionnaire. The second-wave respondents differ only minutely from the first in objective characteristics—i.e., they are very similar in terms of age, sex, race, education, and type of agency employment. In short, they appear to be a representative subset of the original respondents.

<sup>4</sup> This research is reported in Jack L. Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880–899, and in "Innovation in State Politics," in Jacob and Vines, pp. 354–387. Subsequent references to Walker's research will refer to this latter work.

<sup>5</sup> Walker, pp. 364–368.

<sup>6</sup> Walker, p. 380.

<sup>7</sup> Walker, pp. 376–377.

were state executives from California, Ohio, Wisconsin, Louisiana, New Mexico, Tennessee, Kansas, Montana, South Dakota, and Nevada.<sup>10</sup> The reader will note the absence of any New England or Middle Atlantic states in this sample. This may lead to fewer references to Northeastern states. Our concern, however, is not with detailing communication patterns, but rather with measuring, if only roughly, the extent and effects of agreement about the most esteemed state agency among state executives within general policy areas. And, as we shall see, the data suggest that where states have outstanding programs, administrators in other states are aware of them—e.g., among our respondents in Wildlife and Fisheries agencies, there is near unanimity that Missouri has the best programs.

Other differences between Walker's research and our own require discussion. First, our sample included the high-ranking appointed officials of all agencies administering statewide programs within the ten states. The major policy areas included in this study, but not in Walker's, include natural resources, business and economic regulation, health (only mental health officials are included in Walker's interviews) and public works agencies. The responses of advisory personnel, such as budget officials, legislative reference personnel, and governor's staff, constitute a large portion in Walker's sample.<sup>11</sup> The overrepresentation of these kinds of officials reflects Walker's great concern with the adoption of innovative and possibly controversial programs. The relevance of

such advisory personnel is obvious, since new programs will be scrutinized for cost and political acceptability prior to adoption. Our interest is more general. We wish to identify those state agencies which are widely esteemed on the assumption that the innovative policies which interest Walker are perceived initially by practitioners rather than by advisory personnel, and that important program and policy changes may take place within agencies without attracting much attention. For example, the practice of "grooving" highways to reduce skidding on wet roads may spread throughout the country without much controversy or change in highway department budgets.

A second distinction concerns the measures which were used. Many of Walker's findings are based on reported communications between state officials, of the sort, "from which states have you sought information; which states have contacted you for information?" He also constructed an Index of Approval composed of responses to several questions about program quality in other states.<sup>12</sup> This latter approach most nearly resembles our own. Each administrator in our sample was asked to rate his or her own agency on a ten-point, self-anchoring scale in the form of a ladder with the top labeled "absolutely ideal" and the bottom labeled "the very worst." Subsequently, the executives were directed to rate the "best state agency carrying out the same type of program" as their own, and then to identify the state. This produced assessments of the perceived quality of their own agency, identification of the state with the best agency, and a measure of the difference in quality between their agency and the one that they most highly esteem. For the purposes of this paper, we shall concentrate on the extent of agreement about the states having the most nearly ideal agencies.

The data presented in the first column of Table 1 indicate that two states, California and New York, are mentioned much more frequently than any of the others. Together they account for 34 per cent of the total citations. This finding is fully consistent with Walker's. What might not have been anticipated on the basis of his research are the high levels of esteem accorded Texas, Oregon, Washington, Colorado, and Florida.<sup>13</sup> According

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The state executives are all appointed, politically or under a merit system, and serve in a state's executive branch. Excluded are elected officials; officials of state institutions of higher learning; members of professional and occupational licensing boards; executives in agencies with less than a statewide jurisdiction, such as a port authority; executives in state institutions, such as prisons or hospitals; and uniformed police officials. The goal was to include only upper-level appointed state executives and to include comparable executives in each of the ten states.

<sup>10</sup> The choice of states was influenced by Norman R. Luttbeg's study, "Classifying the American States: An Empirical Attempt to Identify Internal Variations," *Midwest Journal of Political Science*, 15 (November, 1971), 703-721. Four political "regions" emerged from his analysis: Industrial, with 17 states; Southern, with 17 states; Sparsely Populated, with 13 states; and Frontier, with 3 states. Our sample of states includes three from the Industrial Region (California, Wisconsin, and Ohio), three from the Southern Region (Louisiana, New Mexico, and Tennessee), three from the Sparsely Populated Region (Kansas, Montana, and South Dakota), and Nevada from the Frontier Region.

<sup>11</sup> Walker at p. 379 describes the respondents to his survey as "top administrators in the departments of education, labor, mental health, welfare, the budget bureau, legislative reference service, and the governor's chief assistant for program development."

<sup>12</sup> Walker, p. 381.

<sup>13</sup> Walker (pp. 378-385) deals with the persistence of regionalism in cue-taking among state administrators. Regional leaders, such as Texas and Florida, emerge here as well. Overall, 20 of the 48 contiguous states are included in the two studies. The combined results suggest the existence of national and regional leaders. Because of the nature of the sampling involved, however, only the rough outlines of the influence system are revealed. No definitive mapping of the system is claimed.

Table 1. Number of Citations<sup>a</sup> as State with Best Agency by General Policy Area

State Cited <sup>b</sup>	Policy Area <sup>c</sup>									State Cited
	Total	Public Works	Health	Natural Resources	Business Regulation	Education	Human Relations	Welfare	Auxiliary	
Alabama	1		1							Alabama
Alaska	1							1		Alaska
Arizona	4					1	2		1	Arizona
Arkansas	3					1		1	1	Arkansas
California	103	30	9	21	10	11	2	3	17	California
Colorado	15	3	1	2	1	4		1	3	Colorado
Connecticut	4		1	1		1		1		Connecticut
Florida	19	3	1	6	1	4		2	2	Florida
Georgia	7		1	1	1	4				Georgia
Hawaii	5	1			1	2			1	Hawaii
Idaho	1								1	Idaho
Illinois	14	2		5	4	3				Illinois
Indiana	2		1				1			Indiana
Iowa	5	2				1		1	1	Iowa
Kentucky	7	1	1	2			1		2	Kentucky
Maine	1							1		Maine
Maryland	4		1		1		1		1	Maryland
Massachusetts	5				2	1		2		Massachusetts
Michigan	30	1	7	6	2		2	3	9	Michigan
Minnesota	18	1	4			1	1	8	3	Minnesota
Missouri	10		1	7	1			1		Missouri
Montana	1			1						Montana
Nebraska	3		1	1					1	Nebraska
Nevada	5	1		1			1	1	1	Nevada
New Hampshire	1					1				New Hampshire
New Jersey	5				2	1			2	New Jersey
New York	65	2	19	1	11	10	9	3	10	New York
North Carolina	11		2	2	1	1	1		4	North Carolina
North Dakota	6	1		2				3		North Dakota
Ohio	14			1	2	8			3	Ohio
Oklahoma	6		1	1	2	1		1		Oklahoma
Oregon	20	1	2	4		3	2	3	5	Oregon
Pennsylvania	7			2	1	2		1	1	Pennsylvania
Tennessee	1					1				Tennessee
Texas	30	7		1	3	9	2	4	4	Texas
Utah	10			1	1	2	5		1	Utah
Vermont	1							1		Vermont
Virginia	3		1	1	1					Virginia
Washington	21	1		6		5	1	3	5	Washington
West Virginia	2						1	1		West Virginia
Wisconsin	24	2	1	2	4	2	2	6	5	Wisconsin
Totals	495	59	56	78	52	80	34	52	84	Totals

<sup>a</sup> Of the 907 respondents, 158 did not name the state with the "best agency carrying out the same type program." In addition, the 242 executives who named their own state as having the best agency were excluded since they are not likely to seek policy guidance from other states. Finally, the responses of 12 executives in civil defense or statewide cultural agencies were also deleted since those categories contained too few cases for analysis. After all exclusions, 495 responses remain.

<sup>b</sup> Nine states received no citations and, therefore, are not listed in the left-hand column of the table. These states are: Delaware; Kansas; Louisiana; Mississippi; New Mexico; Rhode Island; South Carolina; South Dakota; and, Wyoming.

<sup>c</sup> The agency coding within policy areas is presented in the Appendix.

to the data in Table 1, it is clear that esteem is distributed unequally among the states. Nine states received no citations at all, while an additional eight were mentioned as having the best state agency only once. Viewed another way, if esteem were distributed equally, each state would have received almost 10 of the 495 total citations. In fact, only 15 (30 per cent) of the states were mentioned that frequently.

There are also great variations in the patterns of distribution of a state's esteem across policy areas as shown in the rows of Table 1. For example, only California, New York, and Wisconsin received favorable mentions in all eight categories of agency type. Some states, such as Nevada, North Carolina, and Oklahoma, tended to have evenly distributed citations across a relatively large number of policy areas. In contrast, other states (Missouri, Minnesota, and Illinois are good examples) received a large proportion of their favorable citations in one or two policy areas.

More important are the variations in the distribution of esteem within policy areas as shown in the columns of Table 1. The extent to which esteem in a particular policy area is distributed unequally among the cited states is a measure of consensus about the best state agency. Where proportionately greater numbers of states are cited or where the citations as best state agency are distributed more evenly, consensus is lacking and less agreement should exist on policy and programs worthy of incorporation. No policy area displayed in Table 1 is characterized by total consensus (where one state received all the citations) or by total dissensus (where each of the states are cited equally). Yet there are some obvious differences in the patterns of responses within policy areas: some tend toward consensus, others toward dissensus. For example, more than half (30 of 59) of the upper-level state executives employed in public works departments cited California as the state with the best public works agency. In contrast, no state received more than 14 per cent (11 of 80) of the citations of education officials.

One way to measure the tendency toward consensus within policy areas would be to calculate the percentage of esteem awarded to the most esteemed state and cumulate the per cent obtained as the second most esteemed state is added, and so on. This procedure is cumbersome, however, and the results, while suggestive, do not lend themselves to direct comparison. Fortunately, economists have developed better ways of displaying and comparing inequalities in the distribution of resources, such as income. They frequently utilize the Lorenz curve to display the extent of income inequality within a system and utilize Gini coefficients

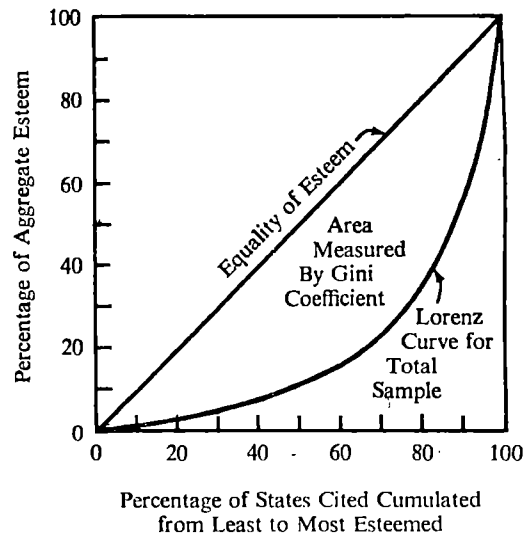


Figure 1. The Lorenz Curve of Inequality of Esteem

to compare the extent of inequality between systems. The Lorenz curve displays the cumulative proportions of the resource or attribute (in this case, esteem) on the vertical or y-axis accruing to cumulative proportions of the population (in this case, states) from the least to the most esteemed on the horizontal or x-axis. Perfect equality in the distribution of an attribute is represented in a Lorenz figure by a diagonal straight line from the lower left-hand corner to the upper right-hand corner. The extent of inequality is indicated by the degree of "convexity" of the Lorenz curve. The Gini coefficient is obtained by dividing the area between the diagonal line and the Lorenz curve by the total area under the diagonal line. Gini coefficients range from +1.00 (perfect inequality) to 0.00 (perfect equality). Thus, the larger the Gini coefficient, the more unequal the distribution of esteem and the greater the consensus about the better state agencies.

A Lorenz curve of inequality of esteem is presented in Figure 1. This curve is based on the overall distribution of esteem (495 citations) among the 41 states listed in Table 1. The Gini coefficient for this curve is .603.<sup>14</sup>

<sup>14</sup> Gini ratios are finding increased use in political science. See, among others, Bruce M. Russett, "Inequality and Instability: The Relation of Land Tenure to Politics," *World Politics*, 16 (April, 1964), 442-454; Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), chap. 3; and Thomas R. Dye, "Income Inequality and American State Politics," *American Political Science Review*, 63 (March, 1969), 157-162.

The formula utilized here in calculating Gini coefficients is described in Herman P. Miller, *Rich Man, Poor Man* (New York: Thomas Y. Crowell, 1971), pp. 274-276.

Table 2. Inequality of Esteem by Policy Area, in Rank Order

Policy Area	Gini coefficient
Public Works	.602
Health	.554
Natural Resources	.506
Business Regulation	.456
Education	.433
Human Relations	.383
Welfare	.374
Auxiliary	.367

Of interest is the extent to which the Gini coefficients vary between policy areas. It will be remembered that the larger the coefficient, the greater the inequality of esteem. A large Gini coefficient, therefore, indicates that a small percentage of states received a large percentage of the favorable citations; there is general agreement about the better state agencies. A small coefficient indicates the absence of any such consensus. The Gini coefficient for each of the policy areas is presented in Table 2. Inequality of esteem is greatest in public works agencies.<sup>15</sup>

A brief discussion of the variations in Gini coefficients between policy areas will lay the groundwork for linking those results with policy making in the American states. To begin, let us examine the probable causes of the differences in Gini ratios displayed in Table 2. A most apparent difference between agency types characterized by higher levels of consensus about the better state agencies and those with lower levels of consensus is the extent to which their programs generate class-based conflict. By and large, the policy areas ranked high in Table 2—public works, health, natural resources—are not central to the “have/have-not” dimension of politics.

<sup>15</sup> While Gini coefficients are frequently directly compared to one another, there are biases which may lead to distortions in the coefficients if the data are aggregated on the basis of unequal numbers of cells or if a large percentage of the cases fall in one cell. See Richard A. Genson, “Gini Ratios: Some Considerations Affecting Their Interpretation,” *American Journal of Agricultural Economics* (August, 1970), 444-447.

The Gini coefficients computed here are subject to these sources of distortion. In each instance, the coefficients were calculated on the number of states receiving citations, rather than on a base of 50 states. The rationale for this procedure is that our interest lies in the extent to which esteem is distributed unevenly among those states which are cited. When the ratios are computed on the basis of 50 states, the effect is to increase all of the coefficients to the .8 and .9 range, and the data are not as revealing. What is important is that the rank ordering displayed in Table 2 is not changed by the alternate computation method.

In addition to differences in clientele, the agency types for which inequality of esteem is greatest also have program outputs which are more easily evaluated. Either they are tangible, such as roads or boat-docking facilities, or they lend themselves to easy comparison, such as infant mortality statistics. It is noteworthy that public works agencies, where consensus about the better state programs is greatest, are characterized by both few organized clients and tangible outputs. Education, welfare, and human relations agencies, on the other hand, are more frequently involved in class-based conflict, deal directly with larger publics, and have less clearly defined standards for evaluating program effectiveness.

Thus far, all that can be said with assurance is that levels of consensus about the best state agency vary by policy areas among upper-level state executives. While this is an important modification in the diffusion-of-innovation literature,<sup>16</sup> it contributes only indirectly to our understanding of public policy making in the American states. Far more precise measures of the relative influence of various political actors, including state administrators, are needed before the public policy process will be understood.

#### Inequality of Esteem and Political Influence

Is there a relationship between an administrator's ability to point to a successful program in another state and his relative influence in his state's policy-making process? Walker points out that “uncertainty and the fear of unanticipated consequences have always been formidable barriers to reform. Proponents of new programs have always had to combat the arguments of those who predict dire consequences if some innovation is adopted. . . . Inertia can more easily be overcome if the proponent of change can point to the successful implementation of his program in a similar setting.”<sup>17</sup>

We hypothesize that there is a relative increase in the policy-making influence of those administrators who are able to cite effective programs in other states. One difficulty in testing the hypothesis with these data is that each of the executives (Table 1) did, in fact, name another state as having the more nearly ideal agency. Thus, each of the administrators should receive an equal increment of additional influence.

This is not a likely result, however, since in some policy areas there is little agreement about

<sup>16</sup> See also Virginia Gray, “Innovation in the States: A Diffusion Study,” *American Political Science Review*, 67 (December, 1973), 1174-1185. Professor Gray makes a similar point: diffusion of innovation varies by policy area.

<sup>17</sup> Walker, p. 366.

which state's programs are effective and worthy of emulation. Rather, we assume that the greater the consensus among executives about the leading state agencies, the more likely that policy recommendations which cite program innovations from those states will be received credibly by policy-making actors, such as the governor, legislators, and the like.

In contrast, in welfare departments, for example, where there is little agreement about any state having programs worth emulating, the executive has less to offer the other policy makers. The executive cannot provide a proven solution to the problem and, therefore, will be less influential in the decision-making process. Thus, our revised hypothesis is: *The greater the inequality of esteem in a policy area, the greater the policy-making influence of administrators in that policy area.* Policy areas were ranked by inequality of esteem in Table 2. According to our hypothesis, public works officials should be relatively more influential than executives in agencies characterized by less consensus about the leading state programs.

Assessments of policy-making influence were gathered from the respondents by means of three openended questions. The executives were asked to list in order of importance the "three agencies, groups, or individuals (such as governor, legislators, etc.) that are the most influential in determining your agency's (a) Broad general policy; (b) Specific programs implementing policy; and, (c) Individual decisions implementing programs." These questions elicited a wide variety of responses, ranging from powerful private individuals and groups to federal agencies and the U.S. Supreme Court.

Reported in Table 3 are the percentages of executives within policy areas naming their own agency or agency personnel as the most influential actor(s) at each of the three policy-making levels. Since we are dealing with perceptions of influence, it is appropriate to distinguish the responses of the most powerful executives from the responses of those further down in the hierarchy. It should be remembered that *all* of the respondents are high-level executives employed in important positions in their states. The distinction we make is between agency heads and other executives at the top of the agency's organization chart and those executives who are lower in the hierarchy by one level or more.

In addition to percentages, Spearman's  $\rho$  between the rank ordering of levels of consensus (Table 2) and ordering of percentages of executives naming their own agency as most influential in the policy-making process is presented for each of the three policy levels. According to our hypothesis, the  $\rho$ 's should be positive; that is, as consensus about best state programs increases, the percentage of state executives perceiving their

own agency as most influential should increase.

The rank-order correlation statistics presented in Table 3 are uniformly positive and reach statistical significance at each policy level among the very highest level executives.<sup>18</sup> We are less impressed by the statistical significance than by the relative consistency of the findings when level of organization is controlled. The fact that the predicted relationship between rank orderings of policy areas by inequality of esteem and percentage citing own agency as most influential persists—though short of statistical significance—among the more junior executives, strikes us as additional support for the hypothesis.<sup>19</sup>

Consideration of other responses to the three policy questions (not reported here) indicate that the executives understood what was being asked of them and answered in realistic ways. For example, executives in agencies receiving grant-in-aid funds more frequently cited federal agencies as influential than did those in agencies receiving little or no federal funds. Education officials perceived local governmental units as influential, while welfare, human relations, and business regulation administrators were mindful of interest groups and their clientele. Furthermore, the patterns of responses make sense. As the policy level becomes less general and more specific—i.e., across the columns of Table 3—the percentages of administrators identifying their agency as most influential increases. In short, we believe that the executives' responses are valid and reliable, *at least from their point of view.* This does not mean that their perceptions are not distorted, nor does it determine whether the assessments of top-level executives are more accurate than those of executives lower down in the organizational hierarchy. The data at hand do not permit distinctions of this kind, and the policy-making process is so little understood that we cannot either affirm or dismiss the executives' view of

<sup>18</sup> It might be argued that the inclusion of the heterogeneous auxiliary agencies artificially inflates the  $\rho$ 's. The respective rank-order correlations ( $\rho$ ) with auxiliary agencies excluded are: .555; .439; .505; .378; .714; and, .392. The critical value ( $p$ . .05) for  $N$  of seven (one-tailed test) is .714.

<sup>19</sup> An alternate test of the hypothesis, suggested by one of the anonymous reviewers, is to compare the perceptions of agency influence at each of the three levels of policy making between those executives (type I) who named either of the two most esteemed states as having the more nearly ideal agency and those of the executives (type II) who cited a less esteemed state. This analysis was performed. The type I executives were more likely to perceive their agency as most influential in the policy-making process at each of the three levels of decision making—General policy, 24% to 19%; Specific program, 61% to 56%; and Individual decision, 84% to 79%. Each of these results is in the direction predicted by the hypothesis, though none of the percentage differences reaches the .05 level of statistical significance.



Table 3. Executives Identifying Their Own Agency as Most Influential by Policy Level, Organizational Position and Agency Type, in Per Cent

Agency Type	Policy Level							
	General Policy		Specific Program		Individual Decision		Minimum (N)	
	I <sup>a</sup>	II <sup>b</sup>	I <sup>a</sup>	II <sup>b</sup>	I <sup>a</sup>	II <sup>b</sup>	I <sup>a</sup>	II <sup>b</sup>
Public Works	31	21	67	58	90	81	(17)	(31)
Health	43	27	86	63	93	87	(16)	(61)
Natural Resources	32	26	57	70	89	90	(58)	(78)
Business Regulation	13	19	64	53	73	74	(24)	(30)
Education	28	29	76	64	88	80	(19)	(70)
Human Relations	27	18	46	41	75	73	(27)	(22)
Welfare	16	16	59	55	87	85	(19)	(54)
Auxiliary	15	17	39	48	74	79	(60)	(53)
(N) <sup>c</sup>	(224)	(411)	(221)	(406)	(220)	(399)		
Spearman's <i>r</i> <sub>ho</sub>	.690 <sup>d</sup>	.629	.666 <sup>d</sup>	.512	.713 <sup>d</sup>	.429		

<sup>a</sup> Agency heads and other highest level executives.

<sup>b</sup> Executives one level or more down in the organizational hierarchy.

<sup>c</sup> The 242 executives who named their own state as having the most nearly ideal agency are included here to maintain cell sizes. Of the 717 executives, 82 did not respond to the policy questions. The maximum N for this table is 635.

<sup>d</sup>  $p < .05$ . The critical value for an N of eight (one-tailed test) is .643.

power relationships in state policy making. Yet there is a relationship between the executives' perceptions of state agencies worthy of emulation and their perceptions of the influence of their own agency in state public-policy formation and administration.

#### Evidence from the State Expenditure Studies

The state expenditure studies also indicate that the relative influence of actors in the policy-making process in the American states varies by policy area.<sup>20</sup> To what extent do those findings support our hypothesis that administrators in policy areas characterized by high levels of inequality of esteem are relatively more influential than are executives in agencies where there is less consensus about the leading state programs?

The most directly relevant and, in many ways, the most impressive of the policy output studies is that in which Sharkansky and Hofferbert factor analyzed 21 socioeconomic variables, 53 political system variables, and 34 state expenditure and public service variables. From this factor analysis emerged two environmental factors (industrialization and cultural enrichment); two political factors (competition-turnout and professionalism-local reliance); and three policy factors (welfare-education, highways-natural resources, and public safety). Their methodology is noteworthy since it avoids some of the problems

associated with state spending studies by incorporating additional nonfiscal measures of policy output. For example, with regard to education the authors included the percentage of 1959 ninth-grade students who actually graduated three years later and also the percentage of students passing the Selective Service mental exam. Concerning highways they included a measure of highway safety (population per highway fatality) and measures of the numbers of miles of types of highways in relation to the population. In each instance, the attempt was made to utilize some measures of the actual service rendered to the state's population rather than relying solely on spending as an indicator of state public policy.

Sharkansky and Hofferbert find that political factors correlate more closely with the welfare-education dimension of state policy than with the highways-natural resources dimension. This finding supports our hypothesis.<sup>21</sup> Where consensus among state executives about the better state programs is low, as it is in welfare and education agencies (Table 2), the administrators can be expected to be relatively less influential in the policy-making process. Other political actors, such as political parties, interest groups, and elected officials, may be more influential. On the other hand, in those policy areas characterized by high con-

<sup>20</sup> Cnudde and McCrone, p. 865. See also Sharkansky and Hofferbert, note 3 above. But compare Thomas R. Dye, *Understanding Public Policy* (Englewood Cliffs, N.J.: Prentice-Hall, 1972), p. 261 where he finds environmental factors (income, education, urbanization, and federal aid) are predominant.

<sup>21</sup> For those interested in the origins of an hypothesis, the actual evolution began with the Sharkansky and Hofferbert findings. How might one account for the waxing and waning of political factors as policy determinants? The serendipitous juxtaposition of Walker's research in the same issue of the *American Political Science Review* (September, 1969) suggested a possible explanation.

sensus, such as highways (Public Works) and natural resources, the administrators may be expected to press for policies perceived as successful in other states, thus reducing in importance other influences such as the "competition-turnout" factor.

### Summary and Conclusion

Following Walker, we have assumed that executives in large complex organizations frequently make decisions by analogy and emulation. Administrators in the fifty states are linked by both formal and informal communication networks. They are increasingly aware of programs and policies, both in force and under consideration, in other states. These information sources provide the executive with a wide range of possible program solutions to similar problems in his own state. We further assume that a state executive is more likely to take policy cues from state agencies which are reputed to administer effective programs than from those he considers less worthy.

Our research not only substantiates Walker's conclusions about the existence of a set of highly esteemed states, but also expands upon his research by revealing variations in the patterns of esteem dependent upon the policy area under consideration. In addition, we hypothesized that the amount of policy-making influence accorded to state administrators would be directly related to their ability to provide proven solutions to problems in their state. In other words, executives in those policy areas characterized by high levels of consensus about the better state programs have their influence increased because they are able to approach legislators or the governor's staff with specific policy proposals which are known to be working well in those other highly esteemed states. This advantage does not accrue to executives in policy areas where there are no acknowledged leaders.

We drew upon the perceptions of agency influence cited by the state executives in our sample and upon published research on state spending in attempting to test the hypothesis. The evidence from the literature and from our own data provide some support for the hypothesis, though it cannot be considered conclusive. State administrators are only one of many influences in the policy process, albeit one that has received little attention. It was not our intent to ascribe to state executives an inordinate amount of influence; rather we believe that their ability to influence policy outcomes varies by policy area, within as yet uncharted limits.

Both the research on American state policy discussed here and the responses of the executives in our sample to questions about influential actors in the policy process indicate that the "mixes" of

factors determining state policy outputs vary according to program. There is obvious need for better developed theory to account for the variations by policy area.

### Appendix

#### Agency Type:

*Education*—Department of Public Education (Instruction etc.), Central Agencies for Higher Education, and Vocational Education, Scholarship and Loan, and Educational Finance agencies, but *not* Educational Retirement agencies.

*Natural Resources*—Agricultural, Forestry, Mineral, Wildlife and Fisheries, Land, Water, Air, Conservation, Oil, Gas, and other Natural Resources agencies, and Parks and Recreation agencies.

*Business Regulation*—Commerce and Industry, Development and Planning (Tourist, Economic, Industrial), Transportation, Public Service and Public Utilities, Gambling and Alcohol (Sin), Insurance, Corporations, Banks, and Securities, Central Licensing, Consumers', and other Business and Economic Regulation agencies.

*Human Relations: Labor, Industrial and Special Groups*—Employment Security, (State Employment Service), Labor, Women's, Youth, Indian, Aged, Veterans', and other Human Relations agencies.

*Public Safety Agencies*—Civil Defense, Nuclear Energy, Fire Marshall, Disaster, and other Public Safety agencies.

*Health*—Public Health (General), Mental Health, Statewide Central Hospital or Institutional Health, and Public Health and Welfare (health division, professional line employees and all non-professional line health and welfare employees).

*Welfare*—Public or Social Welfare, Central Statewide Correctional Institutions, Rehabilitation, Probation and Parole, and Public Health and Welfare agencies (welfare division, professional line employees).

*Public Works*—Highway, Airport, Ports and Waterways, and other Public Works agencies.

*Auxiliary*—Immediate Office of the Governor, Department of Justice, Offices of Secretaries of State, Offices of Treasurers, Comptrollers and Auditors, Tax Revenue Collecting agencies, Budget and Finance agencies, Statewide Personnel agencies (Employee and Education Retirement included), State Building agencies, General Services Administration (Purchasing, Central Data Processing, Records Management, and other auxiliary agencies).

*Other*—Statewide Fair, Exhibition, Museum agencies, and other agencies.

# Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe\*

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## Alliances and Balance of Power Systems

In this paper we partially test Professor Morton A. Kaplan's "theory" of the Balance of Power System first proposed in his well known *System and Process in International Politics*.<sup>1</sup> Given that nineteenth-century Europe comprised a balance of power international system and given that Kaplan has specified the "essential rules" of such a system, we ask the question: Did European alliance behavior in this period conform to Kaplan's theoretical expectations of a random alliance process?

The outbreak of World War I in 1914 traditionally marks the end of the nineteenth-century historical period. Conventional diplomatic wisdom suggests that war occurred because the balance of power system collapsed owing to a "hardening of alliances" some time after 1900. If a balance of power system loses its flexibility because the participants have abandoned systemic alliance formation rules, Kaplan's theory predicts such a result.<sup>2</sup> We therefore also ask the question: Why did a general European war break out in 1914?

We attempt to answer these related questions by applying probability theory, specifically a Poisson model, to the analysis of new data on fifty-five alliances among the five major European powers during the period 1814-1914. Because our research questions are so large-scale, our conclusions cannot be regarded as definitive. We do find, however, that the data examined very strongly support our hypotheses.

**Definitions.** No two concepts are more central to the analysis of international politics than "alliance" and "balance of power."<sup>3</sup> Theory in the

field is so underdeveloped, however, that these concepts are usually vaguely defined, mixing description, prescription, and evaluation, so that an agreed-upon meaning for these terms does not at present exist.

Since we intend to test aspects of Morton Kaplan's theory of the behavior of balance of power systems, we shall use his definition of this concept. The pattern of interactions between two or more state actors, which composes a system of action, will be called a *balance of power system* if the following three conditions hold: (1) the system is without a political subsystem that authoritatively regulates the behavior of system members, such as a fully effective United Nations, (2) there are at least five essential state actors, and (3) the six rules of actor behavior specified by Kaplan are followed.<sup>4</sup> Kaplan regards nineteenth-century European international politics as one such balance of power system.<sup>5</sup>

Our conception of *alliances* is that they are a subclass of alignments wherein at least two states make a military commitment against at least one other state to fight or to remain neutral. Alliances share with alignments the following features: interstate cooperation or coordination over a prob-

pendence (Baltimore: The Johns Hopkins University Press, 1962); Julian Friedman, Christopher Bladen and Steven Rosen, eds., *Alliance in International Politics* (Boston: Allyn and Bacon, 1970); Inis Claude, Jr., *Power and International Relations* (New York: Random House, 1962); Ernst B. Haas, "The Balance of Power: Prescription, Concept, or Propaganda," *World Politics*, 5 (July, 1953), 442-447 and "The Balance of Power as a Guide to Policy Making," *Journal of Politics*, 15 (August, 1953), 370-398; Kaplan, *System and Process*; and Paul Seabury, ed., *Balance of Power* (San Francisco: Chandler, 1965).

<sup>4</sup> Kaplan, *System and Process*, pp. 22-23. These rules are: "1. Act to increase capabilities, but negotiate rather than fight. 2. Fight rather than pass up an opportunity to increase capabilities. 3. Stop fighting rather than eliminate an essential national actor. 4. Act to oppose any coalition or single actor which tends to assume a position of predominance with respect to the rest of the system. 5. Act to constrain actors who subscribe to supranational organizing principles. 6. Permit defeated or constrained essential actors to re-enter the system as acceptable role partners or act to bring some previously inessential actor within the essential actor classification. Treat all essential actors as acceptable role partners."

<sup>5</sup> Kaplan, *System and Process*, pp. 22, 28-29.

\* The authors wish to thank Professors Philip L. Beardsley, William D. Coplin, Richard E. Hayes, Michael K. O'Leary, Randolph M. Siverson and several anonymous referees for their helpful criticisms of the original version of this paper.

<sup>1</sup> New York: Wiley Science Editions, 1964. First published in 1957. All quotations of Kaplan in this paper are from the 1964 paperback reprint unless otherwise noted.

<sup>2</sup> Kaplan, *System and Process*, preface and pp. 27-29, 35-36.

<sup>3</sup> Representative discussions may be found in: George Liska, *Nations in Alliance: The Limits of Interde-*

lem; combination of state capabilities; pursuit of state interests; and mutual assistance. Alliances are distinct from alignments because: they have an actual or anticipated enemy (in Liska's terms—"Alliances are against, and only derivatively for, someone or something"<sup>6</sup>); they contemplate military engagement and the risk of war; and they are based upon mutual interest in the international *status quo* or its revision.<sup>7</sup> While alliances are often embodied in formal treaties, as part of the process of international politics, they can be informal and highly dynamic.

Most authorities recognize that alliances are basic to the operation of a balance of power system.<sup>8</sup> Kaplan argues that the six essential rules of the balance of power system operate to produce fluid alliances that "regulate" the system and prevent any one power or coalition of powers from achieving hegemony.<sup>9</sup> Indeed, the literature suggests that alliances are the primary means available to the foreign-policy maker in a balance of power situation; they enable leaders to maintain the pluralism of the system and to ensure the continued existence of its essential actors, albeit at the cost of solidarity. In a balance of power system, alliances are able to deter hegemonial ambitions by being fluid and flexible. Alliances are made without regard to ideology, cultural affinities, relations among monarchs and other elites; and in particular, each alliance is *independent of past alliances and alignment patterns*. Each is based solely upon present state interest and current threats to the balance of power.<sup>10</sup>

Behind this model of alliance process in balance of power systems is an assumption of actor ra-

tionality.<sup>11</sup> First, it is assumed that national interests have been ordered into a scale of preferences and, second, that decisions on when and with whom to form an alliance are based upon carefully considered cost-benefit analyses.<sup>12</sup> Thus, the process of alliance combination and recombination in a balance of power system resembles an infinite, ongoing, *n*-person, non-zero-sum game where the payoff remains indefinitely in the pot.<sup>13</sup>

**Theory.** In his study of *System and Process in International Politics*, Professor Kaplan produced a "heuristic" theory of six international systems that was meant to be explanatory, predictive, and prescriptive.<sup>14</sup> Kaplan's theory is a rational theory, for it only "predicts what state behavior will be if statesmen are rational, completely informed, and politically free to make external decisions on the basis of considerations of external rationality."<sup>15</sup> The theory is "heuristic" or general<sup>16</sup> for two reasons; first, the paucity of comparable and coded historical data made precise predictions "incautious" in 1957,<sup>17</sup> and second, a theory of international politics should not be expected to predict individual events. It should, however, be able to predict characteristic or modal behavior patterns.<sup>18</sup> Because in many respects Kaplan's theory represents a verbal formalization of traditional theorizing on international political behavior, his theory of the operation of a balance of power system is more specific than that of any of the other five types of system he analyzes. Moreover, Kaplan makes it explicit how his theory of balance of power politics can be tested: "... a high correlation between the pattern of national behavior and the essential rules of the international system

<sup>6</sup> Liska, *Nations in Alliance*, p. 12.

<sup>7</sup> Julian Friedman, "Alliance in International Politics," in *Alliance in International Politics*, ed. J. Friedman et al., pp. 4-5 and George Modelski, "The Study of Alliances: A Review," *Journal of Conflict Resolution*, 7 (December, 1963), 769-776 provide discussions of the salient characteristics of alliances.

<sup>8</sup> Representative discussions are: Kaplan, *System and Process*, pp. 35, 66, 115; Hans J. Morgenthau, *Politics Among Nations: The Struggle for Power and Peace*, 3rd ed. (New York: Knopf, 1963), pp. 167-223; and Friedman, "Alliance in International Politics," p. 23.

<sup>9</sup> Kaplan, *System and Process*, p. 125. See also Friedman, pp. 21-22 for the "functions" of alliances in international systems.

<sup>10</sup> These aspects of the role of alliances as a tool of foreign policy are further discussed in: Herbert Dinerstein, "The Transformation of Alliance Systems," *American Political Science Review*, 59 (September 1965), 589-601. See also: Liska, *Nations in Alliance*, p. 16; Kaplan, *System and Process*, p. 66; J. G. Cross, "Some Theoretic Characteristics of Economic and Political Coalitions," *Journal of Conflict Resolution*, 11 (June, 1967), 187; and Roger Masters, "A Multibloc Model of the International System," *American Political Science Review*, 55 (December, 1961), 788.

<sup>11</sup> This "model" is clearly prescriptive, and *hypothetically it is an accurate description and prediction of alliance politics in any balance of power system*. Like other rational models, it mixes description, explanation and possible prescription, a point well made by Otto Davis in "Notes on Strategy and Methodology for a Scientific Political Science," in *Mathematical Applications in Political Science IV*, ed. Joseph K. Bernd (Charlottesville: The University Press of Virginia, 1969), pp. 22-38.

<sup>12</sup> Friedman, "Alliance in International Politics," p. 23.

<sup>13</sup> Arthur Lee Burns, "From Balance to Deterrence: A Theoretical Analysis," *World Politics*, 9 (July, 1957), 495.

<sup>14</sup> Kaplan, *System and Process*, preface to the 1964 Wiley Science Edition.

<sup>15</sup> *Ibid.*

<sup>16</sup> Heuristic, of course, does not mean general; it means serving to discover. Professor Kaplan is not alone, however, in apparently equating the two terms.

<sup>17</sup> Kaplan, *System and Process*, 1964 preface.

<sup>18</sup> *Ibid.*, p. 24, and Morton A. Kaplan, "Some Problems in International Systems Research," in *International Political Communities: An Anthology* (Garden City: Doubleday Anchor Books, 1966), p. 471.

would represent a confirmation of the predictions of the theory."<sup>19</sup> One does not have to share Professor Kaplan's positivism to be intrigued by the question of whether or not historical data fit his predictions about modal alliance behavior in a balance of power system.

In such a system, Kaplan states, alliances are fluid and flexible, they are made for instrumental and not ideological reasons, and actors are indifferent about whom their alliance partners are.<sup>20</sup> These characteristics flow from the operation of the six essential rules of such a system, and they are similar to the views of many other authors on this topic. Kaplan's contribution lies in his prediction that, *when viewed from the perspective of the system*, alliances are equiprobable and time-independent: "the 'balance of power' system postulates that any alignment is as probable as any other alignment prior to a consideration of the specific interests which divide nations. Moreover, any particular alignment should not predispose the same nations to align themselves with each other at the next opportunity."<sup>21</sup> We read Professor Kaplan as saying that the alliance formation process in a balance of power system is a *stochastic process*. That is, in a balance of power system alliances occur from time to time, and these events over time are subject to probability laws because the past behavior of the alliance process has no influence on future behavior. If we have a process in which "the future is independent of the past" we have a purely random or stochastic process.<sup>22</sup> Kaplan specifically indicates that he is thinking in such probabilistic terms when he uses the metaphor of the behavior of molecules in a tank of gas to characterize balance of power alliance politics.<sup>23</sup>

<sup>19</sup> Kaplan, *System and Process*, p. 24.

<sup>20</sup> *Ibid.*, pp. 66, 115-116.

<sup>21</sup> *Ibid.*, preface.

<sup>22</sup> D. V. Lindley, *Introduction to Probability and Statistics from a Bayesian Viewpoint, Part I, Probability* (Cambridge: Cambridge University Press, 1965), p. 67.

<sup>23</sup> The metaphor, which comes in the middle of Kaplan's discussion of the operation of balance of power, is worth quoting in full. "Just as any particular molecule of gas in a gas tank may travel in any direction, depending upon accidental bumpings with other molecules, particular actions of national actors may depend upon chance or random conjunctions. Yet just as the general pattern of behavior of gas may represent its adjustment to pressure and temperature conditions within the tank, the set of actions of national actors may correspond to the essential rules of the system when the other variables take the appropriate specified values.

Thus, by shifting the focus of analysis from the particular event to the pattern of events, seemingly unique or accidental occurrences become part of a meaningful pattern of occurrences. In this way the historical loses its quality of uniqueness and is translated into the uni-

**Hypotheses.** Now, there are many probability models of stochastic processes,<sup>24</sup> but of all processes that generate random variables, *this property of independence of past history is unique to the Poisson process*.<sup>25</sup> Poisson processes are also stationary stochastic processes, meaning that the origin of time is irrelevant.<sup>26</sup> This accords with Kaplan's theory because it implies that at whatever point in time new decision makers or new states enter the alliance process, they too are free of the past. Finally, in a balance of power system, alliances must be formed from time to time but it is predicted that they must not be so infrequent as hardly ever to occur or so frequent as to make rational calculations of state interest impossible.<sup>27</sup> Thus, alliances are *relatively rare events*, and it is a Poisson process that generates the distribution of such rare events over time.<sup>28</sup>

We therefore hypothesize, following Kaplan, that

H1: *in a balance of power international system, the occurrence of alliances will be stochastically distributed* (the number of alliances formed per unit of time is a Poisson random variable), and

H2: *in a balance of power international system, the time intervals between alliances are randomly distributed* (the distribution of interalliance intervals is a negative exponential random variable).

Because nineteenth-century Europe represents an

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versal language of science" (*System and Process*, p. 25).

Professor Richard E. Hayes of C.A.C.I., Inc. disagrees with our reading of Kaplan's example, arguing in a personal communication that the metaphor refers to the limited predictability of social systems in general. Our disagreement illustrates the difficulties involved in deriving falsifiable hypotheses from verbal "theories" and "models" such as Kaplan's.

<sup>24</sup> An authoritative survey is given by William Feller, *An Introduction to Probability Theory and Its Applications*, 3rd ed. (New York: Wiley, 1968).

<sup>25</sup> Discussions of the Poisson distribution and the uniqueness of Poisson processes from a variety of perspectives are presented by: Howard Raiffa and Robert Schlaifer, *Applied Statistical Decision Theory* (Cambridge, Mass.: MIT Press, 1961), pp. 275-276; Oscar Kempthorne and Leroy Folks, *Probability, Statistics, and Data Analysis* (Ames: The Iowa State University Press, 1971), pp. 199-200; James S. Coleman, *Introduction to Mathematical Sociology* (Glencoe: The Free Press, 1964), pp. 288-289; Lindley, *Introduction to Probability and Statistics*, pp. 63-73; Richard E. Hayes, "Identifying and Measuring Changes in the Frequency of Event Data," *International Studies Quarterly*, 17 (December, 1973), 471-493.

<sup>26</sup> Lindley, *Introduction to Probability and Statistics*, p. 68.

<sup>27</sup> Kaplan, *System and Process*, p. 35.

<sup>28</sup> Karl Schuessler, *Analyzing Social Data: A Statistical Orientation* (New York and Boston: Houghton Mifflin Co., 1971), p. 412; Kempthorne and Folks, *Probability, Statistics and Data Analysis*, p. 91.

empirical instance of a balance of power system, data on alliance behavior between 1814 and 1914 permit a direct empirical test of these two hypotheses inferred from Kaplan's theory.<sup>29</sup>

These two hypotheses about alliance behavior in balance of power systems can be thought of as characterizing the system while it is in an equilibrium state; that is, when it is operating according to Kaplan's six rules. Kaplan's theory is also concerned with the conditions under which one type of international system changes into another type. Conventionally, we think of the nineteenth-century European balance of power system as having broken down in World War I and as having been replaced by a new worldwide system featuring the League of Nations. Kaplan tries to identify factors that lead to changes in balance of power systems.

He identifies a number of "parameters" whose values, if they change, can lead to changes in system structure.<sup>30</sup> A key parameter is the "activity rate of a system," which is a cause of system flexibility.<sup>31</sup> An instance of this parameter in an empirical balance of power system would be the rate of alliance formation among essential system actors as measured over time for the system as a whole. The six essential rules of a balance of power system imply a pattern of fluid and moderately frequent alliances.<sup>32</sup> If these rules are violated, either by a rigid enmity, such as existed between France and Germany after the annexation of Alsace Lorraine, or by a decline in the systemic alliance formation rate,<sup>33</sup> then a loss of system flexibility will result and system-changing events are likely. We have set aside Kaplan's rigidity hypothesis for subsequent research and in this paper examine his prediction that

H3: in a balance of power international system, a decline in the systemic rate of alliance formation precedes system changing events, such as general war.

This hypothesis agrees with considerable speculation by historians about the origins of World War I and it can also be tested against data on nineteenth-century European alliance politics.

**Previous Research.** Published studies that empirically and systematically test propositions deduced from Professor Kaplan's heuristic theory are extremely rare. Several studies have used his concepts to describe historical interstate systems

—Chi on the Chinese warlord system and Franke on the Italian city-state system<sup>34</sup>—but description is not hypothesis testing. One recent study by Healy and Stein does test Kaplan's rule 4 and part of his rule 6 against historical data on balance of power politics in the short interval 1870–1881. They find no support for Kaplan's rules, however.<sup>35</sup>

The study of historical alliance patterns has been conceptual and analytical, with few hypothesis tests.<sup>36</sup> Singer and Small, however, have made major contributions to our existential and correlational knowledge of historical alliances in their "Correlates of War" project.<sup>37</sup> The possibilities of theoretically informed research on alliances are illustrated in the recent volume by Holsti, Hopmann, and Sullivan<sup>38</sup> and in the most recent work of the "Correlates of War" project.<sup>39</sup>

There have been a few applications of stochastic models to international political phenomena, but only four that apply such models to alliance behavior. The first application of a Poisson model to international politics was that of Lewis Fry Richardson, which was extended by J. R. Moyal.<sup>40</sup> As discussed by Richardson, both authors demon-

<sup>34</sup> Hsi-sheng Chi, "The Chinese Warlord System as An International System," pp. 405–425 and Winfried Franke, "The Italian City-State System as an International System," pp. 426–458, both in *New Approaches to International Relations*, ed. Morton A. Kaplan (New York: St. Martin's Press, 1968).

<sup>35</sup> Brian Healy and Arthur Stein, "The Balance of Power in International History: Theory and Reality," *Journal of Conflict Resolution*, 17 (March, 1973), 33–61.

<sup>36</sup> See Julian Friedman et al., eds., *Alliance in International Politics*, for a survey of this literature.

<sup>37</sup> J. David Singer and Melvin Small, "Formal Alliances, 1815–1939: A Quantitative Description," *Journal of Peace Research*, 3/1 (January 1966), 1–32; "Alliance Aggregation and the Onset of War, 1815–1945," in *Quantitative International Politics: Insights and Evidence*, ed. J. D. Singer (New York: The Free Press, 1968), pp. 245–286; "National Alliance Commitments and War Involvement, 1815–1945," *Peace Research Society (International) Papers*, 5 (1966), 109–140; and "Formal Alliances, 1816–1965: An Extension of the Basic Data," *Journal of Peace Research*, No. 3, (1969), 257–282.

<sup>38</sup> O. R. Holsti, P. T. Hopmann and J. D. Sullivan, *Unity and Disintegration in International Alliances: Comparative Studies* (New York: Wiley, 1973).

<sup>39</sup> Bruce Bueno de Mesquita and J. D. Singer, "Alliances, Capabilities, and War: A Review and Synthesis," in *Political Science Annual*, ed. C. P. Cotter (Indianapolis: Bobbs-Merrill, 1973), IV, 237–280 and Michael Wallace, "Alliance Polarization, Cross-Cutting, and International War, 1815–1964: A Measurement Procedure and Some Preliminary Evidence," *Journal of Conflict Resolution*, 17 (December, 1973), 575–604.

<sup>40</sup> Lewis Fry Richardson, "The Distribution of Wars in Time," *Journal of the Royal Statistical Society*, 107 (1945), 242–250; J. R. Moyal, "The Distribution of Wars in Time," *Journal of the Royal Statistical Society, Series A*, 112 (1949), 446–449.

<sup>29</sup> G. U. Yule and M. G. Kendall, *An Introduction to the Theory of Statistics*, 14th rev. and enlarged ed. (New York: Hafner, 1950), p. 169.

<sup>30</sup> Kaplan, *System and Process*, preface and pp. 8, 35–36, 54–85.

<sup>31</sup> *Ibid.*, p. 74.

<sup>32</sup> *Ibid.*, pp. 35, 125.

<sup>33</sup> *Ibid.*, pp. 27–29, 74.

strated that Richardson's data on wars between 1820 and 1929 and Wright's data on wars between 1500 and 1931 were Poisson-distributed over time.<sup>41</sup> Much more recently, Singer and Small have demonstrated that the intervals between the outbreaks of all international wars and all interstate wars between 1816 and 1965 fit the negative exponential Poisson density function.<sup>42</sup> A further application of the Poisson model is Midlarsky's demonstration that coups in Latin America (1935-64) and Africa (1963-67) were Poisson-distributed over time.<sup>43</sup> Finally, another stochastic process model, Markov chains, has been applied to foreign conflict behavior 1955-1960,<sup>44</sup> and to the outbreak of World War I.<sup>45</sup>

The first application of a probability model to alliance behavior was by Horvath and Foster, who showed that Richardson's data on the size of wartime alliances fit a Yule distribution. This is consistent with the hypothesis that nations join alliances at a rate proportional to the number of nations in alliances of that size and that such alliances dissolve whenever a single member leaves.<sup>46</sup> Rood, who used the same data that we present in this paper, found that choice of alliance partner in nineteenth-century Europe was approximated by a probability model of random choice in voting bodies developed by Brams and O'Leary.<sup>47</sup> Brian Job has tested the hypothesis that alliance formation is a random process with a constant formation rate by applying a Poisson model to Singer and Small's data on 178 formal international alliances between 1815 and 1965. Despite imaginative treatment of the data, Job was unable to confirm

his hypothesis except for all alliances, ententes and defense pacts between 1871 and 1914, and for all alliances and defense pacts from 1914 to 1939.<sup>48</sup> Finally, Siverson and Duncan have applied three different stochastic models (the Poisson, the contagious Poisson, and the Yule-Greenwood heterogeneity model) to the same Singer and Small alliance data set. Unlike Job's paper and this article, Siverson and Duncan do not present hypothesis tests because their objective was simply to examine long-run patterns in the initiation of alliance activity. They find that for the 1815-1914 period both the Poisson process model ( $\chi^2 = .76$ ,  $df = 1$ ,  $p = .40$ ) and the contagious model ( $\chi^2 = .00$ ,  $df = 1$ ,  $p > .99$ ) fit the data, a finding that contradicts Job.<sup>49</sup>

To our knowledge, these are the only applications of stochastic models, including the Poisson, to historical international political behavior. It is encouraging that certain central phenomena such as wars and the size of wartime coalitions can be described by stochastic process models. This suggests that other basic international behavior, such as alliances, may also be generated by such processes. It is discouraging to note the conflicting findings of Job and of Siverson and Duncan and the fact that except for the studies by Job, Rood, and Horvath and Foster, applications of stochastic models have not been related to hypothesis tests. Even in these three exceptions, the hypotheses tested were not inferred from an explicit theory of international politics. We therefore believe that this paper can break new ground in research on alliance behavior in balance of power systems and that it can make a contribution to positive theory building in international politics by testing three hypotheses inferred from Professor Kaplan's well known heuristic theory of system and process in international politics.

### Data Making

In order to evaluate our hypotheses, a data set on nineteenth-century European alliance behavior was created. The procedure used was Poisson sampling, which "consists of observing the process over a predetermined amount of time, length or other dimension, and counting the number of events which occur. . . ."<sup>50</sup> In Poisson sampling the time dimension ( $t$ ) is predetermined,

<sup>41</sup> Lewis Fry Richardson, *Statistics of Deadly Quarrels* (Chicago: Quadrangle Books, 1960), pp. 128-142.

<sup>42</sup> J. David Singer and Melvin Small, *The Wages of War, 1816-1965: A Statistical Handbook* (New York: John Wiley, 1972), pp. 205-206.

<sup>43</sup> Manus Midlarsky, "Mathematical Models of Instability and a Theory of Diffusion," *International Studies Quarterly*, 14 (March, 1970), 60-84.

<sup>44</sup> Jonathan Wilkenfeld, "Models for the Analysis of Foreign Conflict Behavior of States," in *Peace, War and Numbers*, ed. B. M. Russett (Beverly Hills: Sage Publications, 1972), pp. 275-298 and Dina A. Zinnes and Jonathan Wilkenfeld, "An Analysis of Foreign Conflict Behavior of Nations," in *Comparative Foreign Policy*, ed. W. F. Hanrieder (New York: David McKay, 1971), pp. 167-213.

<sup>45</sup> Dina A. Zinnes, J. L. Zinnes and R. D. McClure, "Hostility in Diplomatic Communication: A Study of the 1914 Crisis," in *International Crises*, ed. C. F. Hermann (New York: The Free Press, 1972), pp. 139-162.

<sup>46</sup> W. J. Horvath and C. C. Foster, "Stochastic Models of War Alliances," *Journal of Conflict Resolution*, 7 (June, 1963), 110-116.

<sup>47</sup> Robert M. Rood, "Agreement in the International System," (Ph.D. dissertations in Political Science, Syracuse University, 1973); Steven J. Brams and Michael O'Leary, "An Axiomatic Model of Voting Bodies," *American Political Science Review*, 64 (June, 1970), 449-470.

<sup>48</sup> Brian Job, "Alliance Formation in the International System: The Application of the Poisson Model," a paper presented at the Annual Meeting of the International Studies Association, Americana Hotel, New York (March 13-17, 1973).

<sup>49</sup> Randolph M. Siverson and G. T. Duncan, "Stochastic Models of International Alliance Initiation, 1815-1965," Department of Political Science University of California, Davis, mimeo, n.d. [1973?], p. 12.

<sup>50</sup> Raiffa and Schlaifer, *Applied Statistical Decision Theory*, p. 283.

in the present case  $t$  represents the one hundred and one year period from January 1814 to August 1914. On the other hand, in Poisson sampling the number of events ( $x$ ) that occur is left to chance. In the present study  $x$  represents an alliance between two or more core European powers and  $\bar{x}$  is the distribution of these fifty-five alliances across the  $t$  dimension. Raiffa and Schlaifer prove that if the number of years preceding the  $x$ th alliance is  $t$  or less, the conditional distribution of  $\bar{x}$  given  $t$  and the intensity of the alliance formation process,  $m$ , is provided by the Poisson function:<sup>51</sup>

$$P\{\bar{x} | t, m\} = f_P(x | m) = \frac{e^{-m}(m)^x}{x!},$$

$$x = 0, 1, 2, \dots \quad (1)$$

$$m > 0.$$

Thus, in undertaking a Poisson sampling data collection strategy, we have made it possible to analyze the distributional characteristics of our data by application of the well-known theoretical Poisson distribution and its variants.<sup>52</sup>

Our data-making strategy was similar to the events data approach,<sup>53</sup> but as applied to diplomatic histories rather than current events chronologies. A similar data-making strategy is being used by Rosecrance in his Situational Analysis Project at Cornell.<sup>54</sup> Following our detailed ab-

stracting and coding rules,<sup>55</sup> we read nine authoritative and representative diplomatic histories and recorded each instance of alliance behavior reported by at least one historian to have occurred between January 1814 and August 1914.<sup>56</sup>

Operationally, the events ( $x$ ) we call alliances are defined as all commitments, formal and informal, for the use or nonuse of military force (a) that were made between 1 January 1814 and 29 July 1914 by Great Britain, France, Prussia-Germany, Austria-Hungary, or Russia and at least one other power including those mentioned and (b) that had as their target the behavior in Europe of Great Britain, France, Prussia-Germany, Austria-Hungary, Russia or any grouping of these five states. Our sources usually reported formal commitments by citing a treaty, such as the 1834 Quadruple Alliance of Great Britain, France, Spain, and Portugal against Prussia, Russia, and Austria, who were joined by the Munchengrätz Agreement of 1833. Informal alliances were most often embodied in the exchange of diplomatic notes or even verbal agreements by ambassadors to temporary joint fleet demonstrations such as that made in October, 1849, by Britain and France against Russia in support of Turkey.<sup>57</sup> Following this definition and our coding rules, fifty-five alliances were observed during the 1814-1914 time period. Twenty-eight of these alliances are of the formal type and are also part of the Singer and Small alliance data set.<sup>58</sup> The remaining twenty-seven are informal alliances and are unique to the present study. A complete listing of all fifty-five alliances is given by Rood.<sup>59</sup>

The reliability of our data collection operation

<sup>51</sup> *Ibid.*

<sup>52</sup> The best discussion of social scientific applications of the Poisson distribution is given in J. S. Coleman, *Introduction to Mathematical Sociology*, pp. 288-380.

<sup>53</sup> Discussions of aspects of events data making are given in: Edward Azar, "Analysis of International Events," *Peace Research Reviews*, 4, No. 1 (1970); Edward Azar, R. A. Brody, and C. A. McClelland, *International Event Interaction Analysis: Some Research Considerations*, Sage Professional Paper in International Studies 02-001 (Beverly Hills and London: Sage Publications, 1972); C. F. Hermann, "What is a Foreign Policy Event?" pp. 295-321 in *Comparative Foreign Policy*, ed. Hanreider; P. M. Burgess and R. W. Lawton, *Indicators of International Behavior: An Assessment of Events Data Research*, Sage Professional Paper in International Studies 02-010 (Beverly Hills and London: Sage Publications, 1972); and P. J. McGowan, "A Bayesian Approach to the Problem of Events Data Validity," pp. 407-433 in *Comparing Foreign Policies*, ed. J. N. Rosenau (New York: Halsted Press (a Sage Publications Book), 1974).

<sup>54</sup> Brian Healy and Arthur Stein, "The Balance of Power in International History: Theory and Reality," Ronald Goodman, Jeff Hart, and Richard Rosecrance, "Testing International Relations Theory: Methods and Data in a Situational Analysis of International Politics," Ithaca: Cornell University Situational Analysis Project Paper No. 2, mimeo, January, 1970; and Jeff Hart, "Symmetry and Polarization in the European International System: 1870-1879," Ithaca: Cornell University Situational Analysis Project Paper No. 3, mimeo, (1972).

<sup>55</sup> As described in Rood, "Agreement in the International System," pp. 171-175.

<sup>56</sup> Our sources were: René Albrecht-Carrié, *A Diplomatic History of Europe Since the Congress of Vienna* (New York: Harper, 1958); E. V. Gulick, *Europe's Classical Balance of Power* (New York: W. W. Norton, 1955); H. A. Kissinger, *A World Restored: Metternich, Castlereagh and the Problems of Peace, 1812-22* (Boston: Houghton Mifflin, 1957); W. L. Langer, *European Alliances and Alignments, 1871-1890*, 2nd ed. (New York: Random House-Vintage Books, 1964); Andrei Lobanov-Rostovsky, *Russia and Europe 1825-1878* (Ann Arbor: George Whar Publishing, 1954); R. B. Mowat, *The European States System: A Study of International Relations*, 2nd ed. (London: Oxford University Press, 1929); L. C. B. Seaman, *From Vienna to Versailles* (New York: Harper, 1963); R. W. Seton-Watson, *Britain in Europe, 1789-1914* (New York: Howard Fertig, 1968); and A. J. P. Taylor, *The Struggle for Mastery in Europe 1848-1918* (Oxford: The Clarendon Press, 1954).

<sup>57</sup> Taylor, *The Struggle for Mastery in Europe*, pp. 2-3, 34-35.

<sup>58</sup> As described in Singer and Small, "Formal Alliances, 1815-1939," and "Formal Alliances, 1816-1965."

<sup>59</sup> Rood, "Agreement in the International System," pp. 183-201. This dissertation is available from University Microfilms in Ann Arbor, Michigan.



cannot be assessed by the usual means of an inter-coder reliability coefficient because all coding decisions were made by one author. Our data can be easily replicated, however, by consulting the methodological appendices of Rood's dissertation.<sup>60</sup> The use of multiple sources was our strategy for achieving an acceptable level of reliability in the alliance counting process. Most of the fifty-five alliances, 74 per cent, are cited by two or more authorities. The average number of references per alliance is nearly three. Only fourteen alliances were mentioned by just one diplomatic historian. Finally, the nine histories we used were taken from the most frequently cited authorities used by the "Correlates of War" Project.<sup>61</sup> We therefore doubt that we have included in our data set an alliance that in fact did not occur in nineteenth-century Europe. We may have failed, however, to include some alliances, particularly the informal type, that did in fact take place between 1814 and 1914. This possibility can only be discounted after our data set has been independently replicated.

The validity of our data set for the purposes of this paper is enhanced by the fact that we ask only two simple questions of the data: (1) did an alliance commitment consistent with our operational definition occur? and (2) in what year did it occur? In this paper we do not engage in the difficult task of attempting to measure such things as the scope, level of commitment, or duration of alliances. The validity issue as it concerns this paper thus reduces to the question of whether or not our operational definition of alliance is suitable for the study of alliance behavior in balance of power systems.

Our assertion that nineteenth-century Europe was a balance of power international system is supported by the fact that thirteen out of fourteen scholars surveyed, including Morton A. Kaplan, claim that it was.<sup>62</sup> Only Rosecrance does not accord the term balance of power to the nineteenth-century European state system.<sup>63</sup> Our decision to study only the alliance formation behavior of the five greatest European powers is validated by the theoretical argument presented earlier that it is the essential or great powers that operate a balance of power system and that these five states were the major European powers of the nineteenth century.<sup>64</sup> Our focus on alliances di-

rected at the European behavior of one or more of these five states derives from the fact that the balance of power system we are studying is the system of Europe from 1814 to 1914 and not the incipient worldwide system that emerged as a result of World War I. According to Singer and Small, extra-European powers achieved great-power status only very late in the 19th century—Japan in 1895 and the United States in 1899.<sup>65</sup> Finally, we have included informal as well as formal alliances because a focus on written commitments would exclude much of the nineteenth-century "balancing" behavior of Great Britain, a power regarded by many authorities as central to the operation of the European balance of power system,<sup>66</sup> and because there is nothing in Kaplan's theory to suggest otherwise.

Figure 1 presents our alliance formation data arrayed by five-year intervals for the entire nineteenth century. The first five years, which included the Congress of Vienna, saw the greatest amount of alliance formation by the five great powers. These relations held for the next period, when no new alliances were formed. In 1825 there began a fifty-year period of moderate levels of alliance formation that included the Crimean War and the unifications of Italy and Germany. The Bismarck era then saw a high level of alliance behavior that continued until 1909, the year of our last observed alliances. The five years preceding the outbreak of World War I in August 1914 saw no new alliances among the five great European powers. The mean rate of alliance formation by five-year periods is 2.75 with a moderate standard deviation of 1.94. As the figure suggests, formal and informal alliances co-occur. The product moment correlation coefficient ( $r$ ) between the two types of alliances by five year period is 0.43 ( $p \leq .05$  in a two-tailed test).

### The Poisson Distribution

We shall use the Poisson distribution in two distinct fashions in this paper. First, we shall compare the fit of our historical data to Poisson-derived distributions that predict the number of alliances formed over time and the intervals between alliances. Second, we shall use the Poisson

<sup>60</sup> *Ibid.*, pp. 171–202.

<sup>61</sup> Singer and Small, "Formal Alliances, 1815–1939," pp. 24–27.

<sup>62</sup> Rood, "Agreement in the International System," pp. 64–67.

<sup>63</sup> R. N. Rosecrance, *Action and Reaction in World Politics* (Boston: Little, Brown, 1963), pp. 239–256.

<sup>64</sup> Supporting this argument are: Gulick, *Europe's Classical Balance of Power*, p. 4; Morgenthau, *Politics Among Nations*, p. 201; J. David Singer and Melvin

Small, "National Alliance Commitments and War Involvement, 1815–1945," in *International Politics and Foreign Policy*, ed. J. N. Rosenau, 2nd ed. (New York: The Free Press, 1969), p. 515.

<sup>65</sup> Singer and Small, "National Alliance Commitments . . .," p. 515 in the Rosenau reader.

<sup>66</sup> Albrecht-Carrié, *A Diplomatic History of Europe*, pp. 9–17; Taylor, *The Struggle for Mastery in Europe*, p. 54; Seton-Watson, *Britain in Europe*, p. 312; and Ludwig Dehio, *The Precarious Balance: Four Centuries of the European Power Struggle* (New York: Random House-Vintage Books, 1962), *passim*.

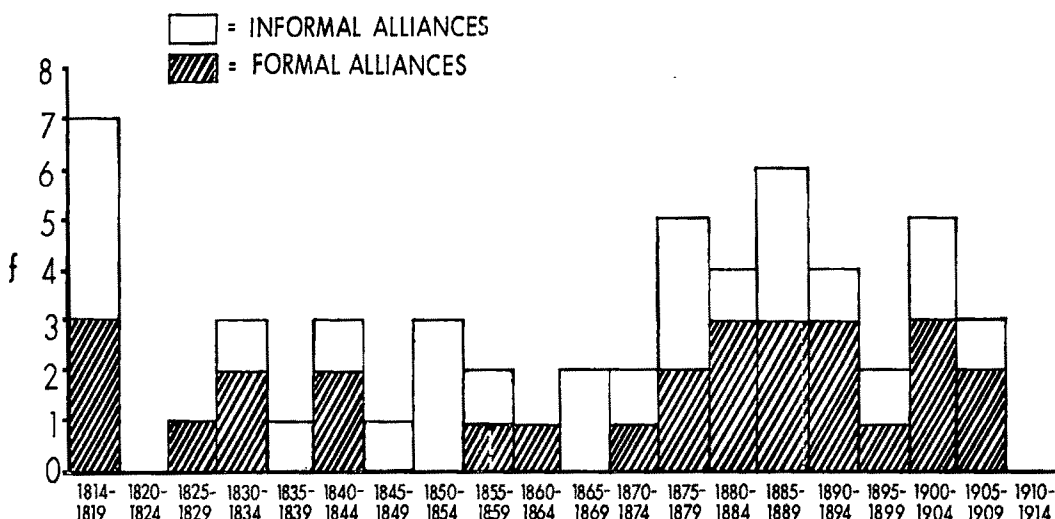


Figure 1. Frequency Histogram of Alliance Formation in 19th Century Europe by Five-Year Period

distribution to compute rates of alliance formation in nineteenth-century Europe.

The Poisson distribution, one of "a few distributions of great universality which occur in a surprisingly great variety of problems,"<sup>67</sup> is discussed in most texts on probability and mathematical statistics.<sup>68</sup> A comprehensive treatment of the Poisson distribution is given by Haight, including many applications, and values of the distribution for given means ( $m$ ) are tabulated by Kitagawa.<sup>69</sup> The applicability of the Poisson distribution and its variants to social science questions is extensively discussed with numerous examples by Coleman.<sup>70</sup>

Following Lindley,<sup>71</sup> the aspects of Poisson process theory relevant to our paper can be briefly outlined. If in a period of time  $(0, t)$   $A$  events occur and in a period  $(t, t+h)$   $B$  events occur and  $A$  and  $B$  are independent so that  $p(B|A) = p(B)$ , this not depending upon  $t$ , the process is said to be a

purely random stationary process, or a *Poisson process*. In a Poisson process the following theorems hold:<sup>72</sup>

- (1) the probability of no events in a fixed interval of length  $t$  is—

$$p_0(t) = e^{-mt}, \text{ where } m > 0 \quad (2)$$

- (2) the density of time between any two events ( $x_1$  and  $x_2$ ) is given by—

$$f_1(x) = me^{-mx}, \text{ when } x \geq 0 \quad (3)$$

- (3) if  $t$  is any fixed number, the probability of  $x$  events in a fixed interval of length  $t$  is—

$$p_x(t) = \frac{e^{-m}(m)^x}{x!} \quad (4)$$

- (4) the expected number of events ( $x$ ) in a time interval of length  $t$  is  $m$  (5)

- (5) the expectation of time up to the  $x$ -th event,  $\bar{t}_x$ , is  $x/m$  (6)

The key *parameter* in a Poisson process is seen to be  $m$ , which equals  $\lambda t$  when  $t$  is one. The proper physical interpretation of  $m$ , given theorem 4, is that it is the expected number of alliances per unit of time, i.e., the *rate* of formation. Conversely, from theorem 5, when  $x=1$ , the expected or average time between successive alliances is  $1/m$ . Theorem 2 states that the interalliance intervals are independently distributed negative exponential random variables. This theorem will be used to test H2: that in a balance of power system, the time interval between alliances is randomly dis-

<sup>67</sup> Feller, *An Introduction to Probability Theory*, p. 156, who argues that the three principal distributions are the binomial, the normal, and the Poisson, a point agreed to by Yule and Kendall, *An Introduction to the Theory of Statistics*, p. 169.

<sup>68</sup> Feller, pp. 153-164; Lindley, *An Introduction to Probability and Statistics from a Bayesian Viewpoint*, pp. 63-74; William Mendenhall and Richard L. Scheaffer, *Mathematical Statistics with Applications* (North Scituate, Mass.: Duxbury Press, 1973), pp. 81-85; Raiffa and Schlaifer, *Applied Statistical Decision Theory*, pp. 221-222, 275-289; and Yule and Kendall, pp. 189-194.

<sup>69</sup> F. A. Haight, *Handbook of the Poisson Distribution* (New York: Wiley, 1967); T. Kitagawa, *Tables of Poisson Distribution* (Tokyo: Baifukan, 1952).

<sup>70</sup> Coleman, *An Introduction to Mathematical Sociology*, pp. 288-311.

<sup>71</sup> Lindley, *An Introduction to Probability and Statistics*, pp. 63-73.

<sup>72</sup> We give only the theorems presented by Lindley relevant to this paper.

tributed. Theorem 3 states that the number of alliances per unit of time is a Poisson random variable. This theorem will be used to test H1: that in a balance of power system, the occurrence of alliances will be randomly distributed.

The standard statistical test for comparisons between Poisson-based predicted distributions and empirical alliance distributions is the Chi-square goodness of fit test as described by Pearson and Hartly and by Yule and Kendall.<sup>73</sup> The null hypothesis is that the observed distribution does not significantly depart from the expected Poisson-based distribution.<sup>74</sup> A second test of goodness of fit is based on the fact that in a Poisson distribution,  $\sigma^2 = m$ , the variance equals the mean, a unique feature of Poisson distributed data.<sup>75</sup>

In a recent article, Hayes has discussed applications of Poisson theory to the measurement of changes in the frequency of international political phenomena.<sup>76</sup> He presents a Poisson-based comparison technique that permits one to say, for example, how likely it is that eleven alliances occurred in the period  $(t, t+h)$ , given that just seven happened between 0 and  $t$ . This technique can be directly applied to test H3: that, in a balance of power system, a decline in the systemic rate of alliance formation precedes system-changing events, such as general war. In this application of Hayes's measurement routine to our data, one calculates the rate of alliance formation by specified time intervals over the century between 1814–1914 and then calculates the probability of observing  $x$  alliances in each interval under the assumption that the observed  $x$  represents a decline from the average rate  $m$  (H1:  $x < m$ ). These probabilities then become one's measures of balance of power system flexibility over time.

We conclude this discussion of the Poisson distribution by noting Coleman's belief that it is particularly appropriate for the analysis of sociopolitical phenomena because the Poisson distribution does not assume continuous level measurement; because a Poisson process occurs continuously over time rather than at discrete "trials" and

thus is readily applied to naturally occurring events like alliances; and because the Poisson process is a rational model whose assumptions can mirror our assumptions about actual phenomena. Moreover, when its assumptions are met, a Poisson distribution is not an approximation to data; "it is the exact distribution and any other becomes an approximation."<sup>77</sup> We believe our research problem and our data fit the assumptions of the Poisson model, especially in that alliance formation in a balance of power international system is said by Kaplan to be based on rational calculations of costs and benefits.<sup>78</sup>

### Empirical Results

Let us assume that in a balance of power system that is in equilibrium the decision makers of each essential actor have a propensity to form alliances with and against other essential actors that is rationally designed to keep the system in equilibrium. Let this alliance propensity be denoted by  $\alpha$  and let us additionally assume that each essential actor has about the same propensity. Then, if there are  $N$  actors in the system, the systemic propensity to form alliances during a fixed period of time,  $t$ , will be  $\alpha N$ . When our data on alliance formation frequencies among the five great European powers between 1814 and 1914 are arrayed as in Table 1,  $\alpha N = m = 0.545$ . That is, one alliance was formed about every two years, or 0.545 alliances per year; this is the average rate of alliance formation in Europe in the nineteenth century when  $t$  equals one year. Therefore,  $\alpha = 0.545/5 = 0.109$ , is our estimated actor propensity to form alliances. In applying the Poisson distribution to our data we therefore assume that  $m$  is constant throughout the nineteenth century, and by implication since  $N$  is constant,  $\alpha$  was constant as well.

Table 1 represents our test of H1: that in a balance of power international system, the occurrence of alliances will be stochastically distributed over time. The evidence in the table strongly supports this hypothesis, for all alliances and for both formal and informal alliances. The Poisson expected frequencies are very close to observed frequencies; the Chi-square values indicate that in all three experiments more than 50 per cent to 90 per cent of comparable observations would show worse agreement; and the means and variances of the three empirical distributions are remarkably close. Table 1 indicates that formal and informal alliances are homogeneous, that they are part of the same alliance formation process. Moreover, since the alliances are Poisson distributed, we may conclude that they were generated by a Poisson process because "not only does

<sup>73</sup> E. S. Pearson and H. O. Hartley, *Biometrika Tables for Statisticians, Volume I*, 3rd ed. (Cambridge: Cambridge University Press, 1966), pp. 11–12; Yule and Kendall, pp. 469–477.

<sup>74</sup> That is, only when  $X^2$  is large, so that for a given degree of freedom its probability is less than .10 or .05, is the null hypothesis rejected and the inference made that the observed distribution was *not* generated by a Poisson process. This is not a very conservative procedure. It unfortunately leaves open the possibility of Type II error, i.e., the inference that the null hypothesis prevails when in fact the alternate hypothesis is correct.

<sup>75</sup> Kempthorne and Folks, *Probability, Statistics, and Data Analysis*, p. 92.

<sup>76</sup> Richard E. Hayes, "Identifying and Measuring changes in the Frequency of Event Data," *International Studies Quarterly*, 17 (December, 1973), 471–493.

<sup>77</sup> Coleman, *An Introduction to Mathematical Sociology*, pp. 291, 299.

<sup>78</sup> Kaplan, *System and Process*, 1964 preface.

Table 1. The Formation of Alliances in Europe, 1814–1914, as a Poisson Distribution

Experiment <sup>a</sup>	Years with $x$ Alliances				$\chi^2$ <sup>b</sup>	$p$	$m$	$s_x^2$
	$x=0$	$x=1$	$x=2$	$x \geq 3$				
All Alliances:								
Observed $Nx$	61	31	6	3			.545	.501
$Np(x; 0.545)$	58.6	31.9	8.7	1.8	.338	> .50		
"Formal" Alliances:								
Observed $Nx$	77	21	2	1			.277	.299
$Np(x; 0.277)$	76.6	21.2	2.9	0.3	.019	> .80		
"Informal" Alliances:								
Observed $Nx$	78	21	3	0			.267	.256
$Np(x; 0.267)$	77.3	20.6	2.8	0.3	.012	> .90		

<sup>a</sup> Probabilities of the Poisson distribution,  $p$  in  $Np$ , determined from T. Kitagawa, *Tables of Poisson Distribution* (Tokyo: Baifukan, 1952), pp. 13–14, 25.  
<sup>b</sup> The degree of freedom for Chi Square in each test is one.

the process yield the distribution but the distribution, with mean proportional to the length of the interval, can only arise from the process.”<sup>79</sup>

Table 2 presents our test of H2: that in a balance of power international system, the time intervals between alliances are randomly distributed. As the table indicates, the negative exponential Poisson distribution provides a good fit to the data when intervals of six months are the time units. The  $\chi^2$  value of 4.74 does not permit us to reject the null hypothesis that the two distribu-

tions are similar. More than 30 per cent of comparable observations would be expected to show a worse fit. Hypothesis 2 is therefore confirmed. We note, however, that the observed frequencies of less than six months and between thirty-six and forty-two months are somewhat higher than expected. This tendency for alliances in nineteenth-century Europe to be formed quickly upon one another or with a lag of about three and one-half years deserves further study.

In order to test H3: that in a balance of power international system a decline in the systemic rate of alliance formation precedes system changing events, such as general war, we recoded our data into five-year periods as given in Figure 1. When so recoded, the number of five-year periods in which  $x$  alliances occurred ( $x \geq 0$ ), were also found to be Poisson distributed with a  $\chi^2$  value of 1.11 with one degree of freedom, thus giving a  $p > .20$ . The closeness of fit between theory and observation is illustrated in Figure 2. This finding

Table 2. Interalliance Intervals in Europe, 1814–1914, as a Poisson Distribution

Length of Time Between Alliances in Six Month Units	Observed Relative Frequency	Expected Relative Frequency
1	.315	.222
2	.148	.174
3	.093	.135
4	.074	.105
5	.074	.082
6	.056	.064
7	.167	.050
8	.000	.038
9	.018	.030
10	.000	.023
11	.018	.018
12	.000	.014
13	.000	.011
14	.018	.009
15	.000	.007
16	.000	.006
17	.000	.004
18	.018	.004
Totals	.999	.996
$\chi^2 = 4.74,$	d.f. = 4,	$p > .30$

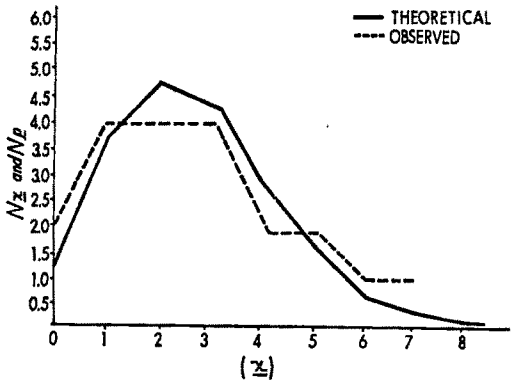


Figure 2. Theoretical and Observed Distribution of Fifty-Five 19th Century European Alliances by Five-Year Period Under the Assumption that Alliance Formation is a Poisson Process ( $m = 2.75$ )

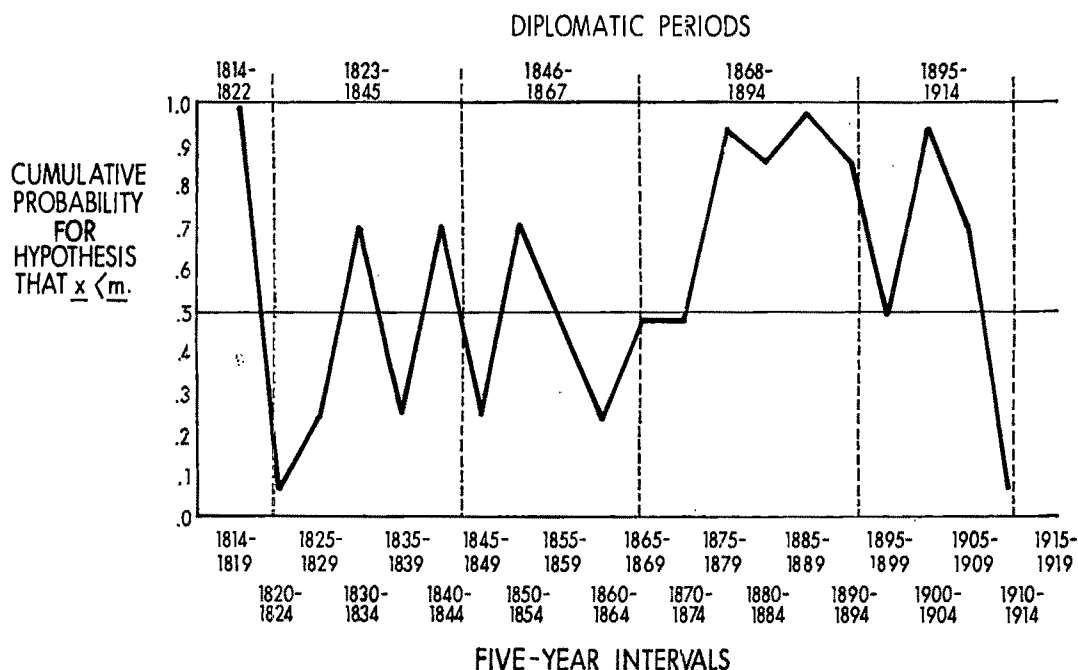


Figure 3. Poisson Based Measures of Changes in the Rate of Alliance Formation in 19th Century Europe

lends additional support to our confirmation of the first hypothesis.

Given that the five-year period average rate of alliance formation is  $m=2.75$ , we can calculate the cumulative probability that the observed number of alliances formed in each period,  $x$ , is less than  $m$ . Taking our probabilities from Kitagawa,<sup>80</sup> we have plotted and joined them in Figure 3. The vertical axis of the figure represents the cumulative probability that  $x < m$ ; as a probabilistic measure of changes in the rate of alliance formation it obviously ranges from zero to unity. Two horizontal axes are given in Figure 3. The lower one represents twenty periods of five-years length each. The upper time dimension divides the century into five diplomatic periods quite similar to Rosecrance's five nineteenth-century periods of multipolar concert (1814-22), quasi-polar truncated concert (1822-48), multipolar concert (1848-71), unipolar concert and alliances (1871-90), and bipolar concert and alliances (1890-1918)<sup>81</sup> but in fact based on Frank Denton's hostility cycles which indicate that war intensity was highest at each demarcation point.<sup>82</sup> Thus, the vertical dashed lines that separate the five periods

represent the approximate points in time at which historical evidence suggests that the system changed to a new equilibrium state (Rosecrance) and at which quantitative data suggest war was most intense (Denton).

Figure 3 supports hypothesis 3, for the cumulative probability of observing no alliances being formed in the period 1910-1914, given that the century-long average activity rate of the system was 2.75, is only .064. This is the lowest measured alliance formation rate of the century except for 1820-1824, the quiet period after the burst of diplomatic activity that concluded the Napoleonic Wars. If we take the two diplomatic periods of 1868-1894 and 1895-1914 and calculate their five-year period activity rate, which is  $m=3.3$ , the cumulative probability that  $x < m$  in 1910-1914 is just .037. Whichever way we look at it, a clear-cut decline in system flexibility occurred after 1909, and this period immediately preceded an event that destroyed the European balance of power, perhaps forever.

While we would not want to push it too far, there appears to be a serendipitous finding of some theoretical interest in Figure 3. Note that in four out of five instances of change from one diplomatic period to another a decline in the alliance formation rate occurred. Only in the change from 1846/67 to 1868/94 is the change point not crossed by a negatively sloped line. These five points represent the times at which war was most intense (Denton) and when diplomatic historians

<sup>80</sup> Kitagawa, *Tables of Poisson Distribution*, p. 65.

<sup>81</sup> Rosecrance, *Action and Reaction in World Politics*, pp. 232-266.

<sup>82</sup> F. H. Denton, "Some Regularities in International Conflict, 1820-1949," *Background*, 9 (February, 1966), 283-296. This journal is now the *International Studies Quarterly*.

point to changes in the structure of the system (Rosecrance). This finding would appear to lend further credence to our third hypothesis and it certainly merits further research.<sup>83</sup>

#### Interpretations and Conclusions

Our data strongly support our three hypotheses, and if these hypotheses have been reasonably inferred from Professor Kaplan's theory of the process of balance of power systems, we conclude that his theory has greater credibility than heretofore. The principal limitations of our study center

<sup>83</sup> This research is under way. In a recent paper we correlated our alliance flexibility scores with the Singer and Small interstate war data for the same period of time and for our five actors only. We found strong and statistically significant evidence for the hypotheses that alliance formation (hence balance of power system flexibility) is negatively associated with the occurrence of war and war magnitude, severity and intensity (R. M. Rood and P. J. McGowan, "Flexibility in Balance of Power Alliance Systems and International War," a paper delivered at the Third Annual Conference of the Southern Section of the Peace Science Society [International], Durham: Duke University, April 4-5, 1974). Our findings represent an independent replication of the well-known results of Singer and Small that alliance aggregation in the nineteenth century was negatively related to warfare; see Singer and Small, "National Alliance Commitments and War Involvement, 1815-1945," and "Alliance Aggregation and the Onset of War, 1815-1945."

on the fewness of the testable propositions we were able to derive from Kaplan, their high level of generality, and the simplicity of the data with which they were tested. Our research cannot be regarded as definitive on any of the questions it asks, but we would argue that our evidence is decisive with respect to our three hypotheses and that our paper overall presents a potentially fruitful research strategy for work on alliance behavior and international systems research.

Further research topics immediately suggest themselves. Obviously, our study should be replicated on other data sets on nineteenth-century Europe and extended to alliance behavior in other historical balance of power systems. The implications of Figure 3 about systemic change and alliance behavior and the clustering of interalliance intervals in Table 2 should be looked at. If it is true that alliances are generated by a Poisson-type process, then theoretical models that account for other Poisson-type processes, such as subatomic behavior and telephone exchange performance, might well be adapted to the study of international systems. For not only are alliances Poisson-distributed, so are wars.<sup>84</sup>

<sup>84</sup> Richardson, *Statistics of Deadly Quarrels*, pp. 128-142; Singer and Small, *The Wages of War*, pp. 205-206.

# Models, Measurement and Sources of Error: Civil Conflict in Black Africa\*

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Comparative politics has experienced both substantive and methodological progress during the past decade, but that progress has been hindered by inadequate communication between those with substantive or theoretical concerns and those with more technical interests. The literature on comparative analysis has warned of some fundamental problems which center on the notion of error and the distortions in results that error can produce. Most comparativists, however, fail to grapple with these problems. Too frequently researchers warn of possible bias but proceed as if results constitute verified knowledge. Comparativists concerned with improvement of theory or substantive knowledge must examine carefully these potential sources of error and estimate their effects on findings.

One area in political science that has experienced theoretical achievements as well as technical sophistication is the study of civil conflict, which became a major topic in cross-national research following Harry Eckstein's<sup>1</sup> plea for a consideration of general preconditions of strife rather than historical or particularistic explanations. Early empirical works focused on the dimensionality of conflict<sup>2</sup> and the testing of hypotheses relating various forms of conflict to conditions such as economic growth, land inequality, and inferred psychological states of individuals.<sup>3</sup> A body of

theoretical literature offered conjectures on the causes of conflict,<sup>4</sup> while later quantitative works moved beyond hypothesis-testing research to the development of simple causal models which reflected on the proffered theories.<sup>5</sup> Over time these models have evidenced greater substantive detail and have been evaluated with increasingly sophisticated techniques. For example, two recent but independent studies, one by Hibbs<sup>6</sup> and one by Gurr and Duvall,<sup>7</sup> have presented complex models of civil conflict employing systems of simultaneous equations to approximate more adequately the complexity of full theory.

Most of these contributions to our knowledge of conflict involve large-scale cross-national analyses of aggregate data and are hence subject to several acknowledged but rarely assessed sources of error inherent in comparative analysis. The thrust of the research reported below is to investigate the impact of three fundamental problems of

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1948-62: A Cross-National Study," *Journal of Conflict Resolution*, 10 (September, 1966), 249-271; Bruce Russett, "Inequality and Instability: The Relation of Land Tenure to Politics," *World Politics*, 16 (April, 1964), 442-454.

<sup>1</sup> See, for example, James Davies, "Toward a Theory of Revolution," *American Sociological Review*, 27 (February, 1962), 5-18; Johan Galtung, "A Structural Theory of Aggression," *Journal of Peace Research*, 1 (1964), 95-119; Ted Robert Gurr, "Psychological Factors in Civil Violence," *World Politics*, 20 (January, 1968), 245-278; Chalmers Johnson, *Revolution and the Social System*, Hoover Institution Studies #3 (Stanford: The Hoover Institution on War, Revolution, and Peace, Stanford University, 1964); Mancur Olson, "Rapid Growth as a Destabilizing Force," *Journal of Economic History*, 23 (December, 1963), 529-552.

<sup>2</sup> See, for example, Douglas Bwy, "Political Instability in Latin America: The Cross-Cultural Test of a Causal Model," *Latin American Research Review*, 3 (Spring, 1968), 17-66; T. R. Gurr, A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," *American Political Science Review*, 62 (December, 1968), 1104-1124; Manus Midlarsky and Raymond Tanter, "Toward a Theory of Political Instability in Latin America," *Journal of Peace Research*, 4 (1967), 209-227; Donald Morrison and H. M. Stevenson, "Integration and Instability: Patterns of African Political Development," *American Political Science Review*, 66 (September, 1972), 902-927.

<sup>3</sup> Douglas Hibbs, *Mass Political Violence* (New York, 1972).

\* I want to acknowledge the assistance of Raymond Duvall at many stages of this research effort, especially in the final revisions for the section on measurement error. Comments of unidentified referees also have been useful.

<sup>1</sup> Harry Eckstein, "On the Etiology of Internal Wars," *History and Theory*, 4 (1965), 133-163.

<sup>2</sup> See, for example, R. J. Rummel, "Dimensions of Conflict Behavior Within Nations, 1946-59," *Journal of Conflict Resolution*, 10 (March, 1966), 41-64; R. J. Rummel, "A Field Theory of Social Action with Application to Conflict Within Nations," *General Systems Yearbook*, 10 (1965), 183-211; R. J. Rummel, "Dimensions of Conflict Behavior Within and Between Nations," *General Systems Yearbook*, 8 (1963), 1-50; Raymond Tanter, "Dimensions of Conflict Behavior Within and Between Nations, 1958-60," *Journal of Conflict Resolution*, 10 (March, 1966), 41-64; Raymond Tanter, "Dimensions of Conflict Behavior Within Nations, 1955-60: Turmoil and Internal War," *Peace Research Society Papers*, 3 (1965), 159-183. For a summary of the dimensionality studies see Leo Hazel-

comparative analysis on one recent model of civil conflict. The three problems or sources of error are: measurement error (differences in score for the same concept for the same observation), sampling error (biases due to an unrepresentative sample), and error in theoretical specification (in particular, error caused by the exclusion of important theoretical variables). The model to be investigated is that developed by Gurr and Duvall as a formalization of Gurr's earlier theoretical work.<sup>8</sup> The concepts in that model will be measured by two distinct data sets to estimate measurement error; measures will be applied to a sample of black African nations, most of which were excluded from Gurr and Duvall's analysis, to partially assess sampling bias; and new concepts will be introduced to assess error in theoretical specification.

While the primary objective is to evaluate a major empirical effort at general theorizing on strife, the results of this research have important substantive implications for the Africanist as well as the general comparativist. Some argue, on the one hand, that data on African countries are so unreliable that quantitative studies of these nations are meaningless. If our evaluations indicate that the two African data sets provide very disparate results, that position is supported, but if results are comparable across data sets for the African sample, that position is weakened. A second argument states that efforts at generalization are useless since circumstances in Africa are so different from those elsewhere. It is believed that regional (or even country-specific) characteristics require explanations within the bounds of a particular region or type of system and that no general statements can hold across all systems. If the following analyses of the African sample correspond to Gurr and Duvall's findings, the generalist's position is strengthened, but if different relationships from the general model obtain in the African sample across both data sets, the country or area specialist's position is supported. Following a specification of the sample, the two data sets, and the measurement of concepts in each data set, each of the three sources of error will be discussed and their extent and effects estimated.

#### Procedures for Model Evaluation

Gurr and Duvall present "an eleven variable, bloc recursive, simultaneous equation model."<sup>9</sup>

<sup>8</sup> See, in particular, Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970). The Gurr and Duvall model has been chosen for this analysis since it is more fully specified and operationalized than others. Although Hibbs's work may be better known, it does not represent a single, fully developed body of theory.

<sup>9</sup> Gurr and Duvall, p. 135.

Their eleven concepts are strain (structural constraints on the equitable distribution of values within and among societies), economic development (ECDEV), governmental centrism (CENT), political democracy (DEMOC), immediate past conflict (past MPC), historic traditions of conflict (TRAD), dissident institutional support (DIS), regime institutional support (RIS), foreign or external intervention (EXINT), stress (shortages or declines in supply of valued goods), and magnitude of political conflict (both turmoil and rebellion). In addition a multiplicative identity, tension, is incorporated in their model. They maintain that the variables can be divided into four subgroups:

- (1) Strain, ECDEV, CENT, and DEMOC are as a group independent of other variables in the model. They are exogenous—that is, none of their variance is to be explained within the model. We can think of them as the more remote or "necessary" conditions of political conflict. Note too that they are relatively invariant over time. Few societies can rapidly reduce strain or rapidly change their gross level of economic productivity; nor do societies rapidly institutionalize either polyarchy or autocracy.
- (2) Past MPC is a lagged endogenous variable.
- (3) TRAD, DIS, RIS are an interrelated set of variables which partly determine one another. The expansion of dissident institutional support, for example, tends to occur at the expense of RIS, and vice versa. These three conditions are partly, not wholly, determined by the first two blocs of variables. They are also, we assume, more susceptible to change.
- (4) The final bloc of variables includes stress, EXINT, and magnitude of political conflict (as well as the multiplicative term because of its stress component). They are closely linked to one another and, except EXINT, are largely determined within the model. All except the multiplicative term are "volatile," subject to considerable fluctuations over time.<sup>10</sup>

The primary interest here is in conflict, and therefore the original model is simply reduced to two blocs: (1) exogenous or pre-determined variables—strain, economic development, dissident institutional support, regime institutional support, past conflict (both turmoil and rebellion), and conflict traditions as a simple function of past conflict; and (2) endogenous variables—stress, external intervention, and conflict (both turmoil and rebellion), corresponding directly to the conflict bloc in the Gurr and Duvall model. Such modifications do not permit evaluation of the complete Gurr and Duvall model. In particular, I am unable to deal with the set of equations that they give as their first bloc in which regime institutional support, dissident institutional support, and conflict

<sup>10</sup> *Ibid.*, pp. 141–142.



traditions are reciprocally endogenous. On the other hand, this modification permits an elimination of variables that do not appear in the conflict bloc, namely democracy and centrism, which in the African sample do not vary (at least as Gurr has judgmentally coded them), and it enables the reduction in importance of the conflict traditions concept, which in Gurr and Duvall's whole model plays a central role but which is relatively marginal to the conflict bloc. Changes in scoring of that concept that I have introduced (see below) should not, therefore, drastically affect results of analysis.

Gurr and Duvall assess their model of civil conflict with a sample of 86 nations for the period 1961-1965. The authors exclude from their sample a number of less technologically developed nations primarily because of missing or inadequate data, but as a result, their sample overrepresents Western, industrialized nations and raises the possibility of sampling bias. Their results, in other words, might not be general but might instead apply only to a restricted universe of relatively contemporary, "more developed" countries. They use primarily data that Gurr has reported elsewhere, although they do collect additional measures to supplement the earlier set. Gurr claims that his coders did extensive practice coding, but no formal reliability tests were conducted. Thus, a second problem with their work is the unknown magnitude of measurement error and, more important, the unestimated effects of error on the authors' results.

In an effort to assess the extent and impact of these possible sampling and measurement biases, I have replicated the Gurr and Duvall model employing two distinct data sets in a different sample that consists of 25 black African nations, all but seven of which were excluded from the Gurr and Duvall study.<sup>11</sup> This procedure provides only an imperfect test of sampling bias since numerous nations remain systematically excluded from both the 86-nation sample and the African sample. Most apparent is the absence of a truly spatially and temporally representative sample of societies. The African sample is no more a general representative sample than is Gurr and Duvall's. If the model proves invalid for the African context, however, one can conclude safely that it fails a weak test for sampling error and is probably far from general.

The first data set used in this study consists primarily of Gurr's for the black African countries. Because his data on these countries are not

as extensive as for Gurr and Duvall's 86 nations, use is made of measures from the *World Handbook of Political and Social Indicators*.<sup>12</sup> The second data set consists of measures taken from Morrison *et al.*'s *Black Africa: A Comparative Handbook*<sup>13</sup> and supplementary *World Handbook* measures are employed for two indicators. Only one *World Handbook* indicator (government sanctions) is incorporated in both data sets; hence there is a minimum of shared data between them.

Gurr and Duvall follow certain procedures to measure their concepts, and the primary criterion in scoring concepts from these African data sets will be to follow as closely as possible the logic of their measurement procedures. In particular, they utilize two measurement strategies: (1) indirect measurement through multiple indicators, and (2) direct operationism. The more abstract concepts in their model, such as strain and stress, are indirectly measured. For these concepts, what is attempted here is to develop a set of seemingly valid indicators, subject them to the same validating procedures, and generate composite indices in a similar fashion. In this way one is probing measurement error as the differences in observational bases (particularly as manifested in different indicators) since the logic of the measurement procedures is comparable. When scoring more concrete concepts, Gurr and Duvall employ direct operationism in two forms. First, they use a single indicator for economic development, and I employ that same indicator as it is reported in two different sources. Measurement error in this case is attributable solely to differences in observational base. Second, for conflict and external intervention they assess the number of events and relevant attributes of events (e.g. number of participants). Here I use two different measurement procedures since one data set follows procedures parallel to Gurr and Duvall, while the other uses a more standard procedure of simple counting of events unweighted by attributes. In these cases, then, the assessment of measurement error involves not only differences in observational base but also differences in procedure.

Table 1 lists all of the concepts to be included in replications of the model and compares the indicators employed from each data set to measure each concept.<sup>14</sup>

<sup>12</sup> Charles L. Taylor and Michael Hudson, *World Handbook of Political and Social Indicators*, 2nd ed. (New Haven: Yale University Press, 1972).

<sup>13</sup> Donald G. Morrison, Robert Mitchell, John N. Paden, and Hugh M. Stevenson, *Black Africa: A Comparative Handbook* (New York: Free Press, 1972).

<sup>14</sup> Concept scores are created by calculating the mean standard scores of all indicators of a concept. When a concept consists of several dimensions (e.g., economic, political, and social dimensions of stress), a mean standard score for each dimension is first cal-

<sup>11</sup> The twenty-five countries are: Burundi, Cameroun, CAR, Chad, Dahomey, Ghana, Guinea, Ivory Coast, Kenya, Liberia, Malawi, Mali, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Somali, Sudan, Tanzania, Togo, Uganda, Upper Volta, Zaire, Zambia.

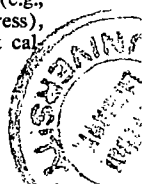


Table 1. Comparison of Concept Measurement Bases for two Data Sets and the Initial Model

Concept	Gurr and Duvall 86-Nation Study	Gurr Indicators for Present Sample	Black Africa Indicators for Present Sample
Strain	Economic Discrimination  Political Discrimination Religious Cleavages Separatist Feelings Economic Dependence: negative net factor payments abroad as % GDP Proportion exports to largest country	Economic Discrimination  Political Discrimination Religious Cleavages Separatist Feelings Export Concentration by Com- modity (W.H.)	Ethnic Unrepresentativeness of cab- inet at Independence Number of Languages Language Classification Ethnic Diversity: Hierarchy <sup>a</sup> Ethnic Diversity: Stratification <sup>a</sup>  Ethnic Diversity: Agricultural De- pendence <sup>a</sup> Export Dependence Trade as % GNP % US Aid to Africa 1961 (sqrt) % US Aid to Africa 1964 (sqrt) Aid from East Bloc nations Aid from Metropole 1963
Economic Development	GNP per capita c. 1965	GNP per capita c. 1965 (W.H.)	GNP per capita c. 1963
Past Strife: Turmoil and Rebellion	Regress Gurr 1961-65 on <i>World Handbook</i> data and retrodict for 1956-60	Regress Gurr 1961-65 on <i>World Handbook</i> data and retrodict for 1956-60	Regress <i>Black Africa</i> data 1961-65 on <i>World Handbook</i> data and retrodict for 1956-60
Regime Institutional Support	Party System Stability weighted by # parties National Integration and central- ization Electoral Support Union Support Proportional Police Manpower	Growth in Central Gov't Expendi- ture Size of Central Gov't Expenditure  Electoral Support Union Support Coercive Potential Defense Expenditure per capita (W.H.)	Size Gov't Budget 1961 Expenditure/GNP 1961 Expenditure per capita 1966 Expenditure/GNP 1963 Legislative Support <sup>b</sup> Defense Expenditure 1965 Defense Expenditure/GNP 1963 Military/population 1967 Change Police Size 1964-67
Dissident Institutional Support	Size, Organization and physical isolation of dissidents Transportation Networks relative to area and population density Size of Communist Parties weighted by party status Size of Unions weighted by opposi- tional status	Size, Organization and physical isolation of dissidents Transportation Networks relative to terrain	No comparable indicators—Gurr's DIS measure used
Conflict Traditions	Durability of Regime and Scope and Success of Internal War 1850-1960 Frequency and Success of Coups 1900-60 Recency of Last Military interven- tion, 1960 Union Opposition Status, 1955-65	Past Turmoil + Past Rebellion	Past Turmoil + Past Rebellion
Stress	Short Term Trend in export Val- ue 1950-60 Declines in Short Term Trends in Export Value, 1961-65 Defense Expenditure as % GNP # of Negative Gov't Sanctions Proportional Military Manpower (Europe) Proportional Police Manpower (Europe)	Trade Declines X Economically Mobilized Population Growth Energy Consumption, re- versed (W.H.) Negative Government Sanctions (W.H.) Downturns in Secondary Educa- tion	Decline GDP Decline GNPCAP Decline Energy  Decline Primary School Enroll- ments, 1960-66 Decline Secondary School Enroll- ments, 1962-66 Decline Literacy, 1950-65  Negative Government Sanctions (W.H.)

<sup>a</sup> Scores indicate diversity among ethnic groups in a country.

<sup>b</sup> To create an indicator parallel to Gurr's electoral support, the author coded each country on the average percentage of legislative seats held by the ruling party 1961-65.

Table 1. (Continued)

Concept	Gurr and Duvall 86-Nation Study	Gurr Indicators for Present Sample	Black Africa Indicators for Present Sample
External Intervention	Extent of External Support for Dissidents	Extent of External Support for Dissidents	Number of External Interventions (W.H.)
Conflict: Turmoil	Extent and Intensity of Riots, Demonstrations, Strikes, Clashes, Local Uprisings	Extent and Intensity of Riots, Demonstrations, Strikes, Clashes, Local Uprisings	Number of riots, Demonstrations Strikes, 1961-65
Rebellion	Extent and Intensity of Plots, Coups, Terrorism, Civil Wars, etc.	Extent and Intensity Plots, Coups, Terrorism, Civil Wars, etc.	Number of Civil Wars, Rebellions, Irredentism, Feuds, Revolts, 1961- 1965
Tension	(Stress+Strain) × Traditions	(Stress+Strain) × Traditions	(Stress+Strain) × Traditions

In general, as indicated above, Gurr's data on the African countries can be seen to be comparable to, though less extensive than, indicators for the 86 countries. *Black Africa* data consist of somewhat different variables but provide numerous indicators to tap the major concepts.

For particular concepts, Table 1 reveals the following:

(1) The only concept for which neither *Black Africa* nor *World Handbook* measures could be found was dissident institutional support, and hence two of Gurr's measures are employed for both analyses.

(2) In their concept of strain Gurr and Duvall include measures of four dimensions: economic discrimination, political discrimination, social cleavages, and international economic dependence (export concentration). *Black Africa* data supply indicators of only three of these dimensions: social diversity or cleavages, economic dependence (export, trade, aid), and political discrimination (ethnic unrepresentativeness of cabinet).<sup>15</sup>

culated, and then the mean of the dimensions obtained. Gurr and Duvall use a similar simple additive model for creating RIS, DIS, and TRAD scores. In scoring strain and stress, however, they regress the separate indicators on their measures of conflict and then weight each indicator according to the structure of the regression equation. That procedure was not employed here since it results in several scores for the same concept and also since it seems a bit arbitrary.

<sup>15</sup> Since Gurr and Duvall include political discrimination in their measure of strain it is included in the following analyses. Evidence suggests, however, that political discrimination has different effects than do the other components of strain. Regressions of the strain components on conflict for both data sets reveal that other forms of discrimination promote conflict, while political discrimination tends to reduce conflict. Consider, for example, turmoil:

$$\begin{aligned} \text{Gurr turmoil} = & .36 \text{ export concentration} + .39 \text{ economic} \\ & \text{discrimination} + .20 \text{ religious} \\ & \text{cleavages} + .11 \text{ separatist feelings} \\ & - .03 \text{ political discrimination} - \\ & 2.16469 \end{aligned}$$

(3) Comparable measures are available for economic development, since all data sets have information on GNP per capita, the sole indicator employed by Gurr and Duvall.

(4) Gurr and Duvall measure past strife by regressing their turmoil and rebellion scores for 1961-65 on the *World Handbook's* four indicators of strife (number of deaths, riots, demonstrations, and armed attacks) for the same period. They then estimate past strife by applying the regression equation to *World Handbook* data for 1955-60. The same procedure is employed here for the African sample, regressing Gurr measures of conflict in the one case, and *Black Africa* measures in the other.

(5) As expected, the concept of regime institutional support, at least as Gurr and Duvall measure it, involves the dimensions of size, support, and coercive potential of the national government. While exact indicators differ from the Gurr and Duvall study, all three of these dimensions seem to be tapped adequately by both Gurr and *Black Africa* data.

(6) The indicators that Gurr and Duvall propose for traditions of conflict simply are not appropriate for the African sample. Regime durability as of 1960, and frequency and recency of

$$\begin{aligned} \text{Black Africa turmoil} = & .34 \text{ economic discrimination} \\ & + .44 \text{ social differences} + \\ & .50 \text{ external dependence} - \\ & .48 \text{ political discrimination} \\ & + 5.04637 \end{aligned}$$

Gurr and Duvall also report in a footnote that political discrimination seems to reduce conflict, unlike the other components of strain, but they proceed to include it in their concept score. Thus, they give positive weights to the other strain indicators and a negative weight to political discrimination. This procedure seems to deviate from their conceptualization of strain, which is viewed as a positive contributor of conflict. Since the negative impact of political discrimination is most marked in the *Black Africa* data, two measures of strain are created and employed in later analyses, one including and one excluding political discrimination.

coups prior to 1960 are generally irrelevant to African countries, most of which were still under colonial rule until 1960. The closest approximation to conflict traditions is immediate past conflict, which is distinguished by Gurr and Duvall as a separate concept. I have collapsed that distinction, however, and scored traditions as the sum of past conflict scores (turmoil and rebellion) for each data set.

(7) Stress measures for Gurr and Duvall involve short-run economic declines and political restrictions. Both data sets provide indicators of economic downturns, and both provide measures of educational declines to tap an additional dimension of social stress. The *World Handbook* variable of government sanctions is used to tap political restrictions for both analyses.

(8) For external intervention, Gurr and Duvall employ a coded variable which estimates the extent of external support for dissidents, and the same measure is available for the African sample. Since *Black Africa* does not have a comparable indicator, the number of external interventions listed in the *World Handbook* is employed for the second data set.

(9) The final and central concept to be measured is political conflict. Gurr's measures of conflict for the African sample include the extent and intensity of turmoil (events with limited political objectives) and rebellion (efforts to bring about more fundamental change in the personnel or structure of government). Extent is measured by summing the man-days of participation which are estimated by multiplying the number of non-governmental participants by the event's duration in days. Intensity is measured by summing the deaths associated with all conflict events. Gurr's measures of turmoil and rebellion used here for the African countries are identical to measures used by Gurr and Duvall in the 86-nation study. In contrast, *Black Africa* measures of conflict consist solely of the number of events summed, with no weightings for duration, intensity, scope of participation, or population size.<sup>16</sup>

#### Measurement Error

The first source of bias in the statistical evaluation of general models is measurement error, the problem of inaccuracy in concept scores. In at-

tempting to assess such error, and its impact on inferences drawn from the analysis of data, one is concerned with the twin problems of validity and reliability. Both of these issues are closely related to, and often confused with, problems of inference imposed by sampling biases. In this section and the next, I attempt to disentangle the two primary sources of error and to estimate the extent and nature of each.

Validity refers to the degree to which we are measuring what we think we are measuring. Gurr and Duvall assess validity through face validation (an argument which logically justifies an indicator as an adequate representation of a concept), and also through construct validation, a procedure in which one judges the validity of a concept by the degree to which the concept relates to other concepts to which it is theoretically related. Gurr and Duvall utilize conflict as the construct according to which measuring procedures for other concepts are validated and discarded as invalid indicators which do not correlate adequately with conflict. In the present study, I have attempted to replicate their validation procedures. Although the primary criterion in developing indicators from the two data sets was to approximate Gurr and Duvall's measures as closely as possible, every seemingly valid indicator of each concept was correlated with measures of conflict and any indicator that correlated strongly in a direction other than that predicted by theory was not included.

The issue of validity becomes quite complicated in cross-national research, for it is related to the problem of conceptual equivalence. It is possible, indeed probable, that indicators which validly represent a concept in one cultural context have different meanings, and hence may not reflect the same concept, in another cultural setting. This problem implies that indicators developed by Gurr and Duvall might not be valid indicators for the African sample. The criticism would not be directed at the validity of their measures for their 86 nations but rather at the validity of their measures when applied to African countries in this particular research. Later efforts to weed out various sources of error necessitate fairly exact replication of the general model, but by adopting nearly identical indicators, I may be introducing invalid measurements for the African sample. Although some indicators in the African sample do differ somewhat from the general model and although construct validation is employed, the primary criterion in concept measurement is comparability. There exists, then, a constant struggle between efforts to obtain cross-research comparability and cross-cultural equivalence. The problems of validity and sampling bias now can be seen to be closely interwoven, since attempts to extend the sample may threaten validity, and in-

<sup>16</sup> Turmoil data have been collected by the authors of *Black Africa* but are not published in that book because of questionable reliability. Turmoil data begin with the year of independence. Pre-independence years for countries independent after 1960 have been updated with *World Handbook* data. Taylor and Hudson code days of rioting, demonstrating, and so forth, rather than the number of events of each type. For comparability, five days were considered roughly equivalent to one event.

variant measurement rules may apply validly to a very restricted sample.

Beyond the initial face validation of separate indicators (dependent on the judgment of Gurr and Duvall), and the simple construct validation of correlating indicators with conflict, the only method to assess measurement validity for the African sample has been a second-order face validation. That is, does one "feel" that the distribution of concept scores among the African countries adequately reflects the nature of the concept? Does one agree that countries ranking high on strain, for example, in reality experience more strain than countries ranking lower? While readers must draw their own conclusions,<sup>17</sup> two general points should be made here. First, on grounds of previous and cumulative knowledge, one can challenge the validity of some of the measurement procedures. GNP per capita is probably not a sufficient indicator of the concept economic development whose valid measurement would require tapping far more than gross productivity. Similarly the validity of Gurr's external intervention is probably weakened by relying solely on support for dissidents and not including interventions for established governments. These issues, however, are not my major concern here. Second, validity is never a directly assessable concept, so that what has been done here is virtually the extent practicable. For that reason, I do not pursue the issue of validity any further in what follows.

Reliability is a problem of the consistency and accuracy of our measures, assuming their validity. A considerable body of literature spells out sources of observational error, methods of controlling such error, and techniques to limit the biases that unreliable data can produce in cross-national or cross-cultural research.<sup>18</sup> But since the data and measurement techniques for this re-

search are largely given, the more appropriate question relates to estimating the extent of error already existing in the data. However, in assessing the impact of unreliable measurement procedures, one again becomes involved in the related problem of sampling bias, for we have reason to believe that data on African countries are less accurate than for most other nations. In attempting to determine sources of error in aggregate data, Rummel has found that the level of economic development is related to some measures of error.<sup>19</sup> African nations, being among the least technologically developed, would be expected to yield relatively more erroneous data. In other words, indicators *could* be relatively reliable measures for the 86-nation study, but considerably less reliable for the African sample. Estimates of measurement error, then, reflect only indirectly on Gurr and Duvall's work.

Standard tests of reliability are not applicable for assessing error in the African data. Interitem correlation is inappropriate since some concepts are directly and singularly operationalized and since most other concepts consist of several conceptual dimensions (e.g. strain). In addition, Gurr's failure to report intercoder agreement, and the coding of *Black Africa* data by single coders mean that standard intercoder tests of reliability are impossible. I offer, however, a test that is similar in logic to intercoder agreement, in being based on two applications of equivalent instruments to the same observational set. That is, if for the same countries, measures of a concept from two independently collected data sets interrelate highly, then the measures can be assumed to be reliable (or systematically unreliable); if there is little relationship between two concept scores, then measurement is not reliable. Low correlations unambiguously reflect low reliability, and high correlations are only suggestive of high reliability, since they can also reflect systematic inaccuracy.

Consider, for example, the following model of measurement:

$$X' = X + f(X) + g(Z) + c + e,$$

where:  $X'$  = observed score

$X$  = true score

$Z$  = other variables

$c$  = some constant

$e$  = random disturbance.<sup>20</sup>

The nature of correlations is such that the linear components of the measurement model have no

<sup>17</sup> Concept scores and rank orders on each concept for the twenty-five nations are available from the author.

<sup>18</sup> See, for example, Donald T. Campbell, "Techniques for Determining Cultural Biases in Comparative Research," in *Comparative Perspectives: Theories and Methods*, ed. Amitai Etzioni and Frederick L. Dubow (Boston: Little, Brown, 1970), pp. 407-410; Kenneth Janda, "Data Quality Control and Library Research on Political Parties," in *A Handbook of Method in Cultural Anthropology*, ed. Raoul Naroll and Ronald Cohen (Garden City: National History Press, 1970), pp. 962-973; Raoul Naroll, "Data Quality Control in Cross-Cultural Surveys," in Naroll and Cohen, pp. 927-945; R. J. Rummel, "Dimensions of Error in Cross-National Data," in Naroll and Cohen, pp. 946-961; Raoul Naroll, *Data Quality Control—A New Research Technique* (New York: Free Press, 1962); Bruce Russett, "Techniques for Controlling Error," in *Statistical and Quantitative Methods*, ed. Michael Haas (Evanston: Northwestern University Press, 1972).

<sup>19</sup> Rummel, "Dimensions."

<sup>20</sup> By random disturbance I mean that:  $E(e_i) = 0$ , all  $i$ ;  $E(e_i e_j) = 0$ , all  $i, j, i \neq j$ ;  $E(e_i^2) = \sigma_e^2$ , all  $i$ .

effect on correlation coefficients. Therefore, if the correlation between two measures of the same concept remains close to 1.00 (i.e. seemingly reflects high reliability) observed scores can contain appreciable error but that error must be systematic and linear in at least one of the observed scores. Alternative interpretations for high correlations between scores is small and random error or nonlinear systematic error that is of the same form in both sets. On the other hand, when the correlations differ greatly from 1.00 it can be concluded safely that at least one of the scores contains a large random component and/or a nonlinear systematic component (i.e.,  $f(X)$  and/or  $g(Z)$  are nonlinear functions).

Thus one can draw some conclusions about the nature and extent of measurement error from correlations between two measures of a concept. Table 2 reports the proportion of unshared variance in concept scores (1.00 minus the squared correlations). The higher the unshared variance, the less reliable the score. One sees that past turmoil, economic development, and past rebellion share most of their variance in common across data sets. At this point all that can be concluded is that error is of one of three forms: (1) small random, (2) linear systematic, or (3) common, nonlinear systematic. For the other six concepts, error is extensive, and it is either random or systematic of a different nonlinear form in each of the data sets. While it is not surprising that the greatest unreliability is evidenced for the three concepts that involve different measurement procedures (turmoil, rebellion, external intervention), it is perhaps surprising that the three multiply indi-

cated concepts are so unreliable (stress, strain, regime institutional support).

It is important to assess how much of the unreliability in these six concepts is due to nonlinear systematic error. One can probe further the nature of error by correlating these concepts with dissident institutional support which is the same in both data sets. Table 3 reports the relevant correlations. If two scores for the same concept correlate very differently with a single referent (dissident institutional support), then systematic error is involved in at least one of the two scores. Random error attenuates the correlations but the attenuation is expected to be similar in each case. It is apparent in Table 3 that dissident institutional support correlates differently with each of the two measures for turmoil, rebellion, strain and perhaps stress, pointing to significant nonlinear systematic error in at least one of the scores for each concept. On the other hand, the similar correlations involving external intervention and regime institutional support suggest the predominance of random error.

Information in Tables 2 and 3 helps to estimate the extent and nature of error in concept scores. A final point of interest for later attempts to estimate the impact of error is the extent to which systematic error is shared across concepts within a data set. The basis for these inferences rests on a comparison of correlations among concepts across data sets. Table 4A includes the correlations among the four variables concluded to contain extensive nonlinear systematic error. Interpretation of this table is based on the following logic: If the correlations between two concepts known to contain extensive systematic error differ markedly across data sets, the form of the systematic error is not similar across concepts in at least one data set; if the correlations for concepts known to be systematically erroneous are similar and also large, then systematic error is of the same form across concepts within data sets. The application of this logic to Table 4A leads to the conclusion that turmoil, rebellion, and stress share systematic error in both data sets and that the systematic error is of a different form in each of the data sets. The systematic error in strain, on the other hand, is not of the same form as that for turmoil, rebellion, and stress in at least one of the data sets.

The final question in the assessment of measurement error is the determination of the likelihood of systematic error in seemingly reliable variables (economic development, past turmoil, past rebellion). Table 4B reports the correlations between these variables and those containing extensive, shared systematic error (turmoil, rebellion, stress). In interpreting this table the following logic is used: If correlations of seemingly reliable scores with systematically erroneous scores are

Table 2. Unshared Variance for Concepts Across Data Sources<sup>a</sup>

Concept	Unshared Variance
External Intervention	.73
Rebellion	.65
Turmoil	.59
Stress	.54
Strain	.54 (.44) <sup>b</sup>
Regime Institutional Support	.48
Past Rebellion	.21
Economic Development	.17
Past Turmoil	.10

<sup>a</sup> Dissident institutional support is excluded from the table since the same score is used for both data sets. Tension also is excluded since it is based on other concept scores.

<sup>b</sup> The first strain figure is the unshared variance between the Gurr score and the *Black Africa* score which replicates the Gurr score; the second strain figure is the unshared variance between the Gurr score and the *Black Africa* score, which excludes political discrimination. See footnote 15.

Table 3. Correlations with Dissident Institutional Support for Both Data Sets

Variable	<i>r</i> Gurr	<i>r</i> <i>Black Africa</i>	Difference in Covariance	Inferred Error <sup>a</sup>
Turmoil	.27	.50	.18	<i>S</i>
Rebellion	.56	.61	.06	<i>S</i>
Strain	.33	.22	.06	<i>S</i>
Stress	.49	.45	.04	?
External Intervention	.36	.32	.03	<i>R</i>
Regime Institutional Support	-.41	-.43	.01	<i>R</i>
Economic Development	-.27	-.32	.03	<i>R</i>
Past Turmoil	.44	.46	.02	<i>R</i>
Past Rebellion	.39	.42	.02	<i>R</i>

<sup>a</sup> The determination of the size of the difference in covariance needed to distinguish systematic from random error is based on the correlations involving economic development, past turmoil, and past rebellion, which are the variables inferred in Table 2 to contain little or no nonlinear systematic error of appreciable difference across data sets. *S* stands for systematic error, and *R* stands for Random error.

similar across data sets, then the former are not sharing appreciable nonlinear systematic error with the latter in either set;<sup>21</sup> if, on the other hand,

<sup>21</sup> The reason for this logic is that the form of error for the systematically erroneous variables is known to be different for the two data sets and nonlinear in at least one of them, whereas the seemingly reliable variables can contain nonlinear systematic error only if the nonlinear form is similar across data sets. Therefore, shared systematic error is possible for a maximum of one data set and is compatible only with differences in correlations across data sets.

correlations are different across data sets, then the form of systematic error might be shared with turmoil, rebellion, and stress in at most one data set. Where the correlations are high, this possibility assumes a certain likelihood. From this logic, one can conclude that economic development is not systematically erroneous in a form similar to turmoil, rebellion, and stress in either set but that past turmoil and past rebellion probably share systematic error particularly with turmoil and rebellion in one of the data sets.

Table 4. Comparison of Correlations Among Variables Between two Data Sets

Variables	<i>r</i> Gurr	<i>r</i> <i>Black Africa</i>	Difference in Covariance	Shared Systematic Error <sup>a</sup>
A. Variables With Systematic Error				
Turmoil with Stress	.43	.50	.07	yes
Turmoil with Rebellion	.27	.45	.13	yes
Rebellion with Stress	.51	.64	.15	yes
Strain with Rebellion	.26	.18	.04	no
Strain with Turmoil	.51	.12	.25	no
Strain with Stress	.07	.06	.00	no
B. Seemingly Reliable Variables With Systematically Erroneous Variables				
Economic Development with Turmoil	-.08	.01	.01	no
Economic Development with Rebellion	-.21	-.07	.03	no
Economic Development with Stress	.11	-.25	.07	no
Past Turmoil with Turmoil	.42	.65	.24	yes
Past Turmoil with Rebellion	.35	.67	.33	yes
Past Turmoil with Stress	.59	.71	.15	?
Past Rebellion with Turmoil	.03	.40	.16	yes
Past Rebellion with Rebellion	.39	.62	.23	yes
Past Rebellion with Stress	.49	.56	.07	?

<sup>a</sup> Although no conventional guide exists, a difference in covariance of roughly .15 is used as a cutting point to distinguish similar from dissimilar correlations.

Three conclusions follow from this analysis of measurement error. First, we have some knowledge of the probable form and extent of error in the African data. In particular, (1) economic development has small and probably random error; (2) regime institutional support and external intervention contain large, random error; (3) strain has some systematic error; (4) stress, rebellion, and especially turmoil have large, shared systematic error; and (5) past turmoil and past rebellion contain some systematic error, which in one of the data sets is shared with turmoil, rebellion, and perhaps stress.

Second, although measurement error can be assessed directly only for the African data, the findings have direct implications for the Gurr and Duvall study. Because turmoil, rebellion, and stress are known to have shared systematic error in both data sets, and because one set of measures for turmoil and rebellion are identical to measures in the 86-nation study, the likelihood that turmoil, rebellion, and probably stress share systematic error in the Gurr and Duvall study is very high.

Third, measurement procedures popular in contemporary political science are implicated. Each of the five concepts believed to share systematic error involve the counting of numbers of events (stress contains other indicators as well). The uniformly higher correlations among these concepts for the *Black Africa* data (see Tables 4A and 4B) suggest that systematic error is of a more similar form across concepts when measurement is based on simple counting of events rather than on scoring attributes of these events. This is not

to say that the *extent* of systematic error is greater, but rather that given a certain magnitude of error, the *form* is more similar under procedures of simple tabulation.

#### Sampling Error

A second major source of error in comparative analysis results from the use of nonprobability samples. Large-scale cross-national studies often analyze all countries on which relevant data are available or else focus on regional subsamples. Neither alternative provides the analyst with a representative sample of the population of nation-states. Instead, the generalizability of results is always suspect because variables may behave differently in different subsamples.

Gurr and Duvall's 86-nation study clearly is subject to sampling error since the authors systematically underrepresent the poorer nations of the world because of data availability. We would like to know, then, if in fact variables behave in other subsamples as they do in Gurr and Duvall's 86-nation sample, so as to estimate if sampling error exists. One way to estimate such error is to compare correlations among concepts in the two African data sets with correlations in the Gurr and Duvall sample. When correlations in the two African samples are similar but both differ from the 86-nation study, we have evidence of sampling error, i.e., that relationships are different in the African subsample.

Table 5 provides the correlations among the concepts that appear to behave differently in the two subsamples—strain, economic development,

Table 5. Comparison of Correlations Across Samples

Variables	Gurr Data African Sample	<i>Black Africa</i> Data African Sample	Gurr-Duvall Data 86-nation Sample
Strain with Past Turmoil	-.01	.01	.46
Strain with Past Rebellion	.06	.10	.38
Strain with Stress	.07	.06	.41
Strain with External Intervention	-.05	-.09	.53
Strain with Rebellion	.26	.18	.64
Economic Development with Past Turmoil	-.04	-.10	-.34
Economic Development with Past Rebellion	-.07	-.08	-.43
Economic Development with External Intervention	-.07	-.14	-.37
Economic Development with Turmoil	-.08	.01	-.36
Economic Development with Rebellion	-.21	-.07	-.48
Economic Development with Tension	-.01	-.06	-.61
Economic Development with Stress <sup>a</sup>	.11	-.25	-.59
Regime Institutional Support with Rebellion	-.23	-.23	-.52
Regime Institutional Support with Tension	-.01	-.26	-.55
Regime Institutional Support with Dissident Institutional Support	-.41	-.43	-.62
Regime Institutional Support with Past Turmoil	-.16	-.15	-.34
Regime Institutional Support with External Intervention <sup>a</sup>	.02	-.18	-.34

<sup>a</sup> Differences across samples are questionable because of the instability of correlations across the two African data sets.



and regime institutional support. While Gurr and Duvall find strong relationships between strain and a majority of their concepts, strain scores in Africa have little relationship to other model concepts. Either Gurr and Duvall's measures of strain are not valid measures of the concept in Africa, or else strain has different effects in the African context. Economic development relates strongly and negatively to most of Gurr and Duvall's concepts, but in Africa little relationship obtains. This finding probably indicates that the threshold at which economic development affects phenomena such as conflict has not been reached yet by most African nations and that among 25 poor nations economic development is unrelated to other model concepts. Similarly, regime institutional support demonstrates somewhat different patterns in Africa, primarily in that its effects are less strong than in the 86-nation sample.

While many correlations are similar across data sets and across samples we do have evidence that some variables intercorrelate differently across samples and hence that Gurr and Duvall's conflict model is subject not only to measurement error but also to sampling error. Two caveats are worth mention, each deriving from the previous discussion of measurement error. First, I probably *underestimate* sampling error to some extent because past turmoil and past rebellion are expected to have different correlations in the two African samples. Second, I probably *overestimate* sampling error because the large random components in regime institutional support and probably strain lead to the expectation of attenuated correlations in the African sample, at least if error is greater for the African countries than others. Overall, sampling error for Gurr and Duvall seems less extensive than measurement error for African countries.

#### Estimating the Impact of Measurement and Sampling Error

The model that Gurr and Duvall offer is in structural equation form; regression techniques are used to estimate structural parameters.<sup>22</sup> The basic assumptions of regression analysis imply first, that there is no specification error, a problem discussed in the following section; second, that there is no systematic error in any variable(s); third, that random error can exist only in the dependent variable; and fourth, that there is no sampling error (variables on observations in the sample do not have a different probability dis-

tribution than do variables in the universe of observations). Violations of any of these assumptions can lead to biased estimates.<sup>23</sup>

The two previous sections indicated that our measures of concepts and the nature of the sample do in fact probably violate the basic assumptions of inferential regression analysis. There was evidence that dependent variables such as turmoil and rebellion contain systematic error in the African sample; that many of the independent variables are quite erroneous, often including some systematic component; and that sampling error is operative since some concepts appear to behave considerably differently in the two samples. The *potential* effects of such violations are severe, but it would be more interesting to discover what the *actual* effects are. It is an easy escape for the researcher to warn the reader that his results might be biased if in fact assumptions are violated, but our knowledge of political phenomena, conflict being the case in point, could develop more rapidly if we had estimates of *how* biased our findings actually are. Thus, the central concern of this research effort has been to go beyond the suggestion and discovery of errors to estimate their actual effects, both in the African sample and in the Gurr and Duvall general model. Analyses reported below enable an estimation of the impact of error in concept measurement for the black African countries, although they do not provide a basis for directly judging the accuracy of measurement in any larger sample. Results do point to concepts, however, that are likely to be most unreliably measured in non-African nations. In addition, the following analyses enable direct assessment of sampling error in the general model by determining if results based on a larger number of countries apply to the black African countries. Thus, conclusions will be directed to the effects of sampling error on the Gurr and Duvall general model and of measurement error by students of African politics.

The strategy followed to estimate the impact of errors is to treat sampling and measurement errors together. Gurr and Duvall's equations will be presented, and the presentation will include not only their parameter estimates but also confidence intervals for those estimates (the range in which the "true" value is most likely to fall). The equations are replicated in the black African sample using both data sets. Parameter estimates in these equations are essentially descriptive of the African sample so that the primary concern is to see if they fall within the confidence intervals of the param-

<sup>22</sup> Gurr and Duvall ("Civil Conflict in the 1960's") provide a brief discussion of the techniques used, and Hibbs (*Mass Political Violence*) provides a helpful appendix on two-stage-least-squares regression. Readers unfamiliar with these techniques might find both discussions useful.

<sup>23</sup> These assumptions were derived from discussions in John Johnston, *Econometric Methods* (New York: McGraw Hill, 1963), p. 232, and Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley, 1964), pp. 299-300.

Table 6. Equations for Estimating the Impact of Measurement and Sampling Error<sup>a</sup>

Sample and Data Set	A. STRESS					R <sup>2</sup>
	Determinants of Stress					
	Strain	EcDev	Turlag			
Gurr-Duvall 86 Nations (Turmoil)	<i>+.27</i> (+.45-+.09)	<i>-.51</i> (-.69- <i>-.33</i> )	<i>+.29</i> (+.46-+.12)			.48
Gurr Data Africa Sample	<i>+.09</i>	<i>+.14</i>	<i>+.60</i>			.37
Bl. Africa Data Africa Sample	<i>+.14</i>	<i>-.23</i>	<i>+.68</i>			.55
	Trad	EcDev	Reblag	Exint		
Gurr-Duvall 86 Nations (Rebellion)	<i>+.15</i> (+.36- <i>-.06</i> )	<i>-.26</i> (-.46- <i>-.06</i> )	<i>+.34</i> (+.54-+.14)	<i>+.23</i> (+.44-+.02)		.45
Gurr Data Africa Sample	<i>+.63</i>	<i>+.15</i>	<i>+.04</i>	<i>-.03</i>		.42
Bl. Africa Data Africa Sample	<i>+.67</i>	<i>-.17</i>	<i>-.04</i>	<i>+.08</i>		.58
	EcDev	Turlag	Rebell	R.I.S.	D.I.S.	
Best Equation Gurr Data Africa Sample	<i>+.40</i>	<i>+.38</i>	<i>+.37</i>	<i>-.27</i>	<i>+.11</i>	.56
Replication with Black Africa Data	<i>-.01</i>	<i>+.46</i>	<i>+.40</i>	<i>-.35</i>	<i>-.17</i>	.65
		Turlag	Rebell	R.I.S.		
Best Equation Bl. Africa Data Africa Sample		<i>+.54</i>	<i>+.17</i>	<i>-.33</i>		.65
Replication with Gurr Data		<i>+.45</i>	<i>+.39</i>	<i>-.04</i>		.45
B. EXTERNAL INTERVENTION						
Sample and Data Set	Determinants of External Intervention					R <sup>2</sup>
	Rebell	Strain	D.I.S.			
Gurr-Duvall <sup>b</sup> 86 Nations	<i>+.58</i> (+.83-+.33)	<i>+.06</i> (+.33- <i>-.27</i> )	<i>+.03</i> (+.24- <i>-.18</i> )			.45
Gurr Data Africa Sample	<i>+.84</i> <i>+1.30</i>	<i>-.31</i>	<i>-.26</i>			.56 .60
Bl. Africa Data Africa Sample	<i>+.71</i> <i>+1.02</i>	<i>-.22</i>	<i>-.26</i>			.38 .42
	Rebell		D.I.S.	Tension	Turmoil	
Best Equation Gurr Data Africa Sample	<i>+1.09</i>		<i>-.36</i>	<i>+.40</i>	<i>-.46</i>	.67
Replication with Black Africa Data	<i>+.59</i>		<i>-.23</i>	<i>+.28</i>	<i>+.10</i>	.46
	Rebell	Strain		Tension		
Best Equation Bl. Africa Data Africa Sample	<i>+.28</i>	<i>-.21</i>		<i>+.50</i>		.50
Replication with Gurr Data	<i>+.84</i>	<i>-.31</i>		<i>+.17</i>		.66

<sup>a</sup> Italicized coefficients are those that fall within the .10 confidence limits of the Gurr-Duvall parameter estimates. Parameter estimates are given as standardized coefficients. Coefficients for the African sample are standardized over that sample rather than relative to the 86 nations. Equations were estimated as a simultaneous system with two-stage least squares techniques.

<sup>b</sup> The Gurr-Duvall model can be viewed either as a theoretical specification of three causal variables (rebellion, strain, dissident strength), or as an empirical conclusion of a single causal variable (rebellion).

<sup>c</sup> The second equation based on *Black Africa* data incorporates the Strain score which eliminates Political Discrimination. P.D. refers to the separate Political Discrimination score. See footnote 15.

Table 6. (Continued)

C. TURMOIL							
Sample and Data Set	Determinants of Turmoil						R <sup>2</sup>
	Strain	R.I.S.	EcDev	Trad	Tension		
Gurr-Duvall 86 Nations	+ .41 (+ .71-+ .11)	- .27 (- .47-+ .07)	+ .15 (+ .31-+ .01)	+ .25 (+ .51-+ .01)	+ .36 (+ .80-+ .08)		.56
Gurr Data Africa Sample	+ .55	- .29	+ .18	+ .89	- .66		.35
Bl. Africa Data Africa Sample	+ .01 <i>Str.</i> <i>P.D.</i> <sup>c</sup> + .52 - .39	- .41 - .42	+ .31 + .42	+ .45 + 1.44	+ .14 - 1.32		.51 .64
	Strain	Exint	Stress	Turlag	Reblag	Rebell	
Best Equation Gurr Data Africa Sample	+ .39	- .49	+ .37	- .48	- .32	+ .34	.60
Replication with Black Africa Data	+ .11 <i>Str.</i> <i>P.D.</i> <sup>c</sup> + .48 - .29	- .04 - .05	+ .50 + .15	+ .46 + .55	- .17 + .05	- .02 - .36	.39 .58
	D.I.S.	R.I.S.	EcDev	Turlag	Reblag		
Best Equation Bl. Africa Data Africa Sample	+ .20 <i>Str.</i> <i>P.D.</i> <sup>c</sup> + .23 - .22	- .56 - .49	+ .46 + .42	+ .79 + .66	- .39 - .27		.66 .70
Replication with Gurr Data	+ .02	- .65	+ .33	+ .55	- .44		.44
D. REBELLION							
Sample and Data Set	Determinants of Rebellion					R <sup>2</sup>	
	R.I.S.	Tension	Exint				
Gurr-Duvall 86 Nations (Rebellion)	- .11 (- .25-+ .03)	+ .52 (+ .81-+ .23)	+ .22 (+ .50-+ .06)				.63
Gurr Data Africa Sample	- .21	+ .11	+ .61				.61
Bl. Africa Data Africa Sample	- .02	+ .86	+ .05				.63
	Strain	EcDev	D.I.S.	Tension	Exint		
Gurr-Duvall 86 Nations (Deaths)	+ .33 (+ .58-+ .08)	- .02 (- .15-+ .11)	+ .10 (+ .27-+ .07)	+ .47 (+ .76-+ .18)	+ .15 (+ .49-+ .19)		.72
Gurr Data Africa Sample	+ .19	- .08	+ .28	.00	+ .53		.69
Bl. Africa Data Africa Sample	+ .03	+ .06	+ .28	+ .64	+ .13		.70
	D.I.S.	Exint	Turlag	Turmoil			
Best Equation Gurr Data Africa Sample	+ .33	+ .69	- .27	+ .36			.70
Replication with Black Africa Data	+ .42	+ .30	+ .42	- .19			.64
	D.I.S.	Tension	Exint	Turmoil			
Best Equation Bl. Africa Data Africa Sample	+ .32	+ .90	+ .07	- .34			.69
Replication with Gurr Data	+ .36	- .17	+ .61	+ .30			.68

eter estimates generated by Gurr and Duvall. In addition, best descriptive equations are generated for each African data set and replicated in the other to assess correspondence of results for the same sample across data sets. To sort out sampling and measurement distortions, the following criteria are used:

(1) If both of the African data sets generate equations similar to Gurr and Duvall's, neither sampling nor measurement error will be considered serious enough to bias results.

(2) If the two African data sets generate similar equations, and both equations differ from Gurr and Duvall's, then we will conclude that the general model is biased by sampling error.

(3) If one African data set generates equations corresponding to Gurr and Duvall's, and the other African data set differs, then the latter data set will be considered the most erroneous. Error in the first will be considered not serious enough to bias results, while error in the second distorts results. Sampling error by Gurr and Duvall will be concluded to be inconsequential.

(4) If both African data sets generate equations that differ from each other and from Gurr and Duvall's, then probably both sampling and measurement error are distorting results. Differences between the data sets are explained by measurement error and differences of each from the Gurr and Duvall model would be due to measurement error for Africa, as well as sampling error and/or measurement error in the 86-nation data.

Tables 6A through 6D report the relevant equations for a comparison of results across samples and across data sets. If the coefficients for the African data sets fall within the confidence bounds we will conclude that parameter estimates correspond and that the Gurr and Duvall estimate is representative. In comparing the two African data sets, however, more impressionistic criteria are employed since the African nations clearly do not constitute a probability sample. Here we will simply look for a correspondence of form, comparing the relative size and sign of coefficients.

An inspection of Table 6A leads to several conclusions concerning the effects of error on models of stress.<sup>24</sup> First, sampling error exists in the general model and is evident in the overestimation of the impact of wealth, the possible underestimation of the effects of past conflict,<sup>25</sup> the weaker effects

of strain in Africa, and the appearance in both best African equations of regime institutional support. The weaker effects of economic development and strain corroborate inferences made above that these two concepts behave differently in Africa. Second, systematic error probably causes the incorporation of different concepts in the African equations. The inclusion of rebellion could reflect sampling error but most likely is caused by systematic error shared with stress. The exaggerated importance of past turmoil in all equations probably has similar roots. The similarity of the best African equations, in other words, is in part deceptive and simply may reflect the systematic error shared between rebellion and stress and between past turmoil and stress. The fact that shared systematic error between stress and past turmoil exists in only one data set, however, suggests that past turmoil has some real impact and that both sampling and measurement error are operating here. Third, error in the dependent variable, stress, drastically affects the structure of the estimated equation. Economic development and dissident institutional support are consistently unstable across data sets. Since the same measure for dissident strength is used for both data sets and since economic development was inferred to be relatively reliable, the instability is most likely caused by error in the dependent variable. The above analysis of measurement error led to the conclusion that the stress measure used by Gurr and Duvall is probably systematically erroneous; therefore, the implication is that for some variable(s) in their model the estimated coefficient is reflecting correlation with "noise" rather than with stress in a fashion similar to that of economic development and dissident institutional support in the African context. In summary, both sampling error in the general model and measurement error in the African data are damaging. Equations based on *Black Africa* data are closer to the general model, but even here results are not encouraging.

A comparison of the equations in Table 6B permits two general conclusions: (1) the sample that Gurr and Duvall utilize slightly biases their conclusions about the causes of external intervention, particularly in underestimating the impact of the primary determinant, rebellion; and (2) measurement error, while admittedly extensive for external intervention in Africa, does not result in grossly divergent results across data sets, except in the case of relating two highly erroneous variables, external intervention and turmoil.

Table 6C provides the relevant equations for turmoil. Two equations are reported for the *Black Africa* data—one employing the basic strain score and the other the strain score excluding political discrimination (see footnote 15)—since the two

<sup>24</sup> Recall that Gurr and Duvall create a couple of stress scores, weighting indicators according to the structure of a regression equation predicting each form of conflict. See footnote 14.

<sup>25</sup> The inclusion of traditions of conflict which is scored differently for the African countries and includes as components past turmoil and past rebellion (see section on Measurement of Concepts), could explain the exaggerated effects of traditions and the weakened effects of past rebellion in the second set of equations.

strain measures produce different results in the case of turmoil. Although there is some convergence between both data sets and the general model, conclusions concerning the impact of sampling error are difficult to draw because of the inclusion of traditions of conflict, which is scored differently for African countries, and tension, which includes traditions as a component. On the other hand, one would anticipate measurement error to produce considerable bias in parameter estimates given previous suggestions that error in turmoil, the dependent variable, is systematic. Attempts to generate best equations in both African data sets do produce extremely divergent results, reflecting differences in shared systematic error. On the one hand, the best equation based on Gurr data includes all of the systematically erroneous variables, and little else, whereas the best *Black Africa* equation includes only past conflict of the systematically erroneous variables. One might infer that the extent of systematic error is greater in the Gurr African data, but that the shared form of systematic error that involves past conflict most likely resides in the *Black Africa* data. The sources of remaining differences, however, seem impossible to disentangle.

The final concept explained in the general model is that of rebellion (Table 6D, where the Gurr and Duvall equation for "deaths" is also reported). Both African data sets provide results similar to the general model, and again the *Black Africa* data set produces the closest correspondence. In replications of both equations, the coefficients for the two African data sets tend to fall at the two ends of the confidence limits, suggesting that measurement error in the African data has strong effects, that the actual parameter falls somewhere in between them, and hence that the general model is relatively accurate. Measurement error is most apparent in the best African equations where turmoil and past turmoil have strong but opposite effects. Sampling error is evident, however, at several points. Both African data sets converge in the deaths equation to demonstrate the weaker effects in Africa of strain and the stronger effects of dissident strength. In addition, both best African equations incorporate dissident institutional support, rather than regime institutional support, corroborating earlier suggestions that the latter concept behaves differently in Africa and implying that dissident support is the more important determinant of rebellion in Africa.

Although the turmoil equations are perhaps too divergent to permit drawing definitive conclusions on the impact of error, equations for the other concepts provide some convergence in evidence. In all cases the *Black Africa* data produce closer correspondence to the general model, and in all

cases there is some evidence of sampling distortions. The effects of measurement error appear to be strongest in the Gurr African data, and hence the decision to exclude African countries from the 86-nation study was probably wise. Moreover, certain concepts do appear to be measured with extensive error, and their inclusion in any analysis, at least of African countries, produces distortions. The most damaging form of measurement error is clearly systematic rather than random. External intervention, the most erroneous variable, has predominantly random error, but distortions resulting from its inclusion are considerably less than from the inclusion of turmoil in which systematic error predominates. The decision of the authors of *Black Africa* not to publish turmoil data in their handbook was apparently wise, since results based on turmoil data are probably too biased to produce meaningful conclusions. Given the extreme error in the turmoil data one might expect error to exist in other samples as well, and hence results based on turmoil ought to be treated with caution in any sample. Knowledge of the extent of error in individual variables, however, does not necessarily help estimate the actual effects that error will have on complex models. For instance, two reliably measured concepts—economic development and dissident institutional support—are highly unstable when related to an erroneous dependent variable such as stress.

While some data and certain concepts are so erroneous to warrant exclusion, omission of certain countries can create distortions from sampling error. Gurr and Duvall do fairly well in setting up general models of conflict (rebellion and perhaps turmoil), but their biased sample leads to unrepresentative models, especially for stress and external intervention. Exclusion of a particular type of nation—the newest and poorest—leads Gurr and Duvall to misrepresent the *general* effects of concepts such as economic development, strain, regime institutional support, and dissident strength. Thus attempts to reduce the effects of one form of error (measurement) have magnified the effects of another form (sampling).

#### Error in Theoretical Specification

A third form of error which can distort model results is that of theoretical specification. The assumptions of regression analysis outlined in the previous section imply that no important variables that correlate with both the independent and the dependent variables are excluded. Their exclusion, like measurement and sampling error, will create biased estimates. Gurr and Duvall explicitly claim to be deductive, i.e., they claim that the model they generate is derived from a "theory" of conflict rather than built inductively. If their theory

is good, then their model should not suffer seriously from specification error. They do, however, modify their deductively derived models in light of data on the 86 nations. Their final specification is in part determined by available data. Hence, the nature of their sample affects the model that is specified, and as a result it is difficult to disentangle specification error and sampling distortions. For example, in the previous section best descriptive equations were presented for the African sample, and several of those equations incorporate terms that are not specified by Gurr and Duvall. The descriptive equations point to possible specification errors in the general model, but the determination of whether they are specification error or distortions caused by differences in samples is impossible to make.

Thus although we are unable directly to assess specification error in the general model, it is possible to investigate the problem in the African sample to obtain some guidelines. Working only with the African sample, we can look for two types of factors possibly omitted. First, there are general factors which should be included in all models of conflict. Second, there are factors specific to Africa which should be included in models of conflict for African countries. It is possible that models of conflict appear to be different in Africa because some variable distinct to and important in Africa has not been controlled. In the language of Teune and Przeworski,<sup>26</sup> this problem involves translating system names (here, Africa) to variable names. In other words, if some variable were incorporated in the model for African countries, other variables would behave as they do in the general model.

Since the *Black Africa* data appear to be the least erroneous and provide results closest to the general model, the following analyses rely solely on those data. Several new variables are incorporated, and best equations are generated to assess if theoretical specification is indeed a problem. One variable, which I have dealt with previously,<sup>27</sup> is conceptualized as a general factor to be incorporated in any model of conflict. The concept is the institutionalization of party systems, which are viewed as linkage mechanisms between publics and governments. One could argue that where there are stable and effective linkages, conflict is less likely; rather conflict is most likely to occur where publics have inadequate channels for articulating interests to governments. Similarly, one

would expect party system institutionalization to inhibit stress, since government sanctions (a major component of stress) are less necessary where linkages are strong. Party system institutionalization has been scored as a lagged variable (1955–60) and as a simultaneous variable (1961–65).<sup>28</sup>

Another potentially relevant general factor omitted by Gurr and Duvall relates to the effects of different forms of conflict on each other. Previous attempts to generate best African equations, as well as the following attempts, incorporate both forms of conflict in the same equation. While Gurr and Duvall do not treat one form of conflict (e.g., turmoil) as a determinant of another (e.g., rebellion), conflict is treated here as another general and theoretically important concept to be incorporated. One might expect that past turmoil might lead to later rebellion, while rebellion could inhibit turmoil.<sup>29</sup>

Other variables are viewed as relevant primarily to the African sample. Mother country (an indicator of the nature of colonial rule), the length of time independent, and the length of period of self-rule are all treated as predetermined variables that might be relevant in models of conflict for newly independent countries. Different colonial powers prepared governments differentially for independence, perhaps affecting later conflict and stress. On the other hand certain countries might have a greater propensity to intervene in the affairs of former colonies. One could also argue that the more mature a country (i.e., the longer self-rule or independence), the less likely are conflict, stress, and external intervention.

Table 7 reports the best equations for the *Black Africa* data set. The stress equation incorporates simultaneous party system institutionalization which apparently acts to reduce stress. Although the strength of the effect of past turmoil on stress is reduced somewhat, creating a closer corre-

<sup>26</sup> Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: John Wiley, 1970).

<sup>27</sup> See in particular Mary Welfling, *Political Institutionalization: Comparative Analyses of African Party Systems*, Sage Professional Papers in Comparative Politics #01-041 (Beverly Hills: Sage Publications).

<sup>28</sup> The author has scored thirteen indicators of party system institutionalization in the black African sample for the period 1945–1970. Previous analyses demonstrated that these indicators fall into four conceptual dimensions. The highest-loading variable on each dimension was selected as representative of that dimension, and the mean standard score of the variables calculated for two time periods, 1955–60 and 1961–65. The four representative variables are percentage of seats held by independents, new party entities, electoral participation, and electoral discrimination (for postindependence years). A fuller explanation of this scoring procedure can be found in Raymond Duvall and Mary Welfling, "Social Mobilization, Political Institutionalization and Conflict in Black Africa: A Simple Dynamic Model," *Journal of Conflict Resolution* (December, 1973); and a full explanation of the scoring of all institutionalization indicators can be found in Welfling, *Political Institutionalization*.

<sup>29</sup> For other suggestions on how different forms of conflict interrelate see Hibbs, *Mass Political Violence*, and Duvall and Welfling.

Table 7. Equations for Estimating the Impact of Error in Theoretical Specification<sup>a</sup>

A. STRESS						
Determinants of Stress						
Turlag	Rebell	R.I.S.	Inst. 61-65			R <sup>2</sup>
+ .45	+ .17	- .23	- .25			.70
B. EXTERNAL INTERVENTION						
Determinants of External Intervention						
Rebell	D.I.S.	Tension	Independence			R <sup>2</sup>
+ .63	- .30	+ .28	- .31			.53
C. TURMOIL						
Determinants of Turmoil						
D.I.S.	R.I.S.	EcDev	Turlag	Reblag	Mother Country	R <sup>2</sup>
+ .22	- .52	+ .39	+ .69	- .39	- .20	.69
Str. P.D. <sup>b</sup>						
+ .20 - .26	- .47	+ .38	+ .58	- .27	- .17	.72

<sup>a</sup> All equations are based on *Black Africa* data for the African sample.  
<sup>b</sup> The second Turmoil equation incorporates the Strain score which eliminates Political Discrimination. P.D. refers to the separate Political Discrimination score. See footnote 15.

spondence to the general model, the basic form of the equation continues to correspond to the best *Black Africa* equation reported in Table 6A. Party system institutionalization may well be an important general determinant of stress, since its inclusion does not render the African results comparable to the general model, and since it has rather marked effects on stress in Africa in a fashion anticipated by theory. I believe that Gurr and Duvall have misspecified in failing to account for the stress-inhibiting effects of institutionalized linkages between publics and governments.

The external intervention equation incorporates as an important variable the length of time independent (the longer an African country has been independent, the less external intervention it experiences during a particular time period). Inclusion of this variable renders the equation more comparable to the general model than was the best *Black Africa* equation reported in Table 6B. The effects of rebellion are increased, while the impact of tension, not a determinant in the general model, is reduced. The negative contribution of dissident support, however, remains exaggerated in the African sample. The length of time independent, then, seems to be a country- or region-specific variable which affects the degree to which similar processes characterize African and non-African politics.

Conflict equations appear little affected by omission of relevant concepts. None of the new

variables improves the rebellion equation, although both of the best descriptive African equations in Table 6D include turmoil. The instability of turmoil coefficients caused by measurement error, however, makes it impossible to discover the "real" effects of turmoil on rebellion. On the other hand, the best African equations reported in Table 6C suggest that past rebellion may reduce future turmoil, indicating an additional failing of the Gurr-Duvall model, which treats each form of conflict as a separate and unrelated phenomenon. Finally, inclusion of mother country in the turmoil equation in Table 7 slightly alters coefficients so as to approximate the general model more closely, but changes are so slight and the increase in R<sup>2</sup> so small that inclusion of another variable is probably not warranted.

**Conclusions**

The purpose of the research reported thus far has been to evaluate one effort to develop a general comparative model of conflict in light of three sources of error common to cross-national research—measurement error, sampling error, and error in theoretical specification. Conclusions are many and mixed, and advocates of diverse positions could find results here to support their cases. Careful conclusions therefore are needed to avoid extreme interpretations.

First, it is clear that the general model suffers from sampling error. An inspection of correlations

across samples suggests that at least three concepts—strain, economic development, and regime institutional support—have weaker effects in Africa than in the 86-nation sample. The development of more complex models supports these suggestions and in addition provides evidence that dissident institutional support and perhaps past conflict have stronger effects in the African sample.

Second, measurement error is clearly present in the African data sets, although the *Black Africa* data appear less erroneous. The extent of error varies considerably across concepts, and both random and systematic error are inferred to exist. Measurement error does create unstable results across the two data sets, particularly when measures with systematic error are involved. Equations explaining turmoil, the most systematically erroneous variable, have no correspondence, and the inclusion of turmoil as an independent variable produces contradictory results. Equations explaining stress, a variable containing some systematic error, include unstable coefficients for what appear to be reliably measured concepts (e.g., economic development), and incorporate as independent variables those which share systematic error with stress (e.g., rebellion). Rebellion equations also differ across data sets, primarily, however, because of the inclusion of turmoil. On the other hand, distortions produced by variables with random error such as external intervention are not so great even though the error is extensive. Assuming that the inferences on the nature of error are correct, one can conclude that fairly large random error is tolerable but even small systematic error is damaging.

Third, the omission of certain potentially relevant concepts from the general model suggests

some error in theoretical specification. The existence of institutionalized linkages between publics and governments seems to reduce stress, while in new nations the length of time independent influences the extent of external intervention. In addition, there was evidence that different forms of conflict affect each other, but interpretations must be extremely tentative due to measurement error in turmoil. This third form of error appears the least severe and is perhaps the most easily remedied.

Although considerable error has been found in these model-building efforts, and although errors certainly do distort parameter estimates, there are still some optimistic conclusions for the general comparativist and for the researcher involved in quantitative cross-national analysis. Some portions of the general model do hold up in the African sample, especially portions explaining rebellion, which is the heart of the model. In addition some African data appear sufficiently reliable so that the results are not distorted. *Black Africa* data provide considerable correspondence to the general model on many concepts, although certain concepts such as turmoil remain too erroneous for meaningful analyses. Two suggestions follow from this discovery. First, data collected by area specialists appear superior to data collected by large cross-national studies, at least for the African countries. Second, general comparativists could and perhaps should use these area data since they appear sufficiently reliable and their inclusion would reduce sampling error. While general models are distorted by some error basic to cross-national research, results clearly represent more than random noise, and there is some basis to believe that errors can be reduced once their nature and effects have been estimated.



# Nondecisions and Power: The Two Faces of Bachrach and Baratz

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Since Bachrach and Baratz published their two articles, *Two Faces of Power*,<sup>1</sup> and *Decisions and Nondecisions: An Analytical Framework*,<sup>2</sup> the concept "nondecision" has become increasingly familiar,<sup>3</sup> and now occupies a prominent position in the hagiology of community power. It was put forward as a means of filling what the authors saw as a gap in the conceptual armory of community studies that had resulted in a misrepresentation of American local politics. This paper looks very briefly at the limitations Bachrach and Baratz noted in the work of Robert Dahl,<sup>4</sup> and then at the two forms of nondecision making their solution appears to take in. The conclusion reached here is that, in its present form, the concept has no empirical value even if it may legitimately be retained to point up a weakness in the literature. But it is only part of their solution. Although "nondecision" is held to identify the gap, it is not advanced as explanation of it. In their view, this has occurred because observers have not differentiated among the various means of resolving relationships, of which power is only one form. This paper examines their proposals and concludes with a more limited view of the significance of power for empirical research.

## Dahl's Approach to Community Politics

The debate over community power has been almost exclusively an American affair. The dominant theme, understandably enough, is the attempt to come to terms, in a systematic way, with the nature of political reality in the American

community.<sup>5</sup> The most authoritative voice in the debate thus far has been that of Robert Dahl, whose decision-making, or issue analysis, method of studying community power involves isolating selected issue areas and showing how crucial decisions are reached within those areas. Since this approach attempts to reconstruct the actual behavior of participants in various important areas of community life, it is held to provide a reliable and representative body of data about what actually goes on in the community. The pluralist conclusions drawn from this, that power tends to be dispersed among a variety of issue-oriented elites, are therefore held to be closer to the reality of community politics in America than are the findings of Floyd Hunter,<sup>6</sup> whose suggestion that Atlanta was dominated politically by a business elite was the spark which set the whole debate going. It was argued that Hunter's elitist conclusions could have been predicted from his reputational methodology.<sup>7</sup> He had, or so he thought, located the "real" holders of power by the simple and inexpensive device of asking strategically located citizens for their opinion of who was powerful, thus confusing hearsay with fact.

How far this apparent conflict is a function of methodology, and how far it reflects real differences in the communities studied, is another matter.<sup>8</sup> It is referred to here simply to put Dahl's

<sup>1</sup> *American Political Science Review*, 56 (December, 1962), 947-952.

<sup>2</sup> *American Political Science Review*, 57 (September, 1963), 632-642.

<sup>3</sup> See for example Matthew A. Crenson, *The Unpolitics of Air Pollution: A Study of Non-Decision-making in the Cities* (Baltimore: The Johns Hopkins Press, 1971); Frederick Frey, "Comment: Issues and Nonissues in the Study of Community Power," *American Political Science Review*, 55 (December, 1971), 1081-1101; K. Newton, "Democracy, Community Power and Non-decisionmaking," *Political Studies*, 20 (December, 1972), 484-547. For a dissenting view see Raymond Wolfinger, "Non-decisions and the study of Local Politics," *American Political Science Review*, 55 (December, 1971), 1063-1080; and see also Wolfinger's "Rejoinder" in the same volume, pp. 1102-1104.

<sup>4</sup> Robert Dahl *Who Governs? Democracy and Power in an American Community* (New Haven: Yale University Press, 1961).

<sup>5</sup> See, for example, the article by Nelson W. Polsby on the "Study of Community Power," *International Encyclopaedia of the Social Sciences* (New York: Macmillan and The Free Press, 1968) III, 157-163, which he opens with the statement that "contemporary research on community power is distinguished by: (1) a concern with characterizing as a whole the political order of an entire community (generally an American local community)" [p. 157, emphasis added].

<sup>6</sup> Floyd Hunter, *Community Power Structure*. (Chapel Hill: University of North Carolina Press, 1953).

<sup>7</sup> Critics of Hunter are legion—see for example Nelson Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); and Arnold Rose, *The Power Structure: Political Process in American Society* (New York: Oxford University Press, 1967).

<sup>8</sup> Rose, pp. 296-297 notes evidence that the size of a community, and its interrelationships with the national economy, may significantly influence the nature of its power structure. This is considered in more detail by John Walton, "The Vertical Axis of Community Organization and the Structure of Power," (*Southwestern Social Science Quarterly*, 48 (1967), 353-368, which has been most recently reprinted in *Com-*

work in context. Hunter's approach devalued the significance of the political process. In a putatively democratic context this was clearly a challenge. Dahl concluded his study of New Haven by arguing that

neither the prevailing consensus, the [democratic] creed, nor even the political system itself are immutable products of democratic ideas, beliefs, and institutions inherited from the past. For better or worse, they are always open, in some measure, to alteration through those complex processes of symbiosis and change that constitute the relations of citizens and leaders in a pluralist democracy.<sup>9</sup>

The central feature of the democratic political community for Dahl, then, is that the relationship between leaders and citizens guarantees the continuing possibility of change because it is the fulcrum of power. Any attempt to describe and analyze must look at this relationship within a context where both sides are motivated to act and, therefore, display the dynamics of the relationship. This can be achieved by observing the decision-making process at work. There are two difficulties which Dahl sees, at first sight, as reducing the significance of this relationship. And, of course, if its significance can be questioned, it would diminish its relevance to any description of community power.

In the first place, citizens are largely apolitical, "strongly influenced by inertia, habit, unexamined loyalties, personal attachment, emotions, transient impulses."<sup>10</sup> A significant political relationship is difficult to envisage where one of the presumed parties to it turns out to be so plainly lacking in interest. Dahl notes that the citizen will engage in political activity when "the actions or inactions of government may threaten . . . (his) primary goals."<sup>11</sup> But even though he examined three issue areas favorable to such activity in his study of New Haven, he found few signs of it. He is quite justified, it would appear, in asserting that "Homo civicus is not, by nature, a political animal."<sup>12</sup>

The second factor which may reduce the significance of a leader-citizen relationship, says Dahl, is the relative freedom of action available to the politician because of the complexity of the political process and the variety of interests to be considered. Thus the calculations of the politician may be critical.

If a party politician sees no pay-off, his interest is

likely to be small; if he sees an adverse effect, he will avoid the issue if he can. . . . Politicians may not see how they can gain by taking a position on an issue; action by government may seem wholly inappropriate; policies intended to cope with dissatisfaction may be blocked; solutions may be improperly designed; indeed, politicians may even find it politically profitable to maintain a shaky coalition by keeping tension and discontent alive and deflecting attention to irrelevant "solutions" or alternative issues.<sup>13</sup>

This sort of maneuvering is quite legitimate within a democratic context. There will be a variety of issues on which the views of constituents are confused, or conflicting. Where there is no clear lead, or support, from the public, the politician must act on the basis of his own calculations.

Yet despite the prevalence of inertia among the "apolitical stratum," and the opportunities for independent action amongst the leaders, the significance of the relationship can continue to be assumed, Dahl argues. The majority of citizens may be politically inactive, but, "although their influence is low, their indirect collective influence is high."<sup>14</sup> And the politicians' actions and beliefs "are all constrained by the wide adherence to the [democratic] creed that exists throughout the community."<sup>15</sup> Dahl plainly believes that this restraint is significant because "to reject the democratic creed is in effect to refuse to be an American."<sup>16</sup> So the creed inhibits the politician and causes him to attend to the wishes of the politically inert citizen, who is thus endowed with indirect influence.

Dahl does not say explicitly that New Haven is America in microcosm. He acknowledges that his "data on New Haven are not wholly adequate for the task at hand," which is a general discussion of "Stability, Change and the Democratic Creed."<sup>17</sup> He nevertheless asserts that "New Haven will provide a convenient reference point" without showing in what way the data are inadequate. Since he does not question the relevance of his findings to the wider context, the implication is that they may be taken as a description of it. Indeed, the innuendo in the statement "the data are *not wholly* adequate" (emphasis added) surely amounts to an assertion that, in large measure, they are. Although many observers felt that his conclusions were justified, others like Parenti,<sup>18</sup> Anton,<sup>19</sup>

<sup>13</sup> *Ibid.*, p. 93.

<sup>14</sup> *Ibid.*, p. 233.

<sup>15</sup> *Ibid.*, p. 325.

<sup>16</sup> *Ibid.*, p. 317.

<sup>17</sup> *Ibid.*, pp. 311-325.

<sup>18</sup> Michael Parenti, "Power and Pluralism: A View from the Bottom," *Journal of Politics*, 32 (August, 1970), 501-530.

<sup>19</sup> Thomas Anton, "Power, Pluralism, and Local Politics," *Administrative Science Quarterly*, 7 (March, 1963), 425-457; "Rejoinder" *Admin. Science Quarterly*, 8 (September, 1963), 257-268.

*munity Politics: A Behavioral Approach*, ed. Charles M. Bonjean, Terry Clark and Robert Lineberry (New York: The Free Press, 1971), pp. 188-97.

<sup>9</sup> Dahl, *Who Governs?* p. 325.

<sup>10</sup> *Ibid.*, pp. 90-91.

<sup>11</sup> *Ibid.*, p. 225.

<sup>12</sup> *Ibid.*, p. 225.

Bachrach and Baratz,<sup>20</sup> and Crenson,<sup>21</sup> believe that they are deceptively optimistic, particularly when they are generalized from the local evidence to American society at large, where, it is clear, the possibility of change is less significant for many than their total inability to effect it.

Dahl argues, then, that statements about power in a community should be based on direct observation, and not on hearsay or supposition. But since the leader-citizen relationship is regarded as the linchpin of pluralist democracy, and the core of community politics, and since there is no significant and observable interaction between its two elements, the relationship has to be rescued by two nonobservables; the leaders' commitment to the democratic creed, and the citizens' indirect influence on the leaders. Once these are accepted as valid, however, they provide the complete justification for focusing on the actions of leaders in pursuit of various objectives in various contentious issue areas, and for ignoring the apparently settled aspects of community life as well as the aspirations of the apolitical. The viability, and value, of the leader-citizen relationship is demonstrated only in those areas determined by public action. Short of public demonstrations to the contrary, Dahl can assume that what the leaders do is more or less what the citizens want. In these terms his restricted focus is justified.

### Nondecisions

**The Response of Bachrach and Baratz.** Bachrach and Baratz believe that the significance of the relationship between leader and citizen may be considerably reduced for either of two reasons. First, when politicians are able to operate out of the public eye, they may not feel themselves bound by any abstract democratic creed, or may so interpret it that they are freely able to ignore the interests of any group of constituents, and that these instances may be more significant than politicians' formal public actions. Alternatively, the most important fact may be neither the full flowering of the leader-citizen relationship, nor the covert machinations of an elite, but the imperatives of the structural setting. Some bias that is inherent in the political process itself, or the political culture, may be more significant than the strivings of either leaders or citizens. Bachrach and Baratz argue, then, that Dahl misses two important fea-

tures of political life—covert control and the mobilization of bias.

In point of fact, they do not treat these as separate forms. The distinction can be inferred from their writing, but it is nowhere made explicit. The result is that such methodological proposals as they make are uninformed by what should have been fundamental to their case. The general thrust of their writing is that covert control and mobilization of bias, however they intend these to be jointly or separately interpreted, may lead to what they call a "nondecision." They define this as "a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision maker."<sup>22</sup> This, they argue, is what an issue analysis approach inevitably overlooks through focusing on "concrete decisions."<sup>23</sup> By these one must assume they mean formal, authoritative decisions arrived at by a constitutionally recognized organization or institution.<sup>24</sup> They believe that such decisions reflect only one face of power.

The question that needs to be asked, then, is whether the nondecision-making concept is a useful addition to the power study vocabulary. It is also worth considering whether the criticisms prompting Bachrach and Baratz's disquisition are properly attributable to a decision-making approach, or whether they are the consequence of Dahl's own partial interests.

**Nondecisions by Covert Control.** The first objection that Bachrach and Baratz make, then, is that the "symbiotic" relationship between citizens and leaders, upon which Dahl places so much emphasis, may turn out to be one in which the leaders, or some hidden elite which in turn controls them, exercise a form of covert control over the whole process. The leaders may not need to take any account of citizen interests. They may be able to

<sup>20</sup> Both the Bachrach and Baratz articles (see notes 1 and 2 above) have been reprinted, together with a sketchy report of an associated empirical study, in their *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970). All further Bachrach and Baratz references will be to this book.

<sup>21</sup> See Crenson, especially pp. 181–182 for a brief, but very penetrating analysis of the logical difficulties posed by the concept of indirect influence.

<sup>22</sup> Bachrach and Baratz, *Power and Poverty*, p. 44. Note that this definition of nondecisions is more restrictive than what is implied by mobilization of bias since it denies the possibility of such bias operating against "the values or interests of the decision makers." Clearly the mobilization of bias may not itself discriminate how it operates, and against whom—as is of the essence of covert control. The failure to distinguish between these two forms of nondecision making creates the confusion which lies at the heart of their approach, and is the justification for Merelman describing them as "neo-elitists." Richard Merelman, "On the Neo-Elitist Critique of Community Power," *American Political Science Review*, 62 (June, 1968), 451–460.

<sup>23</sup> Bachrach and Baratz, p. 9.

<sup>24</sup> They would appear to mean the same class of events as Polsby when he refers to "specific outcomes," and cites examples such as party nomination, urban development programme, and public education. See Polsby, *Community Power and Political Theory*, pp. 113–114.

get their way by behind the scenes manipulation, thus provoking no hostile citizen reaction. The effect of this is that

demands for change in the existing allocation of benefits and privileges in the community can be suffocated before they are even voiced; or kept covert; or killed before they gain access to the relevant decision-making arena; or failing all these things, maimed or destroyed in the decision-implementing stage of the policy process.<sup>25</sup>

Merelman dismisses this argument on the grounds that it presumes an elite in advance of any evidence that one exists. Neither misfortune, frustration, nor inequality can necessarily be taken as the work of a hidden hand—which is an implication of Bachrach and Baratz's approach. Certainly the violence of their description can be dismissed as merely tendentious. After all, the "suffocating, hiding, killing, maiming and destroying" could equally well be directed toward demands for stability in the existing allocation of benefits, where action is necessary to adjust to changing conditions. Or, as Merelman argues, "the neo-elitists formulation of non-decision-making . . . emphasizes only those values which contribute to elite control and support the *status quo*. But are there no 'dominant values,' no 'accepted rules of the game' which favor the initiators of issues?"<sup>26</sup>

The question can, of course, be answered only by locating those instances where this form of nondecision making actually occurs. And if the purpose of all this is a study of community power, it would require a reasonably comprehensive statement covering a communitywide range of nondecisions. Without this one can make no assertion about the community significance of any singular instance, unless one is prepared to suggest that certain nondecisions may be identified and selected for study because they are in some way representative.<sup>27</sup> The issue analysis approach to the study of community power faces the same problem, and resolves it by selecting what are believed to be important issues. Polsby justifies this by asking the rhetorical question, "What sort of power elite asserts itself in relatively trivial matters, but is inactive or ineffective in the most significant areas of community policy-making?"<sup>28</sup>

<sup>25</sup> Bachrach and Baratz, p. 44.

<sup>26</sup> Merelman, p. 459.

<sup>27</sup> See Roy Forward, "Issue Analysis in Community Power Studies," *Australian Journal of Politics and History*, 15 (December, 1969), 26-44, for an exhaustive analysis of the varieties of issue that may occur in a community. But drawing attention to variety is not in itself enough, since the profusion of alternatives simply defeats the objective of focused analysis.

<sup>28</sup> Polsby, *Community Power and Political Theory*, p. 114. This question makes sense only if one is concerned with power comparability. A typical reason for

Bachrach and Baratz argue, however, that "the [pluralist] model provides no *objective* criteria for distinguishing between 'important' and 'unimportant' issues arising in the political arena."<sup>29</sup> Yet they do not establish objective criteria either for selecting nondecisions or for identifying them. Nor do they follow the pluralists' modest precaution of studying a selection of issue areas.<sup>30</sup> In this respect their scheme offers no advantages over the one they criticize.

For example, it is quite clear that this first form of nondecision making, where the barrier to certain forms of action is consciously, but covertly, erected by the "*status quo* defender," is likely to be very difficult to distinguish from the calculations of politicians in confused and complex situations, and leading to inaction, such as Dahl referred to. Methodologically the two classes of events make the same demands. In both cases one is concerned with explaining something which does not necessarily have a public face. In fact, "deflecting attention to irrelevant 'solutions' or alternative issues,"<sup>31</sup> as Dahl puts it, is precisely, although not solely, what Bachrach and Baratz mean by nondecision making. They argue, though, that Dahl is unable to deal with this because his methodology (and, by implication, all decision-making analysis) requires him to focus on an "examination of a series of concrete cases where key decisions are made."<sup>32</sup> But the question is whether this limitation is a function of decision-making analysis generally, or whether it is a peculiarity of the line taken by Dahl. If it is the latter, then the answer to their problem may lie, simply, in a sensible application of decision-making analysis techniques—in which case they may have been indulging in a case of special pleading on behalf of a useless neologism.

In examining politics in New Haven, Dahl was chiefly concerned with asking, "How are important political decisions actually made?"<sup>33</sup> and he focused on "important decisions requiring the

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such a preoccupation with *that* seems to follow from the belief that the sharing of power is a crucial index of democracy. (On this point, see L. J. Sharpe, "American Democracy Reconsidered: Part II and Conclusions," *British Journal of Political Science*, 3 (April, 1973), 129-167, at p. 135). Such an approach tends to ignore the style and content of politics. One should not only consider who governs, but how, and in relation to what.

<sup>29</sup> Bachrach and Baratz, p. 6.

<sup>30</sup> Polsby, p. 113. "More than a single issue area is always chosen . . . because of the presumption among pluralist researchers that the same pattern of decision-making is highly unlikely to reproduce itself in more than one issue area."

<sup>31</sup> Dahl, *Who Governs*, p. 93.

<sup>32</sup> Bachrach and Baratz, p. 9.

<sup>33</sup> Dahl, p. 7.

formal assent of local government officials."<sup>34</sup> In an Appendix he notes that the "three issue areas were chosen because they promised to cut across a wide variety of interests and participants . . . and in each of these issue areas all the decisions that the participants regarded as the most important since about 1950 were selected for detailed study."<sup>35</sup> Now it is clear that these research procedures do not necessarily follow from the initial question. While there is no denying the relevance of the issues selected, a definition of "important political decisions" does not require that a decision not to act, or a behind the scenes maneuver, or supportive, as distinct from initiatory, behavior, be ignored. That Dahl does ignore such aspects of the political scene is incontestable.<sup>36</sup> But this oversight was not determined by the general questions with which he prefaces his inquiry.

The reason is not hard to suggest. Dahl was, perhaps, more concerned with producing a reasoned response to the views of Hunter, who pictured an American community as controlled by big business, than with establishing a general picture of community politics. "The question is," he wrote two years after the publication of *Who Governs?*, "whether businessmen or related groups, social strata or classes, also dominate the decisions of public government . . . if not, what groups or strata have the most influence over the decisions of public governments."<sup>37</sup> He justified this particular focus by arguing that it was "the arena in which the controversy over 'community' power arises."<sup>38</sup> But is community power simply a function of group conflict over governmentally controlled decisions? This is as much a matter for investigation as is the question of who governs. In a critique of Dahl's study Anton complains, "Surely the study of power must involve more than the actions of government agencies."<sup>39</sup> Whatever one may believe to be the case, the answer needs demonstrating rather than merely asserting.

There is no reason, for example, why the decision-making analyst should not decide that the most important decision in community X, reputedly democratic, is that which has resulted in, and continues to maintain, the exclusion of forty per cent of the citizens from any effective part in the political process. This was Bachrach and

Baratz's concern in examining "political interaction between the black poor in Baltimore and the white-dominated 'Establishment',"<sup>40</sup> and which they sought to explain by using the concept "nondecision making." But, if an observer wants to account for some problem in a community, he may do so either by reference to decisions taken, or to some process which is the embodiment of past decisions, or to the fortuitous working of circumstances. Our understanding is not advanced by putting forward "nondecision making," as if another category of possibilities existed. In this sense, then, the term "nondecision making" is superfluous.

**Nondecisions through Mobilization of Bias.** Bachrach and Baratz's first form of nondecision making, then, adds nothing to Dahl's general discussion of power. They are correct to point out that his empirical work fails to exemplify the range of his discussion, but this applies with equal force to their own efforts in this field. They add neither theoretical nor methodological perspective and are wrong to assume that any limitation in Dahl's work is justification for a new approach.

But it seems that they go further in their criticisms and argue that the relationship is also distorted by the mobilization of bias. Both leaders and citizens may be affected by this because, as they point out by quoting Schattschneider, "all forms of political organization have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because *organization is the mobilization of bias*. Some issues are organized into politics while others are organized out."<sup>41</sup> Yet their research strategy does not suggest any means of examining or assessing the significance of a mobilization of bias. In fact, there is some confusion in their thinking here. It is easy to follow through their reference to killing, maiming, suffocating, etc., by whatever means are available to the "status quo defenders."<sup>42</sup> This, as noted above, can be put under the general heading of "covert control." Since this is rational, purposive, calculation it need not be connected in any way with the mobilization of bias. It is not so easy, though, to see the value of their reference to the mobilization of bias except as a general legitimation of their claim that there are more things in their political universe than are dreamt of in Dahl's philosophy.

From what they have written it appears that the mobilization of bias is itself to be considered a form of nondecision making as well as, and

<sup>34</sup> *Ibid.*, p. 102.

<sup>35</sup> *Ibid.*, p. 333.

<sup>36</sup> See Anton, "Rejoinder," p. 265; and also Peter Morriss, "Power in New Haven: A Reassessment of 'Who Governs?'," *British Journal of Political Science*, 2 (October, 1972), 457-465, at pp. 459-460.

<sup>37</sup> Robert Dahl, "Reply to Anton's Power, Pluralism and Local Politics," *Administrative Science Quarterly*, 7 (September, 1963), 250-256, at p. 254.

<sup>38</sup> Dahl, "Reply to Anton," p. 254.

<sup>39</sup> Anton, "Power, Pluralism and Local Politics," p. 453.

<sup>40</sup> Bachrach and Baratz, p. 53.

<sup>41</sup> E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, Inc., 1960), p. 71; cited in Bachrach and Baratz, p. 8.

<sup>42</sup> Bachrach and Baratz, p. 43.

separately from, the actions of individuals in support of, or modifying it. For in discussing "such bias" they write, "To the extent that a person or group—consciously or unconsciously—creates or reinforces barriers to the public airing of conflicts, that person or group has power."<sup>43</sup> If someone does unconsciously create or reinforce a barrier, however, it is unrealistic to say that they are, for that reason, powerful. My style of speech or mannerisms may, for example, inhibit someone else's actions or opinions without my wishing this to be so. In fact it may be my wish that they should not be inhibited. If that is so it would seem rather peculiar to say that, by unconsciously accomplishing what I would not have wanted to happen, I have therefore demonstrated my power. Where my actions do have such an effect, whether I subsequently approve of the outcome or not, then presumably it is because it is customary for some to respond in that sort of way to that sort of stimulus; in which case, it is more proper to impute power to the customary basis of behavior rather than to the behavior itself. In other words, the mobilization of bias may itself be recognized as the effective agent. It is as tangible a fact of life as the overt actions of some who may exploit it for their own benefit. The question is how one makes it the subject of a research exercise.

Its existence, and its significance, can be determined only by reference to some datum point since not only is organization the mobilization of bias, but so, in a sense, is civilization itself. The observer must attack the problem from a perspective which he determines himself, or which emerges from the community he chooses to study. He may thus seek to analyze community interaction in terms of some notion of justice or equality, or in terms of the pursuit of a neglected issue he nominates as of some significance. In addition he may analyze unsettled grievances held by citizens, perhaps established by a sample survey. In neither case is anything proposed here which has not already been touched on by Bachrach and Baratz, and Crenson. Either way the observer has to focus on specific aspects which are believed, on whatever grounds, to be a manifestation of bias.

Now asking the question, "Why did this, or that, *not* happen?" makes the same methodological demands as its opposite. There is, of course, the possibility that the mobilization of bias may be so pervasive, so diffuse and insidious that it simply cannot be captured in action, rule, or speech. But this resurrects, in a different form, the "principle of infinite regress" and may be safely left to the metaphysicians.<sup>44</sup>

<sup>43</sup> *Ibid.*, p. 8.

<sup>44</sup> See Robert Dahl, "Critique of the Ruling Elite Model," *American Political Science Review*, 52 (June, 1958), 463-469; and Polsby, *Community Power and Political Theory*, p. 34.

**Identification of Nondecisions.** Bachrach and Baratz have argued, then, that Dahl has failed to establish sufficiently comprehensive criteria for selecting important decisions, and that this oversight leads him to ignore important aspects of community politics. Crenson, in his study *The Un-Politics of Air Pollution: A Study of Non-Decisionmaking in the Cities*, in which he first sets out a detailed case study of the differing responses of two neighboring American communities to almost identical air pollution problems, and then uses some very slender data from fifty-one cities to test the propositions thrown up by the case studies, agrees with this argument. He believes that the "power to restrict the scope of the political process . . . is not revealed by the investigation of political activities in key issue areas because the issues in which this kind of power is likely to become significant are precisely the ones that never become 'key'."<sup>45</sup> But "which non-events are to be regarded as significant?," Polsby wants to know.<sup>46</sup> Bachrach and Baratz say go and ask the locals.<sup>47</sup> Crenson gets round the problem by arguing that significant nonissues are—well, significant in the way that "the prevention of elephant stampedes or the persecution of witches"<sup>48</sup> in contemporary America is not. In other words, the researcher should use his common sense. There is everything to be said for this, but rather than pointing up the need for a new concept, it merely stresses the cruciality of defining "key issue" in a way that is not restrictive.

Crenson attempts to establish the nature of the problem more firmly by defining nonissues as topics which have not been included on a "community's political agenda."<sup>49</sup> These can be identified because "only when advocacy or opposition has made an appearance among community leaders will we say that a topic has taken its place on the local agenda."<sup>50</sup> "Community political systems," he writes, "may be 'impenetrable' where certain issues are concerned."<sup>51</sup> In other words, nondecision making prevents issues crossing the boundary from the nonpolitical to the political system.<sup>52</sup> Since Crenson's definition throws an em-

<sup>45</sup> Crenson, p. 21.

<sup>46</sup> Polsby, p. 97.

<sup>47</sup> Bachrach and Baratz, pp. 47-51.

<sup>48</sup> Crenson, p. 26.

<sup>49</sup> *Ibid.*, p. 29.

<sup>50</sup> *Ibid.*, p. 30.

<sup>51</sup> *Ibid.*, p. 17.

<sup>52</sup> The notion of "boundary" is descriptively useful so long as one need not be precise about where it actually is. It is useful, that is, as a means of drawing attention to gross distinctions between general classes of variables. Nondecision making, however, requires that the precise boundary between political and nonpolitical be determined for it is only at this point, in Crenson's terms, that it can operate. For the difficulties of boundary definition see Samuel E. Finer, "Almond's Concept of 'The Political System': A Textual Critique,"

barrasingly large area of public discussion into the "nonissue" category, we obviously need rather more selective criteria to determine what topics are kept off the "political agenda" through nondecision making. After all, every issue, whether eventually successful or not, must go through some perinatal obscurity. We have no means of determining that the alleged nondecision has been kept out and is not, in fact, going through a lengthy process of legitimation. Drawing the political boundary in one place rather than another has the effect of creating two classes of event where there may be only one.<sup>53</sup>

If we look more closely at Crenson's argument that nondecision making power can be significant only where issues do not become "key," we can recognize the weakness of the nondecision case in its present form. Since Bachrach and Baratz, and Crenson in his case studies, have all focused on what clearly have become key issues in their communities, it is plain that, in the terms of Crenson's definition, none of them can have been examining significant nondecision-making power. Both issues were, as Polsby would argue, eminently researchable by decision-making analysis techniques.<sup>54</sup> If Crenson wishes to establish the lexical convention of calling neglected, or nascent, topics "non-issues," there can be no objection. What is objectionable is the claim that this provides a new and empirically viable form of political analysis.

### Power

The concept "nondecision" does not provide any new means of identifying and studying the less apparent features of community life that Dahl has been accused of ignoring. It offers nothing that is not already provided by a decision-making ap-

proach. But that is, of course, merely a method of inquiry. In examining power in the community<sup>55</sup> the kinds of data collected should be determined by our understanding of the concept "power" and not by the methodology employed.

One can at least say of the reputational approach to the study of community power that it starts from a fairly clear idea of both those linked concepts. Power, for example, involves "the acts of men going about the business of moving other men to act in relation to themselves or in relation to organic or inorganic things."<sup>56</sup> And community is the complex of socioeconomic relationships within which such a faculty is sustained. Polsby correctly points out that such a view presupposes that "power is a subsidiary aspect of the community's social structure."<sup>57</sup> It is an attempt, nevertheless, to relate political activity to its community environment. "The first and most basic presupposition of the pluralist approach," writes Polsby, however, "is that nothing categorical can be assumed about power in any community."<sup>58</sup> Dahl assumes, instead, the priority of rational self-interest within the vaguely limiting framework of the democratic creed. His approach to power, for example, is that "A has power over B to the extent that he can get B to do something that B would not otherwise do."<sup>59</sup> The effect of this approach, since it focuses on the act of individual choice, is to imply that such choice is the most significant aspect of the polity. The community exists only as "an aggregation of individuals."<sup>60</sup> As much may be said of Crenson who accepts the pluralist view of power<sup>61</sup> and ignores community entirely. Bachrach and Baratz have not sought to come to terms with the community

*Government and Opposition*, 5 (Winter, 1969-70), 3-21.

<sup>53</sup> It is, of course, a perfectly legitimate undertaking to inquire why a community did not do this or that. If it can be shown that inaction was intentional, then it concerns community power. But the result would be a community study only in the most limited sense. Crenson's critique of the pluralist position, particularly of the muddy notion of indirect influence, is extremely well argued. And he displays considerable ingenuity in his study of factors influencing the pollution issue in fifty-one cities. But since these cities are reduced to little more than statistical artifacts, he entirely disposes of the community baby with the pluralist bath water. Of course, in one sense, his is not a community study. But it is so clearly advanced as part of a continuing debate on community power, seeking to validate the current focal concept of that debate, that it must be judged in terms of its contribution in that direction, and not simply on the more limited terms of its subtitle, i.e., "Non-Decisionmaking in the Cities."

<sup>54</sup> "Insofar as . . . goals are in some way explicitly pursued by people in the community, the method of study used in New Haven has a reasonable chance of capturing them." Polsby, *Community Power and Political Theory*, p. 97.

<sup>55</sup> For sociologists, community has been a key concept, although Colin Bell and Howard Newby, *Community Studies* (London: Allen and Unwin, 1971), point to a current disenchantment with the term (pp. 48-53). Political scientists have not, however, tended to regard this as a problem requiring much attention. Polsby, "Study of Community Power," p. 157, notes that community power researchers have adopted a conventional perspective by defining community "as a population living within legally established city limits." This view was also taken by L. T. Hobhouse, *Social Development* (London: Allen and Unwin, 1924) who regarded "all populations living under a common rule as political communities, though they have only the bare bones of a common life" (pp. 41-42). Since "community implies having something in common" (Ronald Frankenberg, *Communities in Britain: Social Life in Town and Country* [London: Penguin, 1966], p. 238), it seems reasonable to take the common element as the defining characteristic.

<sup>56</sup> Hunter, *Community Power Structure*, pp. 2-3.

<sup>57</sup> Polsby, p. 7.

<sup>58</sup> *Ibid.*, p. 113.

<sup>59</sup> Robert Dahl, "The Concept of Power," *Behavioral Science*, 2 (July, 1957), 201-215, at pp. 202-203.

<sup>60</sup> Anton, "Power, Pluralism and Local Politics," p. 445.

<sup>61</sup> Crenson, p. 34.

concept. But they have made an elaborate attempt to resolve the power problem, an attempt which falls between the two extremes offered by Hunter and Dahl. It avoids putting forward definitions at the community level of generality, but goes beyond Dahl's focus on the act of power by insisting that there are a variety of types of relationship which shape the character of the community. They argue that the failure to recognize this variety has caused observers to overlook what is implicit in their general notion of nondecision making.

**The Bachrach and Baratz Proposals.** In examining the two faces of power, Bachrach and Baratz argue, one must distinguish among power, authority, influence, force and manipulation, because "while decision-making frequently does involve power relationships, it very often does not."<sup>62</sup> Now, the point of interest here does not lie in the definitions that they create to sustain these differences, but how these are to be applied in an empirical study. For this purpose Bachrach and Baratz specifically refer to several indicators, all of them positively associated with power, and either positively or negatively associated with the other concepts. The conditions that must be present, for example, before they would agree to the use of the concept "power" to describe the means of arriving at some conclusion to a situation would be as follows.

- (1) There is a conflict of values between *A* and *B*;
- (2) *A* threatens *B* with severe sanctions in the event of *B*'s noncompliance;
- (3) *A*'s demand, and the severity of the sanctions, are rationally perceived by *B*; and
- (4) *A* gets his way with *B*'s compliance.

They feel impelled, however, to make explicit the most general feature of power from which these more specific conditions follow. Power, they argue, is a relational concept.<sup>63</sup> The consequences of drawing particular attention to relationality will be discussed after the value of each of these four indicators—conflict, sanctions, rationality, and compliance—has been examined briefly.

(i) *Conflict.* The first question that needs asking is whether we should be concerned only with the overt signs of conflict, the public words and actions that express an opposition of interests; or whether we should also include the psychological state, the mere awareness of opposed interests. Bachrach and Baratz include both in their scheme; "a power struggle exists, overtly or covertly, either when both sets of contestants are aware of its

existence or when only the less powerful party is aware of it."<sup>64</sup> But, having made the general statement, Bachrach and Baratz do not satisfactorily answer the second question that needs asking. How does one then identify conflict? Instead, in a section misleadingly entitled "Empirical Identification of Nondecisions," they shift the question to "what persons or groups in the community are especially disfavored under the existing distribution of benefits and privileges?"<sup>65</sup> This requires an entirely different set of questions being asked about the scope and purposes of government. Thus they leave the matter unresolved. The final question to be put is whether conflict is a necessary indicator of power. This can be best answered by posing a counter question. Do Bachrach and Baratz mean to say that power may not be exerted to prevent conflict? Merely asking the question suggests the inadequacy of such a view, therefore this first indicator of power may be summarily dismissed.

(ii) *Sanctions.* Bachrach and Baratz do not make it clear if they intend to place their several concepts on some notional "sanctions scale," ranging from the case of manipulation (to which sanctions are held to be not relevant), through influence and authority where there are "no severe sanctions," and power, where the "threat of severe sanctions" applies, to the case of force which requires the actual "application of severe sanctions."<sup>66</sup> Elsewhere they imply that influence involves no sanctions at all. For example, "the exercise of power depends upon potential sanctions, while the exercise of influence does not."<sup>67</sup> In their discussion of authority no reference is made to the subject.<sup>68</sup>

In view of their emphasis on severity, however, it is reasonable to assume that they mean to imply a hierarchy of sanctions. But this raises the whole question of establishing an acceptable "severity scale" against which the interpretation of the sanctions by the "patient" in the power relationship can be measured. This is crucial to their design since severity of sanction is the only significant means of distinguishing power from authority and influence.<sup>69</sup>

Dahl discusses this point in these terms. "Exactly what constitutes a 'severe' loss or deprivation is, to be sure, somewhat arbitrary. No doubt what a person regards as severe varies a good deal with his experiences, culture, bodily conditions, and so on. Nevertheless, probably among all

<sup>64</sup> *Ibid.*, p. 50.

<sup>65</sup> *Ibid.*, p. 50.

<sup>66</sup> These references are all to Bachrach and Baratz, p. 37, Table 1.

<sup>67</sup> *Ibid.*, pp. 30-31.

<sup>68</sup> *Ibid.*, pp. 32-36.

<sup>69</sup> *Ibid.*, p. 37.

<sup>62</sup> Bachrach and Baratz, p. 42.

<sup>63</sup> *Ibid.*, pp. 19-21.



peoples exile, imprisonment and death would be considered severe punishments."<sup>70</sup> If such an approach, with its disarming "somewhat," "no doubt," and "probably," is of any use, it certainly gives no help to the student of community power. But it is the sort of limp conclusion that one is driven to if one insists on a necessary connection between power and severity of sanction. It also places an unjustifiable limitation on the way we use the word "power." For if *A* is able to get his way without resorting to such sanctions, it would be decidedly anomalous to say that he was, therefore, not powerful.<sup>71</sup>

(iii) *Rational Perception*. Bachrach and Baratz maintain that

power has a rational attribute: for it to exist, the person threatened must comprehend the alternatives which face him. . . . In a situation involving power, *B* is rational in the sense that he chooses compliance instead of defiance because it seems the less of two evils. In a situation involving authority, *B* complies because he recognizes that the command is reasonable in terms of his own values; in other words, *B* defers to *A*, not because he fears severe deprivations, but because his decision can be rationalized.<sup>72</sup>

As far as influence is concerned (the only other concept characterized by rationality), Bachrach and Baratz are less helpful. They do state explicitly that "power and influence are alike in that each has both rational and relational attributes."<sup>73</sup> But we are left to infer the nature of such rationality from the example of "the ambitious young man who submits unhappily to the every dictate of his rich uncle . . . because he admires wealthy men (influence)."<sup>74</sup>

They are arguing, then, that rationality is a sufficiently notable characteristic of these three concepts to merit particular mention. But consider the significance of what they are saying. In the case of a "power relationship" *B* is rational because he is dominated by fear; in the case of authority *B* enjoys the vicarious gratification of

cherished values and is, therefore, rational; while rationality in their example of influence seems to mean no more than abject self-abasement. The resolution of a situation in any of these terms is held to be rational, whereas resolution by force is not because, presumably, the act of force does not depend for its success on *B*'s perception.

Notice that, in the case of power, authority, and influence, *B* is alleged to be rational because he has chosen to comply. There is an opportunity after the stimulus for *B* to behave in whichever way he wishes; but there must, by definition, be some limitation on this freedom implicit in the initial stimulus. The rationality of subsequent behavior is, therefore, relative to the restricted definition of the situation contained in the stimulus. In the case of power, authority, and influence, however, *B* has no control over the stimulus, whereas in the case of force the stimulus is, in a sense, *B*'s choice. It is *B*'s definition of the situation in as real a sense as agreement is *B*'s choice in the case of the other concepts. To deny rationality in this case, while claiming it for any emotional and submissive response, is highly tendentious.

The purpose of this brief discussion, however, is not only to demonstrate that Bachrach and Baratz use the term "rational" in a rather inadequate manner, but also to suggest that there is little point in trying to base a definition of rationality on the subjective state of the particular actors in a specific situation, because there are, then, no valid grounds for distinguishing between rational and irrational behavior. Yet as Frohock points out, "If criteria are introduced to allow the rational-irrational distinction, then the analytic construct gets away from that which it is trying to explain, which is the way in which actors ascribe meaning to what they do."<sup>75</sup> To specify rationality as a condition of power is unnecessary, then, because it raises greater problems than it may solve.

(iv) *Compliance*. One of the requirements that must be borne in mind in creating definitions of abstract concepts is how far the terms of the definition vary from conventional usage. A definition may, of course, be as arbitrary as the special purposes of the analyst require. But the more commonplace the concept, the less such semantic deviance is acceptable. Power is a commonplace. It is part of this commonplace to recognize that a man who habitually gets his way is powerful. One does not delay such recognition for the testimony of compliant former opponents. It is, thus, a further limitation on power to suppose that it can apply only to those cases where the patient is

<sup>70</sup> Robert Dahl, *Modern Political Analysis*, 2nd ed. (Englewood Cliffs, New Jersey: Prentice Hall, 1970), pp. 32-33.

<sup>71</sup> We continue to associate power with the ability to inflict severe sanctions, but it is not a necessary association. As Anthony de Crespigny, "Power and its Forms," *Political Studies*, 16 (June, 1968), 192-205, points out, "If it is wished to make 'power' a technical term in the social sciences, it must be stripped of its dyslogistic associations. It must be used without any limitations concerning the ways in which power may be said to be exercised" (p. 193). This analysis has benefited considerably from de Crespigny's preliminary discussion of power, although his major concern, to distinguish between various types of power, is not felt to advance the cause of empirical political analysis.

<sup>72</sup> Bachrach and Baratz, pp. 22 and 34.

<sup>73</sup> *Ibid.*, p. 30.

<sup>74</sup> *Ibid.*, p. 31.

<sup>75</sup> Frederick Frohock, *The Nature of Political Inquiry* (Homewood, Illinois: The Dorsey Press, 1967), p. 137.

compliant. This would reduce success, which we intuitively suppose to be the chief feature of power, to a subordinate status.

If compliance is made into a condition, then it follows that power is a term describing a whole developing sequence, spread over whatever period of time, whose two boundary points, conflict and compliance, are separated by threat of severe sanctions and rational perception of these. This is unwieldy, particularly since it requires that the power relationship ceases to exist, for lack of conflict, the moment the patient complies with a command. It creates the logically difficult position that compliance both creates a power relationship and terminates it.

(v) *Relationality*. The foregoing discussion has indicated the difficulties raised by these indicators of power. More than this there is no sign that they are specifically directed toward the initial problem, which involved an attempted clarification of what Bachrach and Baratz describe as the "Two Faces of Power." They have built these concepts on a set of terms which describe an individual state rather than a political relationship—hence the need to emphasize relationality.

The objection to such an emphasis is, quite simply, that there is no concept in the social sciences that is not relational in some sense. Sociopolitical reality cannot be conceived in terms other than those which involve, pertain to, or imply, a relationship. It is therefore, quite worthless to propose relationality as a *distinguishing* characteristic. Bachrach and Baratz argue that manipulation does not involve a relationship.<sup>76</sup> But all they can mean by this is that they believe a relationship exists only when both parties are conscious of each other, and also of the terms which are held to constitute the relationship. Such symmetry is, however, neither necessary to comply with conventional usage, which would not insist on a direct link being established, nor reasonable in the demands it makes for empirical analysis. Relationality is emphasized, of course, as a rebuttal of the view that power is a commodity that can be possessed by one person outside the context of any relationship. But why go to such lengths to refute what is, after all, irrefutable, and equally undemonstrable?

It is, then, impossible to regard relationality as a characteristic of specific "power events" in an otherwise nonrelational political field. This latter is, to steal a phrase from Bentham, "nonsense on stilts." Bachrach and Baratz argue that one must "distinguish clearly between power over people and power over matter."<sup>77</sup> They overlook the rather

banal point that the political scientist does so by choosing not to study matter but people in their political relationships.

#### **Intention and Effect: The Contextual Alternative.**

If these unnecessary associations are removed from the discussion of power, we are left with an agent and an effect, and the presumption that the agent intended the effect. In this view, then, power is simply "the production of intended effects."<sup>78</sup> If a term is used to describe an effect, however, it cannot also be used to explain its cause. For example, while we use the word "bang" to describe the effect of detonating an explosive, only a child would use the same word to describe the explosive itself. We develop the ability to distinguish between cause and effect. There is no reason to suppose that power should be treated any differently. If it is, then one is faced with the question of what creates power, and the answer would have to be power. In which case one would be driven to conclude that power is an inherent property of the power resource, which is an absurdity. Although we say, for example, that money is power, the expression is clearly an ellipsis. We know quite well that some other agency must intervene before the connection is made. This would be either an individual who actively uses money as a resource, or the institutionalization of money which may result in others responding to what they believe to be the wishes of the wealthy without any overt prompting to do so by the wealthy themselves. Money is, potentially at least, power. But power potential cannot be the same as power, by definition.<sup>79</sup> If we continue using the same word to describe both we confound understanding by obscuring what is to be explained.

Any approach to power which ignores the distinction between cause and effect, then, is bound to lead to difficulties. But in conventional usage we frequently describe someone as powerful outside the context of any particular situation. We may thus describe an agent as powerful where known to have been successful in the past, and believed to retain the capacity for success. We would say that he "possesses" power, or "is powerful." In his case, we would imply, power is a cause of success, and it is because of this that he can get his way. Yet such an imputation is justified by past actions, or by our ready assumption of what, in those circumstances, ought to constitute power. We could not apply these terms to anyone who, although possessing all the objectively appropriate resources, has never appeared to have been intentionally successful.<sup>80</sup>

<sup>78</sup> Bertrand Russell, *Power: A New Social Analysis* (London: Allen and Unwin, 1938), p. 35.

<sup>79</sup> See Rose, *The Power Structure*, pp. 45–53.

<sup>80</sup> See de Crespigny, pp. 192–196.

<sup>76</sup> Bachrach and Baratz, p. 37.

<sup>77</sup> *Ibid.*, p. 19.

So although conventional usage refers to power as cause as well as effect, it does so only where there appears to be some known, or assumed, record of success. It is a convenient simplification where one does not know, or one does not choose to spell out, why such success has been possible. That general usage will be retained here. Power may be variously referred to as being created, sought after, possessed, exercised, structured, eroded, fragmented or lost. Nothing more than the processes related to the "production of intended effects" is implied. Power, then, has the same limited, but graphic, value as "bang." Recognizing the value of a term which points up the salient features of our concern does not mean that we should be led into believing that that term is sufficient explanation in itself. It is more important to consider the context in which power may be discussed.

One could argue that this is little different from the approach adopted by Dahl, which also isolates a reference point and attempts to encapsulate it within a wide range of explanatory data.<sup>81</sup> However, the similarity should not mask the divergent consequences for empirical analysis.

Dahl's approach to power is that "A has power over B to the extent that he can get B to do something that B would not otherwise do."<sup>82</sup> If this is adopted as a guide to research, one must first identify two parties in interaction, and then establish a change of position by either participant which can be attributed to the efforts of the other. The inevitable result is that one must focus on the dynamics of a concretely verifiable relationship. It is not possible for such an approach to deal with the problem of inaction, which is just as relevant to the discussion of power.

The definition adopted here shifts the focus instead to the *intention* to produce an effect, as well as the *effect* itself. It does not require that movement be demonstrated in the position of any other party. This means that empirical analysis is freed from a preoccupation with the intricacies of an ascertainable relationship. It means also that the intentions of actors in the community, institu-

tional as well as individual, must be given equal status with the problem of effects—which has, so far, received the major emphasis.<sup>83</sup> This implies, of course, that one must establish a causal relationship between intention and effect before saying with any certainty that one has identified an instance of power. While this may be an ideal position to work toward, one can never demonstrate conclusively in the social sciences that any given factor is both necessary and sufficient for the production of any given effect. This difficulty indicates the importance of circumstantial evidence. It is more useful to formulate an approach which deals with the context of power than to attempt to capture power *in flagrante delicto*, although one naturally anticipates that specific conclusions may be drawn from such a general account.

**Conclusion.** This discussion suggests that the concept "nondecision" is of no practical value in the analysis of community power, because it masks the useful distinction that can be made between covert control and mobilization of bias, and offers nothing which is not already available through decision-making analysis. The problem it was directed toward can be resolved only by adopting a more modest approach to the concept "power" which does not require the observer to focus on individual initiatives within the context of an observable relationship. Such an unnecessary restriction devalues the significance of the context to the description of power. It is only by capturing something of this that the observer can hope to understand the forces shaping the relationships that constitute the community. There is no shortcut to be gained by elaborating a set of stipulative definitions, as Bachrach and Baratz have attempted. The result is merely to inhibit the observer's sensitivity to his surroundings. Since their work can be taken primarily as an appeal to this, it is unfortunate that the "Two Faces" they have put on their concern should both look inward onto a definitional maze.

<sup>83</sup> A definition of "intentions" and "effects" poses no more problems than does a definition of "issue" or "decision." In both cases the problem of establishing a requisite level of significance arises, but can be dealt with only by the observer using stated criteria within the context of a specific study.

<sup>81</sup> I am grateful to Alan Alexander of Reading University for his forceful probing on this point.

<sup>82</sup> Dahl, "Concept of Power," pp. 202–203.

# Power and Its Two Faces Revisited: A Reply to Geoffrey Debnam

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In a pair of articles published a dozen years ago and more extensively in a book-length essay that appeared in 1970, we expanded the then-existing analytical model for studying the political process (defined to include nongovernmental as well as governmental actors and institutions).<sup>1</sup> Ruthlessly oversimplified and somewhat abbreviated, these were the assumptions and hypotheses that constituted our conceptual framework:

(a) In most polities, large and small, the distribution of benefits and privileges—notably, income, wealth, power, social status—is highly unequal among persons and groups.

(b) Over a period of time, inequality is sustained, in its essentials if not entirely, by the “mobilization of bias,” that is, the prevailing norms, precedents, myths, rituals, institutions and procedures that operate “in favor of the exploitation of some kinds of conflict and the suppression of others. . . .”<sup>2</sup> In other words, although those institutions and procedures appear to be neutral as between those seeking and those resisting distributional change, it actually regulates the nature and extent of conflict.

(c) The use of power and its correlates is a crucial means for sustaining and strengthening the mobilization of bias and thereby perpetuating “unfair shares” in the allocation of benefits and privileges. The exercise of power towards this end is a major form of nondecision making, defined as a process for thwarting latent or manifest challenges to things-as-they-are.

(d) Nondecision making, which is exercised both overtly and covertly, can take several other forms. The most direct and extreme is resort to force. Just as direct but less extreme is exercise of power against would-be change-seekers, ranging from intimidation (potential

deprivation of valued things) to co-option (potential rewards). The indirect forms of nondecision making, however, are the most important: utilization of an existing bias of the political system, such as a norm, precedent, rule or procedure; or reshaping and strengthening the mobilization of bias as a whole.

## The Concept of Nondecision Making

Although these ideas have received warm endorsement from many sources, they have also attracted strong criticism. Particularly sharp disses have been entered by Richard Merelman and Raymond Wolfinger.<sup>3</sup> They are now joined by Geoffrey Debnam.

In his critique of nondecision making, Debnam does little more than echo his two predecessors. He concedes that the concept “is descriptively suggestive of certain possible areas of neglect” in investigations of community power, but like them contends that “it has not been shaped into a useful analytic tool. . . .” Like them, he asserts that nonevents and nonissues are unresearchable. And like them, he concludes that nondecision making creates no demands “which cannot be met by decision-making analysis.”

In view of Debnam’s virtual redundancy, why do we respond? We do so mainly because we are impressed and depressed by the failure of yet another competent scholar to recognize, let alone grasp, the *theoretical* significance of the nondecision-making concept in the larger context of explaining the nature and uses of power. As to this, two features of our concept are central. The first is best expressed by the major assumption upon which the concept is grounded: in a reasonably stable polity, power is mainly exercised not by those who make decisions nor by those who decide agendas, but by persons and groups who direct their energy to shaping or reinforcing pre-

<sup>1</sup> Peter Bachrach and Morton S. Baratz, “Two Faces of Power,” *American Political Science Review*, 56 (December, 1962), 947–952; “Decisions and Non-decisions: An Analytical Framework,” *American Political Science Review*, 57 (September, 1963), 632–642; and *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970).

<sup>2</sup> E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 71.

<sup>3</sup> Richard M. Merelman, “On the Neo-Elitist Critique of Community Power,” *American Political Science Review*, 62 (June, 1968), 451–460; and Raymond E. Wolfinger, “Nondecisions and the Study of Local Politics,” *American Political Science Review*, 65 (December, 1971), 1063–1080. Our brief commentary on Merelman, together with his reply, appear in *American Political Science Review*, 62 (December, 1968), 1268–1269.

dominant norms, precedents, myths, institutions, and procedures that undergird and characterize the political process. What this means, among other things, is that analysis of nondecisions is predicated on the assumptions that power is a crucial variable and that in studying power, the focus should be upon whether or how it is employed to sustain or modify the most fundamental aspects of political institutions and processes. The decision-making model, in stark contrast, incorporates the basic premise that norms, precedents, and institutions are "givens" within which power relationships among actors (e.g., construction of decisional agendas, participation in issues-resolution) are to be studied.

Nondecision making in a power context is based on the additional presupposition that political consensus is commonly shaped by status-quo defenders, exercising their power resources, and operates to prevent challenges to their values and interests. Again in sharp contrast, the decision-making model is built on the assumption that consensus is the result of unguided molding of diverse attitudes and beliefs of a large share of the population and therefore constitutes no significant barrier to entry of important grievances into the decisional process. By declining to regard the consensual area of politics as a "given" and treating it instead as a variable that is modifiable through exercise of power and its correlates, we add an important dimension to study of the political process. In particular, our approach broadens the definition of "political interest" so that it includes overt and covert grievances, as well as challenges to things-as-they-are that have actually become issues.<sup>4</sup> In this way, yet another vital area of politics is opened to examination from a perspective that brings into focus *both* faces of power.

The differences between us and Merelman, Wolfinger, and Debnam are epistemological as

well as conceptual. All three of our severe critics have been able to demonstrate, at least to their own satisfaction, that empirical verification of nondecision making is difficult, if not impossible. But none has been able to demolish our proposition that this second face of power is actually exercised in the real world; witness Debnam's concession that it "is descriptively suggestive of certain possible areas of neglect. . . ." Unrelenting empiricists all, they find themselves in a tight corner. They must either pretend that there is no "second face" of power, limiting the scope of their inquiry to what is empirically determinable and thereby running the risk that they will seriously misrepresent reality. Alternatively, they can claim, as Debnam does, that (1) nondecision making is simply covert control; (2) covert control can be analyzed in the same manner as overt control; and therefore (3) nondecision making is not a distinctive phenomenon and can be discarded on the ground of redundancy. The errors here, as we shall stress below, are that Debnam seriously misconstrues the nature of nondecision making and, by trying to rule out the phenomenon also asks empirical investigators to run the risk of seriously misrepresenting reality.

The way out of this false dilemma is obvious, however distasteful to the empiricists. It is to adopt an analytical model that incorporates hypotheses which, while difficult to verify empirically, compel the investigator to explore a broader range of aspects of the polity within a power context. The product of the research may well be impressionistic or ill-supported by data, but better this than compounding the error by ignoring altogether the elusive elements. To make the point in more general terms, while we recognize that theoretical propositions must always be tested by reference to verifiable data, we also insist that theory is another important form of knowledge that must be encouraged to produce new, expanded, fruitful ways to understand politics. It is a reasonable expectation that as it performs this task, it will spur empiricists to devise appropriate techniques to test its validity.

### Forms of Nondecision Making

Debnam's critique of our works suffers additionally from his serious misunderstanding of the forms of nondecision making. Because others may share his confusion, we now address his mistaken inference that there are two major forms of power's second face—covert control and mobilization of bias.

The notion of covert control is both too broad and too narrow to qualify as a form of nondecision making. It is too broad because *both* faces of power can be exercised covertly. For example, a legislator may be bribed covertly as a means of

<sup>4</sup>We suspect that our conception of "interest" is still too narrowly drawn to permit a thorough analysis of consensus in circumstances where there appears to be no dissent in a polity, overt or covert. Perhaps, as Matthew A. Crenson suggests in his first-rate comparative study of nondecision making (*The Unpolitics of Air Pollution: A Study of Non-Decision-making in Two Cities* [Baltimore: The Johns Hopkins Press, 1971]) and as Steven Lukes argues in his brilliant essay, *Power: A Radical View* (London: Macmillan, 1974), our error is to rely upon a subjective conception of interest. For a discussion of this issue as it relates to "false" consciousness, see Isaac D. Balbus, "The Concept of Interest in Pluralist and Marxist Analysis," *Politics and Society*, 1 (February, 1971), pp. 151-177; William E. Connolly, *The Terms of Political Discourse* (Lexington: D. C. Heath, 1974), pp. 45-85; and Peter Bachrach, "Interest, Participation and Democratic Theory," in *Participation*, ed. Roland Pennock and John Chapman (New York: Atherton Press, 1975).

keeping a grievance from becoming a political issue (nondecision making) or to assure victory in the decision-making arena. The same applies to covert threats of blackmail, physical violence, political threats, and the like. These covert strategies may be used either to prevent issues from surfacing (nondecision making) or winning issues once they have surfaced (decision making). It is too narrow because there are numerous ways in which nondecision making is overt; among several illustrations we gave in *Power and Poverty* (pp. 44-45) are co-option of opponents, public denial of legitimacy to demands for change, and deflection of challenges by referring grievances to study commissions.

Our concern over Debnam's error here goes well beyond annoyance at his slovenly reading and analysis of our work. By construing nondecision making as covert control, he carefully sets up the further argument that the concept is redundant. What else can be concluded from his subsequent assertion that by "a sensible application of the techniques of decision-making analysis" the investigator can observe decisions—and thus the exercise of power and its correlates—on the covert level of politics as well as in the formal decision-making arena?

The allegation of redundancy would indeed be valid if the two faces of power on the covert level were used for the same purposes. They are not. Nondecision making serves a radically different set of purposes than does decision making, and on both levels of the political process. Debnam's failure to catch this crucial distinction is matched by his inability to explain satisfactorily how decision-making analysis can capture those "possible areas of neglect" of which nondecision making is "descriptively suggestive."

What is especially puzzling in this respect is his lack of concern about how the techniques of decision-making analysis can adequately serve to identify nondecisions, when the concepts of decision making and nondecision making are grounded upon such different and conflicting assumptions—assumptions pertaining to the scope and openness of the political arena, the genuineness of consensus, the neutrality of political institutions, and the concept of interest. Turning his back on such theoretical concerns, he is content to assert grandly but vaguely that "if an observer has reason to believe that a problem exists in a community, then it is a modest assumption that an example of that problem will produce some significant evidence concerning the nature of power in that community."

The simplicity of this approach is beguiling. But in the absence of some conceptual framework that provides an adequate basis for observing the interaction of the two dimensions of power with

political institutions and prevailing social values, the analyst will be only partially equipped, limited to studying only the "first face" of power. Under Debnam's approach, how is the observer to determine that the problem exists because of a decision, as opposed to one form or more of what we define as nondecisions? On these questions Debnam is silent, apparently asking us to have faith that he knows what to examine and how, and that in the process he will produce some significant evidence about power.

No less distressing is his erroneous conclusion that we view the mobilization of bias as a form of nondecision making. In very plain language, we said (*Power and Poverty*, p. 44), "The primary method for sustaining a given mobilization of bias is nondecision-making." We went on, there and subsequently, to distinguish carefully between those who exercise power to create, shape or reinforce the mobilization of bias and the mobilization of bias itself, secure in the understanding that failure to make the distinction could lead to the error of focusing solely upon the mobilization of bias to the exclusion of actors who exercise power and its correlates to sustain the mobilization and make it politically effective. Here again, however, we suspect that Debnam's misreading was more deliberate than accidental. By enveloping the exercise of power within the mobilization of bias, he eliminates in his own mind the interaction between actors and the mobilization of bias and, in doing so, permits the conclusion that nondecisions (at least in this form) are non-events. The way is then open for him to ask, quoting Nelson Polsby, "Which non-events are to be regarded as significant?"

Two points must be reiterated before addressing this question. First, nondecision making (almost invariably, Debnam and others of like mind speak of "nondecisions," a noun, rather than "nondecision making," a gerund that connotes active exercise of power) is an act, performed either overtly or covertly, that is susceptible to observation and analysis.<sup>5</sup> Second, the mobilization of bias is an effective instrument of power and is also observable in its nature and impact; true, it is not an "event," but its existence is demonstrable both by reference to its impact upon those who wish to transform their grievances into issues and by reference to its linkage to power wielders (nondecision makers) who shape and support it. The observability of both nondecision making and the mobilization of bias, in

<sup>5</sup> Debnam's argument on this point is contradictory. On one hand, he contends acts of this kind can be observed through use of techniques of decision-making analysis. Yet, on the other hand, he characterizes such acts as nonevents and therefore non-observable.

other words, is materially assisted by their interaction and their direct or indirect effect upon potential issue raisers.

To return to the question at hand, the criterion or criteria for determining if a nondecision is significant, we have answered straightforwardly: A potentially key issue "is one that involves a genuine challenge to the resources of power or authority of those who currently dominate the process by which policy outputs in the system are determined" (*Power and Poverty*, pp. 47-48). Can potentially key issues, so defined, be identified empirically? And can the same be said about nondecision making and the mobilization of bias?

Our research in Baltimore is not the only evidence in support of affirmative answers to these questions. We cite additionally the excellent study by Verba and Nie, which exemplifies the feasibility of identifying a comparatively subtle form of mobilization of bias, coupled with supportive nondecision making within a large polity.<sup>6</sup>

The study confirms the standard socioeconomic model of political participation: upper-status citizens—the more affluent, educated, and prestigious—are significantly more active politically than other members of the polity. The participatory gap between social classes is increased by other forces which the study details. Among the findings is that those who most need to participate in the political process are the nonparticipants. Because they are for all practical purposes outside the process, they have little or no influence either in shaping policy or, more importantly, in determining which issues reach the decision-making arena. Those who do set the agenda have interests that are markedly different from those of nonparticipants.<sup>7</sup>

Verba and Nie interpret these findings as evidence that participatory institutions and rights in the United States, although unequally utilized, are politically neutral. "Equal rights to participate," the authors observe, "are just that: *rights*, not obligations. Some may take advantage of these rights, others may not."<sup>8</sup> They even claim that "participation remains a powerful social force for increasing or *decreasing* inequalities. It depends on who takes advantage of it."<sup>9</sup>

Viewed from our perspective, however, a different interpretation is possible. If rights to participate are considered, not in abstract terms, but by reference to the way in which *they are actually used, by whom, and with what effects over time*, it

tory system as class-biased. The belief in the system's neutrality precludes open consideration of alternatives to it, such as proposals for workers' control of industry and neighborhood governments. Put differently, so long as the existing participatory system's legitimacy remains unimpugned, grievances about it will fail to get on the decisional agenda and changes in it will fail to be made.

Verba and Nie's work is defective to the extent that it overlooks this critical point. What is more important, their neglect contributes in a small but nontrivial way to reinforcement of the mobilization of bias in favor of the established participatory system. Thus, although they were probably unconscious of doing so, they were engaging in a form of nondecision making.

### The Meaning of Power and Its Correlates

Debnam devotes the concluding section to an extended disquisition on our definitions of power and its correlates. In doing so, he has read much more into our definitional exercise than we intended. Our purpose was simply to distinguish rigorously among a group of concepts that are generally regarded as synonymous, in the interest of a greater degree of analytical and expository precision. The importance of doing this is underlined by Debnam's misapprehension, for instance, that "severity of sanction is the only significant means of distinguishing power from authority and influence." We labored—evidently without success in Debnam's case—to make the point that sanctions are not invoked at all in situations involving authority and influence, whereas in power relationships sanctions are threatened but not invoked.

We certainly did not mean, as Debnam charges, that political analysts should spend their time, with our little lexicon at hand, separating and dissecting the attributes of power and its correlates. That would indeed be a waste of time. Nowhere in our writing, expressly or by implication, did we subscribe to such an approach. To the contrary, all of our definitional efforts were carefully cast in a broad political context. We concede, however, the validity of one of his criticisms of our conception of power: it is more relevant to an individual state than to a political relationship. Ironically, the same criticism can be leveled at Debnam's conception of power, which focuses upon "the *intention* to produce an effect"

of power, to quote from our essay, "our main concern is not whether the defenders of the status quo use their power consciously, but rather in and how they exercise it and what effect it has on the political process and other actors within the system" (*Power and Poverty*, p. 50). Dennis Wrong nicely illustrates the relationship between power and unintended effects: "It is only because a mother exercises socially approved power over her children that she may unintentionally shape their personalities along lines that are repugnant to her and defeat her most cherished hopes."<sup>10</sup>

Second, we recognize the existence of *indirect* power relationships, i.e., those in which communication between the power wielder and the power receiver is wholly or partially obstructed. This kind of situation is well exemplified by situations in which power is successfully employed to suppress a prospective challenge to the status quo. In fact, it often happens that neither *A* nor *B* is aware of the other's existence, yet one can exercise power over the other. Contrary to our earlier belief, then, "clear communication" between them is not a necessary condition of a power relationship. It is necessary that *B* be aware that, contrary to his wishes, he is unable to elevate his grievance to the status of public issue without violating "the rules"; and that the rules, which are created or reinforced by an exercise of power, are a barrier against his conversion of the grievance into an issue. If he chooses, however, to resist the rules and thereby invite invocation of the sanction (e.g., strong social disapproval or physical punishment), the attempt to exercise power has failed. The manner in which *A* exercises power over *E* in this kind of situation should no more be discounted because of its indirect character than should the exercise of power by a general against his foot soldiers. In the former case, power is brought to bear through prevailing norms and institutions; in the latter, through a chain of command.

Third, in our essay we proposed a causal view of power, asserting that "*to the extent that a per-*

son or group—consciously or unconsciously—creates or reinforces barriers to the public airing of policy conflicts, that person or group has power" (*Power and Poverty*, p. 8). This formulation is excessively stringent in implying that *A* exercises power only when it is a necessary and sufficient condition of gaining *B*'s compliance. If the necessary-and-sufficient test were held to, there would be few instances, individual or social, in which it could be shown that power was exercised. Especially would this be true when power is exerted in order to shape or reinforce a mobilization of bias as a shield against a potential issue that threatens to become actualized. Here the relationship is indirect and thus the causal connection between *A*'s action and *B*'s response is usually loose. A more useful formulation of power is one which states that a person or group exercises power to the extent that he, she or it *contributes* to shaping or strengthening barriers to public airing of grievances.<sup>11</sup> The problem confronting the analyst is judging the extent of the contribution, a difficult task and one that certainly does not invite exactitude.

### Conclusion

In summary, our approach to power overlaps in important respects that of Debnam and others whose views he represents. Most notably, we accept with few reservations their generalizations about and mode of inquiry into the decision-making face of power. But let no one mistake the extent of difference between us and them.

The essence of the difference can be stated simply: For us, but not Debnam and his cohorts, the test of a construct's worth is the contribution it makes toward gaining a full understanding of the nature and uses of power in a polity. Empirical verification is much to be desired but it is not foremost in importance. What *is* foremost is a solid conceptual framework. Toward that we are building, the dogmatic empiricists to the contrary notwithstanding.

<sup>10</sup> Dennis H. Wrong, "Some Problems in Defining Social Power," *American Journal of Sociology* 73 (May, 1968), 673-681, at 677.

<sup>11</sup> For a defense of this position, see Petro Georgiou, "The Concept of Power: A Critique and an Alternative" (Unpublished; University of Melbourne, 1974).



## Rejoinder to "Comment"

by Peter Bachrach and Morton S. Baratz

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Bachrach and Baratz argue that a decision-making approach carries certain necessary assumptions concerning the nature of the political process, and that these assumptions cause research to be focused on a limited area of the political process. They take this view, I believe, because they have confused a decision-making approach with a pluralist approach. It is interesting to note that what they have to say on the basic assumptions of "the decision-making model" represents the argument raised by them in their first article "The Two Faces of Power," but which was there directed against the *pluralist* model. Only if one is prepared to believe that the pluralist interpretation of decision-making analysis is alone valid can one then indulge in the sleight of hand necessary to condemn *any* decision-making approach for "pluralist sins."

Any method of study may legitimately be modified to suit the purposes of the researcher. Snyder for example suggested that decision-making analysis should focus on decisions "made *within* the governmental structure,"<sup>1</sup> but this proposal was dependent on a restricted view of the nature of authority which suited Snyder's preoccupation with foreign policy making. Dahl's research design for his New Haven study excluded a wide variety of decisions on the reasonable grounds of economy, and his choice was rationalized on pluralist premises. In neither case did the choice of focus spring from the theoretical imperatives of a decision-making approach. The researcher had to import other criteria to sort his way through the heap.

So whether a decision is made within the governmental structure or outside it; whether it is a decision to act or not to act, to decide or not to decide, covert or overt; whether it is made by the hero or the villain; whether it is readily identifiable in formal terms, or whether it is assumed from the observation of its consequences, the short answer is that it may be encompassed within a decision-making approach. There is no reason that anyone making this assumption should have any more difficulty in recognizing and analyzing the forms, processes, and instances of status-quo

dominance than Bachrach and Baratz themselves. After all, as they point out, "a nondecision, as we define it, is a decision." And, lest the point should go unnoticed, a decision is a decision, is a decision! A decision-making approach is adequate to deal with the analysis of decisions, whatever alternative name they are given.

Bachrach and Baratz have made some specific charges of inaccuracy in my analysis. They claim, for example, that I am wrong to suggest that what they describe as nondecision making can be said to have two forms. In refuting this suggestion they quote their own statement that "the primary method for sustaining a given mobilization of bias is nondecision-making." The implication from this isolated sentence is clearly that mobilization of bias cannot therefore itself be a form of nondecision making. If Bachrach and Baratz were consistent, this would be all that needed to be said. But since a nondecision "is a decision that results in suppression or thwarting of a latent or manifest challenge to the values or interests of the decision maker," then clearly any agent effecting such a decision *must* be a nondecision maker. Can the mobilization of bias be held to produce such effects? Bachrach and Baratz can certainly be cited in support of such a view. Their reference to the *unconscious* creation or reinforcement of barriers to the public airing of conflicts can be interpreted in no other way, and neither can the statement in their comment that "mobilization of bias . . . actually regulates the nature and extent of conflict."

Whether the phrase "covert control" is regarded as an accurate description of the other form of nondecision making depends on whether one can envisage a situation where a status-quo defender resorts to the exploitation of "prevailing norms, precedents, myths, rituals, institutions and procedures" and where he is explicit about his objectives. The assumption behind my description is that the overwhelming majority of occasions will involve a decision whose true intention is not made explicit to the "victims." Bachrach and Baratz's objection, then, seems to be based on a misunderstanding of my use of the term "covert." I mean that the *purpose* is hidden, not necessarily the *action which embodies the purpose*.

The third section of their "Comment," referring to my discussion of their definitions of power and its correlates, is disingenuous. They certainly

<sup>1</sup> Richard C. Snyder, "A Decision-Making Approach to the Study of Political Power," in *Approaches to the Study of Politics*, ed. Roland Young (Evanston, Illinois: Northwestern University Press, 1958), p. 16.

do not contest the substance of my argument, except in one small instance to which I refer below. Instead they suggest that in some further unstated way, which by implication invalidates my critique, I have gone beyond the purpose of their discussion. It is as well to bear in mind that this purpose "was simply to distinguish rigorously among a group of concepts that are generally regarded as synonymous, *in the interest of a greater degree of analytical and expository precision*" (emphasis added). The object of my critique is to show that they have failed to do this. If they disagree with my argument, they should explain the grounds of their disagreement and not fall back on the suggestion that I have taken them too seriously.

The only grounds on which they attack my critique is that I have misunderstood their references to sanctions. Should anyone care to read through the relevant sections, they will see that Bachrach and Baratz state that there are "no severe sanctions" in the case of influence and authority, and that I take them to mean that there are sanctions of some sort, but that they are not severe. Why should they qualify what they mean to be absolute? But since I argue that "sanction" is a worthless means of distinguishing between power and its correlates, surely their response is misdirected.

A similar level of confusion is reflected in their reference to my approach to power which they claim is open to the same criticism that I level against their formulation—that it has "more relevance to a description of an individual state than a political relationship." In attempting to substantiate this they cite my emphasis on "*the intention to produce an effect*" yet ignore the further reference three sentences later to "the intentions of actors in the community, *institutional as well as individual*" (emphasis added). I cannot imagine it necessary to explain the significance of my reference to "institutional" in this context.

There are other points where Bachrach and Baratz make claims on my behalf to which I do not subscribe, but an enumeration of minor grievances becomes tedious. I would prefer to move on to their comment that I have presented no guide as to how I would structure an alternative approach—which was, of course, peripheral to my main concern of demonstrating the inconsistencies and inadequacy of their work. I suggested, though, that the study of power would have to be couched in terms which embrace both intentions and effects. Since this would not be sufficiently specific for a focused inquiry I noted that the observer must attack the problem "from a perspective he determines himself, or which emerges from the community he chooses to

study." Similarly, Bachrach and Baratz have suggested that any discussion of power should embrace both decision making and what they choose to call nondecision making. They nevertheless have to provide a focus for their empirical study by referring to criteria which are extrinsic to those concepts. "An empirical analysis of the relationship between the anti-poverty effort and the political process in Baltimore, Maryland"<sup>2</sup> is, surely, both (a) an exercise which they thought would be favorable to a demonstration of their views, and (b) a topic which they think is important. This is perfectly legitimate. There is no reason why they should deplore in my case a procedure which they have followed in their own—unless they feel that they have done more.

I agree with them, though, that the words power, authority, coercion, influence and manipulation have different meanings. My intuitive sense of the ordering of these concepts is as follows. Power is treated at sufficient length in my article. It is the generic term and the one which is used to denote an outcome but which cannot therefore be used to explain the causes of that event. Authority and coercion are the bases of power. In the extreme cases they can themselves account for the production of an intended effect. I may accede to a request because I regard it as unquestionably authoritative (that I recognize a duly elected leader, or accept the advice of an expert), or because it is so coercive that I have no choice but to submit to what I regard as an alien interpretation of my situation. But situations are rarely so straightforward. Where there are no clear and compelling imperatives there is scope for influence. And the language of influence will be more or less authoritative or coercive. Influence, then, is to be distinguished from authority and coercion in that it is not a power base but is the way we describe the exploitation of such a base. Manipulation is a closely related term. It refers, however, to the covert use of a power base.

This simpler formulation emphasizes that coercion is, with authority, an equally significant theoretical aspect of power, rather than being merely one amongst a variety of concepts. Power is frequently used as a term denoting the possession of a faculty which is neither authoritative nor coercive. Authority and coercion, between them, though, describe the limits of legitimate and nonlegitimate action. This states briefly what is contentious and would need elaboration. But where these terms are removed from the center of political analysis and replaced by power masquer-

<sup>2</sup> Peter Bachrach and Morton S. Baratz, *Power and Poverty: Theory and Practice* (New York: Oxford University Press, 1970), p. viii.

ading as a term of technical virtuosity, it is easy to overlook the problems of public morality that Bachrach and Baratz are so properly concerned with.

In view of the length of their "Comment," and of the period of time since their ideas were first mooted (twelve years), it is significant that they have been unable to attempt a more coherent justification of their position. That such is necessary should be apparent from their lament that

"yet another competent scholar" has failed to appreciate their argument. They try to resolve the problem too easily in my case by attributing it to my "slovenly reading and analysis" as they describe it. But it may well be time for them to consider the possibility that there are serious difficulties in their theoretical work. Before they attempt to build the "solid conceptual framework" that they promise, they should inspect the foundations more closely.

# On the Apparent Paradox of Participation: A New Proposal

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In his *Economic Theory of Democracy*, Anthony Downs reached the conclusion that voting is usually an irrational act.<sup>1</sup> Given that voting is costly in time and effort involved in getting to the polls and making a decision and given that the probability any one voter can affect the outcome diminishes very rapidly as the number of voters increases, it appears that a rational, utility-maximizing citizen will most probably abstain. Only if the citizen perceives a phenomenally large difference between (or among) the candidates or if the number of voters is small will he go to the polls.

Empirically, however, we know that substantially more than half of all eligible voters go to the polls in American presidential elections where the probability of any one voter's affecting the outcome is smaller than in any other election. If Downs is correct, at least half of the American electorate is irrational. But of what validity is a rational choice theory which characterizes most individuals as irrational? For this reason, theorists have attempted to modify Downs's theory so as to allow it to better explain why some rational citizens vote while others abstain.

Some of these revisions, like that presented by Riker and Ordeshook, have simply added variables to the original Downs theory so as to make voting rational for some substantial group of citizens.<sup>2</sup> Others, like Ferejohn and Fiorina, have substituted a different calculus of rationality for Downs's expected utility model.<sup>3</sup> Under close examination, however, each of these revisions appears to involve theoretical and empirical problems of the same order of magnitude as Downs's original theory. I discuss these theories and their related problems in the next section. Following this I present a new though seemingly obvious modification of the Downs theory, which will, I believe, substantially improve the empirical validity of his theory while avoiding the problems associated with the other revisions.

## Previous Theories

Using the notation of Riker and Ordeshook, Downs's original theory can be represented by the following equation:

$$(1) \quad R = PB - C$$

where  $R$  is the satisfaction, in utiles, a voter will receive from voting,  $B$  is the difference in utility, in utiles, that a voter perceives between his most and least preferred candidate,  $P$  is the probability that a voter will himself bring about benefit  $B$  by voting, and  $C$  is the cost, in utiles, of voting.<sup>4</sup> In the Downs model, if  $R_i > 0$ , citizen  $i$  votes; if  $R_i \leq 0$ , he abstains.<sup>5</sup> Assuming  $C_i > 0$ , the only way for  $R_i$  to be greater than zero is for  $PB > C$ . But as the number of voters increases,  $P$  declines rapidly so that for any actual election,  $P$  is likely to be very small. In turn this implies that  $B$  must be very large or else  $PB \leq C$  and the citizen abstains. As one example, Tullock has shown that where  $C$  equals one utile and  $P$  equals .000,000,1, even a  $B$  of 10,000 utiles is insufficient to cause a rational citizen to vote (in fact, in this case  $B$  must be larger than 10,000,000 in order to make  $PB \geq C$ ).<sup>6</sup>

The major solution to this problem suggested by Riker and Ordeshook is to add a variable  $D$  to the original Downs equation.<sup>7</sup> This new variable is equal to the utility a citizen receives from the act of voting, regardless of who wins the election. That is, it is the "satisfaction [derived] from compliance with the ethic of voting," from "affirming allegiance to the political system," from "affirming a partisan preference," etc.<sup>8</sup> From its definition it is assumed  $D > 0$  and that:

$$(2) \quad R = PB - C + D.$$

Thus, what Riker and Ordeshook have done is to insure that  $R$  can be positive for a substantial set

<sup>1</sup> Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 274.

<sup>2</sup> William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25-42. See also William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), chap. 3.

<sup>3</sup> John A. Ferejohn and Morris P. Fiorina, "The Paradox of Not Voting: A Decision-Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525-536.

<sup>4</sup> Riker and Ordeshook, "Calculus," p. 25.

<sup>5</sup> Technically the condition  $R_i > 0$  is a necessary but not a sufficient condition. For example, if for individual  $i$ ,  $R_i$  is positive he still may not vote if some alternative activity to voting gave him greater utility.  $R_i > 0$  is sufficient only when it is assumed that the maximum utility of all alternatives to voting is 0. Inasmuch as  $R_i > 0$  is a nice shorthand notation such an assumption is made in this paper.

<sup>6</sup> Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: The University of Michigan Press, 1967), p. 110.

<sup>7</sup> Riker and Ordeshook, "Calculus," pp. 27-28.

<sup>8</sup> *Ibid.*, p. 28.

Table 1. Empirical Test of the Riker-Ordeshook Calculus of Voting<sup>9</sup>

Expected closeness of election result	"Citizen Duty" Score					
	High		Medium		Low	
	High P.D.	Low P.D.	High P.D.	Low P.D.	High P.D.	Low P.D.
Close	91%	83%	85%	71%	63%	44%
Not Close	86%	74%	77%	71%	62%	39%

(P.D. = party differential; i.e., a measure of B.)

of citizens even if they can have no influence on the outcome of an election.

Is this a satisfactory solution? To answer this question one must distinguish between the empirical merits of their solution (does it allow us to better explain voting) and the theoretical merits (how well does it fit in with a rational choice model). Having made this distinction, one can say that empirically the Riker and Ordeshook revisions have merit, while theoretically their revisions are problematical.

Consider first the empirical merits. Table 1 is a reduced version of the complex Table 3 of Riker and Ordeshook. The first thing to note in this table is that the original Downs theory has some empirical validity. When voters expect an election to be close (i.e., when *P* is relatively large) as well as when there is a high *B* (i.e., the voter perceives a large difference between the candidates), more citizens vote. But it is clear in the table that these are minor effects compared to the effect of the *D*-term as measured by "Citizen Duty" scores. As *D* declines, the per cent voting declines much more rapidly than when *B* declines or *P* increases. Hence, the Riker-Ordeshook revision adds considerable explanatory power to the original Downs model.

Theoretically, however, the Riker-Ordeshook theory creates a major problem. As Barry notes: Riker [and Ordeshook say] . . . that people vote because they derive satisfaction from voting for reasons entirely divorced from the hope that it will bring the desired results. This may well be true but it does not leave any scope for an economic model to come between the premises and the phenomenon to be explained. Instead, the question shifts back to: "Why do some people have this kind of motivation more strongly than others?"<sup>10</sup>

In other words, knowing that the *PB* term of the Downsian equation is likely to be quite small, the

Riker-Ordeshook revisions essentially reduce the Downs theory to:

$$(3) \quad R = D - C.$$

Given that both *C* and *D* (as defined by Riker and Ordeshook) are likely to be constants, it can be seen that in the Riker-Ordeshook model, the decision to vote or not to vote is largely independent of the particulars of specific elections.

Ferejohn and Fiorina attempt to improve upon this state of the theory by scrapping the expected utility model and substituting in its place Savage's minimax regret criterion.<sup>11</sup> Under this criterion, a citizen "chooses the act which minimizes his maximum regret."<sup>12</sup> This is illustrated in Table 2, identical to Table 3 of Ferejohn and Fiorina.<sup>13</sup> In constructing this table Ferejohn and Fiorina standardized the voters utility function so that the utility of candidate 1 (the most preferred candidate) winning is 1, and the utility of candidate 2 winning is 0.

The symbols used in this table have the following meaning:

- S*<sub>1</sub>: Candidate 1 (the most preferred candidate) wins by more than one vote without the citizen's vote
- S*<sub>2</sub>: Candidate 1 wins by exactly one vote without the citizen's vote
- S*<sub>3</sub>: Candidate 1 and candidate 2 tie: (a coin flip decides winner) without the citizen's vote
- S*<sub>4</sub>: Candidate 2 wins by exactly one vote without the citizen's vote
- S*<sub>5</sub>: Candidate 2 wins by more than one vote without the citizen's vote
- V*<sub>1</sub>: The citizen votes for candidate 1
- V*<sub>2</sub>: The citizen votes for candidate 2
- A*: The citizen abstains

In this table the maximum "regrets" for each row are *C*, 1, and 1/2-*C* respectively. From the

<sup>9</sup> This table is taken from Brian Barry, *Sociologists, Economists, and Democracy, Themes and Issues in Modern Sociology* (London: Collier-Macmillan, 1970), p. 17.

<sup>10</sup> *Ibid.*, p. 16.

<sup>11</sup> Ferejohn and Fiorina, pp. 527-528.

<sup>12</sup> *Ibid.*, p. 528.

<sup>13</sup> *Ibid.*

Table 2. Regret Matrix for Two-Candidate Election

Acts	$S_1$	$S_2$	$S_3$	$S_4$	$S_5$
$V_1$	$c$	$c$	0	0	$c$
$V_2$	$c$	$1/2+c$	1	$1/2$	$c$
$A$	0	0	$1/2-c$	$1/2-c$	0

Assumption:  $1/2 > c$ .

$c < 1/2$  assumption,  $V_2$ , voting for the least preferred candidate is not viable because  $c < 1/2 < 1$  and hence, strategy  $V_1$  dominates  $V_2$ . Similarly, strategy  $A$  dominates  $V_2$ . Thus, in a two-candidate contest, a citizen either votes for his most preferred candidate or abstains. But a distinction can also be made between these two; if the maximum regret of  $V_1$  is less than the maximum regret of  $A$ , the citizen will elect strategy  $V_1$ . That is  $c < 1/2 - c$ , which implies that he will vote for candidate 1 if  $c < 1/4$ . Substantively, he will vote if his costs of voting are less than one-fourth of the difference between his most-preferred and least-preferred candidate.

To see that this is not equivalent to the expected utility solution of Downs, consider the previous example where a citizen would gain 10,000 units of utility if his most-preferred candidate won the election and zero if his most-preferred candidate lost. Remember that it cost him 1 unit to vote and that the probability he would decide the election was .000,000,1. Under an expected utility model this citizen would not vote. But he would vote if he were a minimizer of maximum regret for  $10,000 - 0 > .25 = 1/4C$ . Hence the Ferejohn-Fiorina model predicts a higher level of voting than does the Downs model.

It is thus clear how Ferejohn and Fiorina "solved" the paradox of participation; they found a means of eliminating  $P$ , the probability that a citizen, by voting, would affect the outcome. In

their model,  $P(S_3)$ , the probability the election results in a tie, and  $P(S_1)$ , the probability candidate 1 wins by more than one vote, as well as the probabilities for all the other states of nature are equal.<sup>14</sup> This is clearly unrealistic. Empirically it is almost always true that  $P(S_3) < P(S_1)$ ; in fact,  $P(S_3)$  is likely to be a very small number as are  $P(S_2)$  and  $P(S_4)$ . If the maximum regret for any of  $V_1$ ,  $V_2$ , or  $A$  (see Table 2) is in one of these columns, is it not rational for the citizen to discount the possibility of himself experiencing the regret? For example, the maximum regret for abstaining is  $1/2 - c$  if either  $S_3$  or  $S_4$  are the states of nature; if the probability of either  $S_3$  or  $S_4$  occurring is very small (e.g.,  $1 \times 10^{-8}$ ) should not the citizen consider his maximum regret for abstaining to be 0 as it would be if  $S_1$ ,  $S_2$ , or  $S_5$  were the state of nature? Analogously, consider a citizen deciding whether to play the following game: a number from 1 to 1000 is to be drawn from a hat containing each of the numbers from 1 to 1000 only once (i.e., the hat has only 1000 numbers in it); if number 500 is drawn the citizen loses \$1.00, if any other number is chosen he wins \$10.00. His maximum regret from playing is \$1.00, from not playing, 0. The minimum of these is 0 so he would not play! This is clearly not an intuitive nor, I suspect, empirically valid conclusion.

Moreover, an examination of Table 1 will show that the minimax regret principle cannot account for some of the variance in voting. In all categories, there was a higher or equal percentage of voting when the election was perceived to be close than when the election was not perceived to be close. This difference, though admittedly small, cannot be explained by the regret model. Hence, use of this model involves the violation of intuition as well as a reduction in explanatory power.

#### A New Reformulation

In the Downs expected utility model,  $R$ , the expected utility of voting equals  $PB - C$  where  $B$ , the "party differential" equals  $B_1 - B_2$ , the difference in utility derived from the victory of the most-preferred candidate and the utility derived

<sup>14</sup> Very technically this is untrue because the Ferejohn-Fiorina model has no probabilities in it whatsoever. Their model without probabilities, however, is equivalent to an equiprobable model.

Table 3. Relevant Electoral Outcomes for a Two-Candidate Election

States of Nature	Description
$O_1$	Candidate 1, the most preferred candidate wins by more than one vote.
$O_2$	Candidate 1 wins by exactly one vote.
$O_3$	Candidate 1 and Candidate 2 tie; a coin flip decides winner.
$O_4$	Candidate 1 loses by exactly one vote.
$O_5$	Candidate 1 loses by more than one vote.

Table 4. Hypothesized Utility Associated with States of Nature when Voting and Abstaining

Symbol	Conclusion	Rationale
$U_{1j}^v$	Positive, moderate size.	The utility citizen $j$ would receive from the victory of his most preferred candidate as well as from contributing to the size of the victory. Likely to diminish as the margin of victory increases.
$U_{2j}^v$	Positive, large.	The utility $j$ would receive from deciding the election in favor of his most preferred candidate.
$U_{3j}^v$	Positive, large but less than $U_{2j}^v$ .	The utility $j$ would receive from saving his most preferred candidate from sure defeat.
$U_{4j}^v$	Negative, moderate.	The disutility $j$ would receive from the loss of his most preferred candidate.
$U_{5j}^v$	Negative, moderate.	Same as for $U_{4j}^v$ .
$U_{1j}^a$	Positive, moderate.	The utility $j$ would receive from the victory of his most preferred candidate.
$U_{2j}^a$	Positive, small.	Same as for $U_{1j}^a$ .
$U_{3j}^a$	Negative, large.	If $j$ had voted he could have decided the election. But he did not and now his most preferred candidate has only a .5 probability of winning. This assumes a "Damn, if I had only voted" psychology, especially if his candidate loses the flip.
$U_{4j}^a$	Negative, large.	Almost the same as above; here if $j$ had voted he could have given his most preferred candidate a .5 probability of winning whereas, by abstaining, his most preferred candidate loses.
$U_{5j}^a$	Negative, moderate.	The disutility $j$ would receive from the loss of his most preferred candidate.

from the victory of the least-preferred candidate. Thus, in this model the  $B$  term represents only the utility difference derived from the victory of one candidate over the other. I will attempt in the following to show that this is a quite narrow interpretation of  $B$  and that, in particular, it fails to take account of how the victory is attained.

The five relevant electoral outcomes for a two-candidate election are given in Table 3. Let  $U_{ij}^v$  be the utility that citizen  $j$  perceives he would derive if outcome  $O_i$  occurs when he votes and  $U_{ij}^a$  be the utility when  $O_i$  occurs when he abstains. I interpret  $U_{ij}^v$  and  $U_{ij}^a$  as follows: let citizen  $j$  be deciding whether or not he will vote; he asks himself, "How would I feel if I voted and  $O_i$  were the outcome?" This is  $U_{ij}^v$ . Similarly, he asks, "How would I feel if I did not vote and  $O_i$  were the outcome?" This is  $U_{ij}^a$ . Hypotheses about the magnitude of these outcome related utilities are presented in Table 4.

The main feature of these hypothesized magnitudes is that they are not symmetrical for voting and abstaining.<sup>15</sup> Most important, I hypothesize

that the utility derived from deciding an election in favor of a voter's most-preferred candidate,  $U_{2j}^v$ , is positive and is large, while the utilities of failing to vote and, then having one's most-preferred candidate lose ( $U_{4j}^a$ ) or draw ( $U_{3j}^a$ ) are negative (i.e., are disutilities) and are large. In part these outcome-related utilities reflect the utility a voter would receive if his most-preferred candidate won or lost. But they also reflect the utilities (or disutilities) a citizen receives from deciding (or failing to decide) the electoral outcome. For example, if a citizen votes and  $O_2$  occurs, he will receive utility from the victory of his most-preferred candidate as well as from having decided the election himself. I would venture that it is reasonable to assume this latter degree of utility to be quite large. And, analogously, it appears reasonable to assume great disutility from failing to decide an election when it was possible to do so.

To this point the model is quite similar to the minimax regret model. The similarity ends, however, with the introduction of probabilities. Let  $P_{ij}$  be citizen  $j$ 's subjective estimation that out-

<sup>15</sup> A somewhat similar situation is the choice between gambling (seeking risk) and insurance (avoiding risk); see Milton Friedman and Leonard J. Savage, "The Utility Analysis of Choices Involving Risk,"

come  $O_i$  will occur when he votes and let  $Q_{ij}$  be  $j$ 's estimation that  $O_i$  will occur when he does not vote. I require that

$$\sum_{i=1}^5 P_{ij} = \sum_{i=1}^5 Q_{ij} = 1.$$

This requirement for the probabilities together with an assumption that the electorate is relatively large determine that  $P_{1j}$ ,  $P_{5j}$ ,  $Q_{1j}$ , and  $Q_{5j}$  are relatively large while  $P_{2j}$ ,  $P_{3j}$ ,  $P_{4j}$ ,  $Q_{2j}$ ,  $Q_{3j}$ , and  $Q_{4j}$  are quite small. This is equivalent to saying that the probability of any one citizen's affecting the electoral outcome is quite small whether he votes or not. These latter probabilities increase rapidly, however, as an election is perceived to be a close contest.

With these specifications I can state the citizen's expected utility from voting as follows:

$$(4) \quad \sum_{i=1}^5 P_{ij}(U_{ij}^v) - C,$$

where, as in the Downs model,  $C$  is the utility cost of voting. Similarly, the expected utility from abstaining is

$$(5) \quad \sum_{i=1}^5 Q_{ij}(U_{ij}^a).$$

Hence, in this model, it is rational for citizen  $j$  to vote if

$$(6) \quad \sum_{i=1}^5 P_{ij}(U_{ij}^v) - C > \sum_{i=1}^5 Q_{ij}(U_{ij}^a).$$

Transposing terms as follows, if the difference between a citizen's expected value of voting and of abstaining exceed the costs of voting, he will vote:

$$(7) \quad \sum_{i=1}^5 P_{ij}(U_{ij}^v) - \sum_{i=1}^5 Q_{ij}(U_{ij}^a) > C.$$

Examining Table 4, we can see that  $U_{4j}^v$ ,  $U_{5j}^v$ ,  $U_{3j}^a$ ,  $U_{4j}^a$ , and  $U_{5j}^a$  are negative. Furthermore, equation (7) can be written as:

$$(8) \quad P_{1j}(U_{1j}^v) + P_{2j}(U_{2j}^v) + P_{3j}(U_{3j}^v) \\ + Q_{3j}(U_{3j}^a) + Q_{4j}(U_{4j}^a) + Q_{5j}(U_{5j}^a) \\ > C - P_{4j}(U_{4j}^v) - P_{5j}(U_{5j}^v) \\ + Q_{1j}(U_{1j}^a) + Q_{2j}(U_{2j}^a).$$

Now all the factors on the left of the inequality

contribute positively to voting while all the factors on the right side contribute positively to abstention. What may initially seem surprising is that certain of the utilities connected with abstention may be important inducements to vote while certain of the utilities associated with voting contribute to abstention. Said another way though, this is less surprising; that is, the disutility of abstaining contributes to voting while the disutility of voting contributes to abstaining.

Does this model predict more voting than the Downs model? I believe that it does if the hypotheses in Table 4 are correct for a substantial number of citizens. This can be shown by simplifying (8) with the help of two assumptions. For a reasonably large electorate,  $P_{ij}$  will closely approximate  $Q_{ij}$ . If then  $U_{1j}^v$  is assumed to equal  $U_{1j}^a$  (utility derived from a victory by the more preferred candidate is the same whether  $j$  votes or abstains) and if  $U_{5j}^v$  and  $U_{5j}^a$  are equal (the disutility of a loss by more than one vote of the most preferred candidate is the same whether  $j$  votes or abstains), then (8) reduces to:

$$(9) \quad P_{2j}(U_{2j}^v) + P_{3j}(U_{3j}^v) + Q_{3j}(U_{3j}^a) \\ + Q_{4j}(U_{4j}^a) > C + P_{4j}(U_{4j}^v) \\ + Q_{2j}(U_{2j}^a).$$

Because, from Table 4,  $U_{4j}^v$  is only moderately large, while  $P_{4j}$  is likely to be quite small,  $P_{4j}(U_{4j}^v)$  is likely to be very small. The same is true for the same reasons for  $Q_{2j}(U_{2j}^a)$ . Hence, the only significant term to the right of the inequality is  $C$ , the cost of voting. But each of the  $U_{ij}$  terms on the left of the inequality in (9) is assumed to be large. Hence, even though all the corresponding  $P_{ij}$  and  $Q_{ij}$  terms are quite small, the terms on the left in (9) may not disappear, so that added together, they may well be larger than  $C$  for a substantial number of citizens.

Further, note that the model predicts increased voting in close elections because, in this case,  $P_{ij}$  and  $Q_{ij}$  for  $i=2, 3, 4$ , are likely to be larger than in elections perceived to be one sided. And, as these  $P_{ij}$  and  $Q_{ij}$  factors increase, the magnitude of the factors on the left side of the inequality in (9) increase while  $C$  on the right side remains constant.

### Summary and Conclusions

It should be clear that the theory presented here departs from both the expected utility model of voting and the Ferejohn-Fiorina minimax regret model. As noted above, it completely changes and broadens the interpretation of the Downsian



*B* term.<sup>16</sup> This is accomplished by deleting the Downsian assumption that utility is derived only when one affects the outcome by voting and, in its place, putting the assumption that an individual associates different utility payoffs with each of a set of electoral outcomes, these outcomes being jointly determined by the state of nature and the

individual's actions. Intuitively this seems a more appropriate assumption than either the restricted Downsian assumption or the Ferejohn-Fiorina assumption that citizens make an equiprobable assumption over the set of relevant electoral outcomes. Further, with some assumptions about the magnitude of the set of outcome related utilities, it appears that this model leads to predictions of more voting than does the original Downs model without, at the same time, introducing a different rationality calculus or a problematic *D*-variable.

<sup>16</sup> One different reinterpretation of the *B* term is presented in George J. Stigler, "Economic Competition and Political Competition," *Public Choice*, 13 (Fall, 1972), 91-106.

# The Paradox of Not Voting: Comment

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It is continually demonstrated in the pages of the *Review* that formal theorists in political science have great difficulty in communicating with one another, in reaching consensus, e.g., on the rationality of minimal winning coalitions and the effects of vote-trading. It has always puzzled me how such confusion and controversy could surround simple arguments which are—or at least appear to be—mathematical. Reading the extraordinarily clear contribution of Ferejohn and Fiorina ("The Paradox of Not Voting: A Decision Theoretic Analysis," *APSR* 68 [June, 1974], 525–536), I am led to the conjecture that a sort of natural selection is involved, that the more lucid papers are so patently silly that many of them fail to achieve publication, leaving the field to papers in which the nonsense is at least obscure.

The authors ask whether "the principal explanation of the voting act is found in its investment or its consumption aspect," and I take their contribution to be the suggestion that some voters are rational investors who obey not the principle of maximization of expected utility, but the more esoteric principle of "minimax-regret." (We cannot pause to consider whether the voting act, any more than the sex act or any other darkly functional social behavior suffused with myth and ritual, is adequately treated with economic categories.) The idea of minimax-regret is that the decision-maker pays no heed to the *probability* that nature is in particular states, but rather democratically scans all *possible* states of nature, saying to himself, "Suppose nature is this way—if I do  $V$ , my regret will be  $c$ ; if I do  $A$ , my regret will be 0 . . .", until he has constructed for himself a "regret matrix," states-of-nature by courses-of-action. (For the precise meaning of "regret," see below.) Examining this matrix, the decision maker chooses that action which exhibits the smallest maximum regret. The authors suggest that someone making a voting decision in a straight fight will ordinarily associate a regret of  $c$ —the cost of bothering to go to the polls—with a vote for either candidate, and a regret of 0 with abstaining: the idea is that ordinarily one candidate will win by a number of votes, and the voter will realize after the election that his vote did not matter; he will regret that he bothered. But, ah, it is just *possible* that the voter could make or break a tie; if he abstains and his candidate loses by one vote, he will be remorseful indeed. Thus, obeying the principle of minimax regret, he

possibility. There are perhaps 50,000 Americans killed annually in motor-vehicle accidents; say 150 a day—it has long been a commonplace that deaths are *not* highly concentrated in holiday-weekend "carnage." For every death, there are several serious injuries; say serious casualties amount to 750 a day. Suppose that on a presidential election day, *one per cent* of these casualties are suffered in traveling to and from the polls; thus the voting act will be profoundly costly for about one voter in ten million. The probability of a tied presidential election is more difficult to assess, owing in part to the electoral college, in part to the possibility of litigation of a very close result, and so on. Let us suppose a popular election, and that a "close" election is one in which the probability distribution of votes for the Democratic candidate is rectangular with a range of one million; in such a situation, the chance that one's vote would be tie-making or tie-breaking is on the order of one in a million.

If a minimax-regretter is going to dip down six factors of ten in probability to pick up the possibility that his sole vote may decide a presidential election, it is difficult for me to see that he will not dip down another factor of ten and pick up the possibility that he will be run over; I am sure there are many professors of political science who would gladly give their legs to elect a Democratic president, but I do not believe such intensity of sentiment is widely distributed in the population. Note that numerical probabilities are actually irrelevant to a minimax-regretter—it is enough to recognize the possibility. We may consider it fortunate that few voters will have imaginations vivid enough to consider such outcomes as: (a) one elects one's candidate by a single vote, (b) one then is in an accident entailing total paralysis, and finally, (c) languishing in the nursing home, one sees one's candidate metamorphose into another Caligula.

This decision-theoretic *grand guignol* is intended to suggest that the juridical outcome of an election—win, lose, or draw,—is not the only uncertainty facing a potential voter. Ferejohn and Fiorina suppose that the voter's cost of voting and the utility he associates with the various outcomes are known and fixed parameters; but surely most voters so thorough as to consider the possibility of a tied outcome will also consider the possibility that their costs and utilities are to some degree uncertain as well. For the decision theorist, alternate states of nature are the symbols,  $S_1$ ,  $S_2$ ,  $S_3$ , printed upon the page; for the man in the world,

Let us consider for a moment another remote

the alternate states of nature frequently must seem indefinite in character and infinite in number, and any final decision based on minimax-regret (which assumes strict symmetry among possible states of nature) will depend mostly on how imaginative the decision maker is in formulating the alternate states of nature.

In a straight fight, there may be some people who choose to vote in just the way the authors suggest; I doubt they are very numerous, and I expect that it would in practice be exquisitely difficult to locate them, for it would be necessary to distinguish them from other persons for whom tied outcomes would be salient for other reasons: some of these would be utility maximizers who grossly exaggerate the probability of a tied outcome (to the extreme of those who apply the principle of insufficient reason, supposing tied and nontied outcomes equiprobable!), and others would associate *different* utilities with the victory of a particular candidate, depending on whether their vote was *necessary* for his victory. This latter possibility is quite distinct from "regret" in the technical sense, a point which wholly escaped the authors and presumably most of the eight theorists whom they thank in their first footnote. Regret has nothing to do with guilt or responsibility—it is the difference between what you "get" acting the way you did and what you might have gotten if you had known in advance the true state of nature and acted optimally on that knowledge (e.g., not voted for a candidate who was going to win anyway). "What you get" is the utility you associate with the victory of a particular candidate (independent of how he is elected—by a landslide, a single vote, or a coin-flip), decremented—if you vote—by your cost of voting. In spite of this, the authors attribute to a minimax-regretter the reasoning, "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." Now one does not normally feel suicidal if one's preferred candidate *loses*; one might, however, if one were *responsible* for his loss. A minimax-regret voter actually feels slightly better if his candidate loses by one vote with the voter abstaining (the payoff is zero), than he feels if the candidate loses in spite of the voter's support: (the payoff is  $-c$ , zero decremented by the cost of voting). The reader who finds this bizarre should examine cells ( $S_4, A$ ) and ( $S_5, V_1$ ) of the authors' Table 2, page 527. This is not the "regret matrix" but the "payoff matrix"—it is the latter that tells you how the voter "feels" after the outcome. (I hope the authors will not dare to argue that "payoffs" and "regrets" in their model are somehow empirically independent experiences: the regrets are nothing more than arithmetic differences of payoffs, and cannot be interpreted as an experience of remorse or pride separate from the value or disvalue the voter

associates with the election result itself. In the real world, voters with identical valuations of outcomes might well differ in their "sense of responsibility," and the mode of calculating regrets of course makes no allowance for this.)

We have been considering the two-candidate model of Ferejohn and Fiorina: the three-candidate model is simply grotesque. The maximizer of expected utility must estimate *eighteen* subjective probabilities, corresponding to such fantastic outcomes as a two-way tie for first place with the third candidate one vote behind. The authors appear seriously to suggest that a voter may examine the sense of an inequality containing *twenty-four* terms in order to decide whether he should vote for his first or second choice. The poor voter who—perhaps in order to avoid the computational burden—chooses to become a minimax-regret decision maker is faced with an even stranger situation. If he is indifferent between his two favored candidates, his minimax-regret rational behavior is abstention, for any non-zero cost of voting. This occurs because he must recognize the possibility that if he votes for one of his preferred candidates, the other may lose to the disfavored candidate by one vote. The voter thus may incur regret both from his failure to tie the election and from his cost of voting; by abstaining altogether, he may avoid the latter. (I have struggled to see that the result is some sort of technical error, but I do not believe it is.) I will agree to let the decision theorist call such behavior "minimax-regret rational" if he will agree that I may call it silly.

Papers such as this one are frequently no more elegant than they are sensible. For example, the inequalities in the second column of page 534 are very clumsily expressed, about a dozen being used where three or four would suffice (this out of *Caltech*, yet). Conditions (7) and (8) may be summarized: if  $k \geq 1/2$ , the voter may rationally vote rather than abstain if  $c < 1/2 - k/2$ ; if  $k \leq 1/2$ , the voter may vote if  $c < 1/3 - k/6$ ; to see this, it helps to make a sketch.

Working through a paper such as "The Paradox of Not Voting" may be a valuable and absorbing exercise, so long as one keeps in mind that one may be dealing with a clay pigeon unable to fly on its own. When such a paper is clear enough to be followed, the models it offers generally show themselves to be deficient in a dozen respects and preposterous in several, and the affirmative claims of such papers are commonly almost wholly spurious. The model presented by Ferejohn and Fiorina neither provides a plausible "theoretical explanation of voting turnout," nor "a means of organizing and explaining the many empirical correlations which exist" (p. 536). We may regret that so few formal papers are so clear, and that so few readers of the *Review* are likely to give such papers the critical examination they deserve.

# Is Minimax Regret Applicable to Voting Decisions?

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In a recent article ("The Paradox of Not Voting: A Decision Theoretic Analysis," *APSR*, 68 [June, 1974], 525-536) John Ferejohn and Morris Fiorina suggest that a (potential) voter may be rational in deciding whether to vote and yet not use the expected-utility calculus introduced for this purpose by Downs and Tullock and extended by Riker and Ordeshook. The authors suggest that a rational voter, in deciding whether to vote, may use the minimax regret criterion due to the late L. J. Savage.<sup>1</sup> Define the regret of an Act A as the difference, if the true state of nature were known (in advance), between the best expected payoff that could be obtained and the expected payoff that would be obtained by act A. Then the minimax regret criterion requires the decision maker to act in such a way as to minimize the maximum regret that he can suffer.

They also discuss the fact that the minimax regret criterion does not require the voter to supply any (objective or subjective) estimates of the probabilities associated with the various outcomes of the election. Thus they feel that it is an attractive criterion for decision making under "uncertainty" as opposed to decision making under "risk."

We first take issue with the notion that a decision must be made either under risk or under uncertainty, for there is an entire continuum of situations (which we shall call quasi-risk situations) interposable between the two. We then argue that the minimax regret criterion is not a reasonable criterion even under complete uncertainty when one does not have an intelligent opponent. Furthermore we shall argue that the problem of deciding whether to vote usually occurs in a quasi-risk situation and that the minimax regret criterion is inappropriate for such situations since it ignores relevant information. We also claim that most voters do not use the minimax regret criterion.

The distinction that the authors make between decision under risk and decision under uncertainty is the usual one that (i) under risk the probabilities associated with the various events that

can occur are known or can be estimated by the decision maker in the sense that he can make statements such as "the probability of event A occurring is .35" and (ii) under uncertainty he can say nothing about the probabilities associated with the various events. Since the probabilities of the possible outcomes in an election are not known or easily estimated by the voter, Ferejohn and Fiorina conclude that the decision to vote is not a decision under risk and therefore must be a decision under uncertainty. Consistent with the theory of subjective probability proposed by one of us,<sup>2</sup> we propose that there are many quasi-risk situations in which the decision maker is not able to supply a "sharp" (single value) estimate of the probabilities of various outcomes, but that he is not totally ignorant of these probabilities. In one Bayesian spirit he may be able to put a prior distribution on these probabilities<sup>3</sup> but more often he would be able to suggest an interval in which he feels the probability of a given event lies. This information is not reasonably ignored in the decision process. For example, the problem whether to vote may be one in which the voter can say that "the probability of Nixon's victory is greater than 0.5." If so, then the voter is in a quasi-risk situation.

Suppose, for the moment only, that the decision to vote is made under complete uncertainty. This assumes that the voter does not suspect that the probability of a tie is small when the electorate is large. The minimax regret criterion would not be a reasonable one for the voter to use since it is a conservative strategy in the sense that it protects the voter against an intelligent opponent whose own interests are contrary to the interests of the voter. When playing against an intelligent opponent who has many strategies at his disposal it may be reasonable to protect one's self by minimizing the maximum possible regret. But the outcome of the election is not chosen by an intelligent opponent who is trying to irritate the voter, for the outcome of a tie is not one determined by any one person. Therefore, the minimax

<sup>1</sup> Savage (in *The Foundations of Statistics*), preferred the term "loss" to "regret" in this context, but "regret" is now the more standard term because of another technical meaning for "loss."

<sup>2</sup> For example, I. J. Good, *Probability and the Weighing of Evidence* (London: Charles Griffin; New York: Hafners, 1950).

<sup>3</sup> For example, I. J. Good, "Rational decisions," *Journal of the Royal Statistical Society*, series B, 14, (1952), 107-114.

regret criterion is apparently not reasonable when deciding whether to vote even if the decision were made under uncertainty.

Although we have just supposed temporarily that voting could be considered to be decision making under uncertainty, we actually claim that it is clearly a case of decision making under quasi-risk for the following reasons. Although the decision to vote is not a decision under risk since the voters do not give point estimates of the probabilities of events such as the event of a tie, we claim that the voters do take into consideration their rough estimates of the probabilities associated with certain events such as a "close race" or a "landslide." For the turnout tends to be greater in a close contest than in a landslide. If the average

voter has more incentive to vote when the election is "on the fence" then his decision cannot be made under uncertainty.

We agree with Ferejohn and Fiorina that the voter does not make (point) estimates of the probabilities associated with all possible outcomes and then makes a decision under risk. But we feel it at least as unrealistic to assume that the voter ignores his rough ideas concerning the outcome of the election so his decision is not made under uncertainty. A study is needed of the conditions under which it is rational to vote when the decision is under partial risk. Empirical work could be used to determine the type of partial estimates that voters can make of the probabilities associated with various outcomes of the election.

# The Paradox of Minimax Regret

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The introduction of decision making under uncertainty by Ferejohn and Fiorina<sup>1</sup> is an interesting addition to the literature on rational theories of citizen participation. Decision making under uncertainty assumes, however, that the actor has no knowledge of the probabilities of the various outcomes; this is obviously no more true than the assumption of perfect information about these probabilities made in the decision making under risk model. Voters have some, but not perfect, information about the probabilities of at least some of the different possible outcomes.

Specifically, let us look at the two-party case. In Ferejohn's notation,  $(p_3 + p_4)$  is the probability of an individual's vote making a difference. We might expect a rational citizen to know that this probability is at most minuscule, even if he cannot calculate its exact value. Now, according to the uncertainty analysis (p. 528), a rational citizen might vote because he fears that his vote will make a difference (i.e., the true state of the world is

either  $S_3$  or  $S_4$ ). But because the potential voter knows that  $(p_3 + p_4)$  is minuscule, he is in the position of comparing the near certainty of a small loss against the near impossibility of a large gain. But this returns us to the decision-making-under-risk case (except that we are now dealing with a citizen's guess at maximal values of probabilities rather than his best guess of those probabilities). Thus, given the relative magnitudes of the gains and losses and probabilities associated with voting, it is still unlikely that a citizen would choose to vote on purely rational grounds, no matter which form of decision making he uses.

Furthermore, minimax regret has an additional defect. Presumably, Ferejohn and Fiorina's analysis should hold for any decision-making situation that is formally equivalent to the decision of whether or not to vote. Thus, they are in the unhappy position of arguing that one should always forego a near-certain small gain in order to avoid the minuscule possibility of a large loss. In particular, they would have to argue that one should never cross a street to buy a newspaper. A decision-making procedure that produces such consequences clearly has its defects.

<sup>1</sup> John Ferejohn and Morris Fiorina, "The Paradox of Not Voting: A Decision Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525-535.

# The Paradox of Not Voting for Oneself

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In most states it is permitted to write in the names on the ballot, and these write-in candidates can win if they assemble enough votes. Thus, in all elections in which I might conceivably participate, there are at least three candidates: the Republicans, the Democrats, and my own favorite candidate for president—Gordon Tullock. Indeed, using the notation of the Ferejohn and Fiorina article, assuming that we are talking about 1972, I would find  $k$  having a value of about .001. Under the circumstances, if I understand the article correctly, I should always vote, and the vote should always take the form of writing in my own name. Further, as far as I can see, this advice can be generalized. Everyone who would really like to be president should vote and write in his own name, because the minimum regret that they produce for three-candidate elections is also correct for the elections in which there are 30 million candidates. The only problem here is that it would, of course, amount to participating in a lottery, and my possible gain from writing in my own name might turn out to be less than the cost of writing it in. Granted that people are willing to buy lottery tickets, even when the various states offering them take very substantial rakeoffs, I do not imagine this would be much of a disadvantage.

This same conclusion could have been deduced from the Casstevens article, which Ferejohn and Fiorina quite properly criticize as being mathematically incorrect.<sup>1</sup> There is, of course, nothing

wrong with the Ferejohn and Fiorina mathematics. Nevertheless, the conclusion is such as to raise considerable doubts about whether they have chosen the right assumptions. Like them, I have always felt that the *ad hoc* procedure of putting in a "D" is undesirable.<sup>2</sup> I would like a better solution. It does not seem to me, however, that their solution is really better.

It is possible to sketch out at least a partial solution to this problem. Clearly the cost of voting is very slight. Under the circumstances, individuals would not be well-advised to invest large resources in determining whether the conventional wisdom (which they picked up in school) about the duty and value of voting is correct. Further, in the present state of the literature, it must be admitted that finding out that voting is not a paying proposition would be quite expensive. Under the circumstances, people making decisions about whether or not to expend resources on *investigating* whether or not they should stop voting might, quite rationally, decide not to make the *investigation*. This explanation for widespread voting is, of course, in addition to the usual social pressure arguments for voting, and the other conventional argument that many people have been indoctrinated in such a way that they get a positive pleasure from voting.

<sup>2</sup> The assumption that  $C_v$  takes a minus value is made in Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: University of Michigan Press, 1967), p. 110.

<sup>1</sup> Thomas Casstevens, "A Theorem About Voting," *APSR*, 62 (March, 1968), 205-207.

# Closeness Counts Only in Horseshoes and Dancing

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## Introduction

During the period in which our article (*APSR* vol. 68 [June 1974]) circulated in manuscript form it provoked an unusual amount of collegial reaction. Of course, we were quite prepared for a reaction from those who use decision-theoretic models in their research—they were our intended audience. More surprisingly, we also received comments from less directly involved bystanders—a medieval historian for example. All this correspondence indicates to us that nearly everyone has his own theory of how voters behave, and that most such theories do not agree with the one presented in our article. The comments of Professors Tullock, Beck, Mayer and Good, and Stephens further support this conclusion.

In an appendix to this note we have responded to the imaginative point raised by Tullock. As for the traditional questions raised by our other critics, however, we adopt a different line of rebuttal. Rather than conduct an unfruitful debate over the *a priori* plausibility of the minimax regret model we will do something that theorists too seldom do: examine some data. Before doing so we will make an important distinction between using a model prescriptively and using it descriptively. (Decision-theoretic types tend to move a bit too easily from one usage to the other.) Then, after reviewing the major point of our article we will turn to the data.

## Prescription and Description

Much of the literature in decision theory reflects a search for optimal decision procedures—those which an intelligent, purposeful being *should* follow to make the “best” decision relative to his values and beliefs. Many theories of decision making found in the literature are avowedly *not* descriptive theories. Savage, for example, always viewed his theory of subjective expected utility maximization as having primarily prescriptive value.<sup>1</sup> Raiffa expresses an identical view.<sup>2</sup> The theory can be used to *guide* a decision maker, to enable him to make better decisions than he might were he to use his intuitive judgments. Thus, to take some particular theory of decision making and assert that it describes the behavior

of ordinary people obviously entails a large leap into faith, or to put it more professionally, a very bold hypothesis.<sup>3</sup>

Those who apply decision-theoretic models to the study of politics sometimes blend the different usages of these models, now rejecting a model as descriptive because it seems unreasonable as a prescriptive model (e.g. minimax regret), then accepting a model as descriptive because it seems reasonable as a prescriptive model (e.g. expected utility). Consider some excerpts from the Mayer-Good comment:

... we shall argue that the problem of deciding whether to vote usually occurs in a quasi-risk situation and that the minimax regret criterion *is inappropriate for such situations since it ignores relevant information*. We also claim that most voters *do not* use the minimax regret criterion [our emphasis].

The first sentence asserts that the minimax regret criterion is unacceptable as a prescriptive model, i.e., rational voters *should not* behave that way. The second sentence advances an empirical claim—that voters *do not* behave as minimax regretters. Again,

The minimax regret criterion *would not be a reasonable* one for the voter to use since it is a conservative strategy in the sense that it protects the voter against an intelligent opponent whose own interests are contrary to the interests of the voter. ... Therefore the minimax regret criterion is apparently *not reasonable* when deciding whether to vote, even if the decision were made under uncertainty.

Here, Mayer and Good are rejecting the model because they believe it to be “unreasonable,” not descriptively wrong. But later they shift back:

... the turnout tends to be greater in a close contest than in a landslide. If the average voter has more incentive to vote when the election is “on the fence,” then his decision cannot be made under uncertainty.

<sup>3</sup> A number of recent experiments have cast serious doubt on the descriptive value of prevailing formulations of expected utility models. See, for example, two articles by Sarah Lichtenstein and Paul Slovic, “Reversals of Preference Between Bids and Choices in Gambling Decisions,” *Journal of Experimental Psychology*, 89 (July, 1971), 46–55. And “Relative Importance of Probabilities and Payoffs in Risk Taking,” *Journal of Experimental Psychology Monograph*, 78 (December, 1968), 1–18. Also, Kenneth R. MacCrimmon, “Descriptive and Normative Implications of the Decision Theory Postulates,” in *Risk and Uncertainty*, ed. Karl Borch and Jan Mossin (London, Macmillan, 1968), pp. 3–32.

<sup>1</sup> Leonard J. Savage, *The Foundations of Statistics*, 2nd rev. ed. (New York: Dover, 1972), pp. 19–21, 59, *passim*.

<sup>2</sup> Howard Raiffa, *Decision Analysis* (Reading, Mass.: Addison-Wesley, 1968), p. x.



Here we have an empirical claim clearly subject to falsification.

Similarly, consider Beck's and Stephens's comments. Upon first reading they appear to be rejecting the minimax regret model on empirical grounds. People do cross streets and drive on freeways when there is some nonzero probability of being wiped out by doing so. Such behavior is contrary to a minimax regret analysis of these situations. But again, such arguments reflect an implicit assumption arising from the prescriptive decision-theoretic literature; namely, that the domain of a decision theory is universal, that people should decide according to a given model in any and all contests. Such generality would be nice. Unfortunately, recent experimental work by Charles Plott and Michael Levine raises the messy possibility that individuals act as if they vary their decision rules in response to the decision context (i.e., the available alternatives and the consequences).<sup>4</sup> We agree with Beck and Stephens that minimax regret fails as a descriptive model of freeway driving behavior, but this observation in principle bears no relation to the accuracy of minimax regret as a descriptive model of voting behavior.<sup>5</sup>

To summarize our position, then, we are prepared to joust with our critics about the descriptive accuracy of various decision-theoretic models of voting behavior. But we reject the notion that a descriptive theory can be rejected on the grounds that the model underlying it appears implausible when advanced as a *prescriptive* theory. After all, the American electorate does not have the benefit of televised election eve lectures by Professors Mayer, Good, Beck and Stephens on the subject of reasonable voting behavior. How the citizenry behaves and how our fellow professors believe it should behave are two different questions. Our concern is only with the former.

#### What Did We Say?

Although Professor Stephens compliments our "extraordinarily clear contribution," we apparently were not quite clear enough. In the first paragraph of our article we wrote

... whatever their ability to account for such general

<sup>4</sup> Charles Plott and Michael Levine, "On Using the Agenda to Influence Group Decisions: Theory, Experiments and an Application," (mimeograph: California Institute of Technology, 1974).

<sup>5</sup> As we indicated earlier, examples such as crossing streets and driving on freeways are traditionally held up against minimax decision rules. For a discussion see R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: Wiley, 1957), pp. 278-282. Professors Beck and Stephens advance such criticisms as if we were unaware of them. Not so. We simply regard these criticisms as not telling when one works in the descriptive mode.

phenomena as candidate strategy and electoral outcomes, many believe that rational choice models do not provide a satisfactory explanation for the most basic political decision in a democratic system: to vote or not to vote. The purpose of this paper is to show *one means* of rescuing rational choice theorists from this embarrassing predicament [our emphasis].

Several pages later we wrote

In this section, we will demonstrate that one well-known classical procedure yields a quite weak condition for voting vis-à-vis abstaining.

Finally, in our conclusion we wrote

Rational-choice theorists are guilty of equating the notion of rational behavior with the rule of maximizing expected utility. Alternatively, we usually define the first as the second. Yet although the rule of maximizing expected utility is the most widely known, widely used and widely accepted rationality criterion, it is not the only one.

As the preceding comments indicate, the major point of our article is simply to remind some of our colleagues that rational behavior is a much broader notion than expected utility maximizing. To pose the alternatives as either expected utility maximizing or "... the mysterious and inexplicable world of the irrational" is far too stark a contrast.<sup>6</sup> In our article we simply offered another alternative without necessarily implying that it was the only additional possibility. This is still our position. We are not so bold as to argue that all voters literally *are* minimax regret decision makers. We argue only that the minimax regret model of voting behavior is a model which has at least one redeeming feature: it does not make the patently wrong prediction (universal abstention) that the Downsian expected utility model does.

Moreover, the minimax regret model can be differentiated empirically from the Downsian expected utility model. The minimax regret model is one member of a class of models in which *B* matters but not *p*. The Downsian expected utility model is a member of a class of models in which the product (*pB*) matters. In the remainder of this note we will evaluate these two classes of models using survey data on the American voter. The critics say that it is unreasonable for people to ignore the probability that they affect the election outcome when they vote. But what does the evidence say?

#### Analysis

The Downsian expected utility model specifies that a citizen should vote if

$$(p_3 + p_4)(B/2) > c,$$

<sup>6</sup> William Riker and Peter Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25.

where  $p_a$  is the probability a citizen breaks a tie,  $p_t$  is the probability he creates a tie,  $B$  is his party differential, and  $c$  his cost of voting. As we noted in our article, virtually no one votes if this model holds. Thus, additions such as the Riker-Ordeshook<sup>7</sup>  $D$  term must be postulated if one is to take the model seriously for predictive purposes.

In contrast, the basic minimax regret model specifies that a citizen should vote if

$$B/4 > c$$

Clearly, this condition will hold for any citizen with a nontrivial party differential, unless his costs of voting are unusually high (e.g., blacks in the South until recently).

For purposes of differentiating between the two models empirically, a simple test may be performed. Under the expected utility model, perceived closeness of the election affects the outcome; under the minimax regret model, it does not.

To elaborate, in the Downsian expected utility model closeness of the election interacts with the party differential. If the citizen's party differential is trivial, closeness should have little or no effect on his decision. Similarly, if he does not see the election as at all close, the size of the party differential should have little effect. The main effect should occur when both a nontrivial party differential and a perception of a close election occur simultaneously.<sup>7</sup> For the minimax regret model, however, closeness of the election is irrelevant. As a citizen's party differential increases, the likelihood of his voting increases, irrespective of  $p$ .

Thus we have the following empirical hypotheses to test:

H—EU: (Expected utility) turnout increases as the product ( $pB$ ) increases.

H—MR: (Minimax Regret) turnout increases as  $B$  increases.

Naturally, between theoretical variables and empirical indicators a large gap intervenes. We cannot hope to measure  $p$  and  $B$  very precisely. But data exist which allow a rough test of these hypotheses, and given that proponents of expected utility models previously have employed these data, we feel justified in following suit.

The data are drawn from the 1952 to 1964 presidential election surveys conducted by the University of Michigan SRC.<sup>8</sup> In each survey

there is a question which asks a respondent how close he believes the election to be. As a proxy for  $p$  we took the answers to this question and dichotomized them into close and not close.<sup>9</sup>

Our measures of the party differential varied across elections. For the 1960 and 1964 elections we took the answers to the most-important-problems questions, weighted them by the respondent's professed degree of concern, and assigned a polarity according to which party could best handle the problem. Thus, the party differential for these years reflects issue concerns almost exclusively.<sup>10</sup> The most-important-problems questions were not asked prior to 1960. Thus we followed Riker and Ordeshook in considering the respondent's degree of concern over the electoral outcome as an estimate of  $B$  for 1952 and 1956.<sup>11</sup>

As a measure of turnout should one use pre-election intentions, or post-election reports? The first are noticeably higher than the second, yet the second are elicited at a point in time considerably removed from a respondent's estimates of  $p$  and  $B$ . Seeing no conclusive argument for either, we have examined both.

Finally, we have examined turnout among registered voters only (i.e., those self-reported as registered). The models under consideration predict variations in abstention rates. But if a citizen is not registered, we have no way of knowing whether or not he would have voted had he been eligible. Some might contend that a decision not to register should be considered equivalent to a decision not to vote. But we do not view the matter so simply, particularly in reference to a time when registration requirements were more stringent than today. The models under consideration predict variations in abstentions when voting is an available alternative. Tests of such models should not depend on citizens with low party differentials or low  $p$ -terms concentrating among the legally ineligible.

Having outlined these numerous practical decisions, we now proceed to the data. We will

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responsibility for our interpretation of the data. The 1968 survey is not used in the following analysis because of the three-candidate race. We do not possess the 1972 data.

<sup>9</sup> Respondents' perceptions of closeness of the election are coded into four main categories ranging from landslide to tie. We have collapsed these categories into not close (win by landslide, or win by quite a lot) and close (win by a little, even). We assume that  $p$  tends to be lower for the first category of respondents.

<sup>10</sup> In the analysis which follows the full range of the scale is not used. Respondents simply are dichotomized into zero scorers and everyone else.

<sup>11</sup> These responses, too, were dichotomized. Those who responded "don't care," or "care a little" were considered low party differential, "care," and "care a lot" as high party differential.

<sup>7</sup> Indeed, Angus Campbell et al. claim that such an interaction occurs in 1956. See *The American Voter* (New York: Wiley, 1960), Table 5-3, p. 99.

<sup>8</sup> The data analyzed in the table were made available by the interuniversity consortium for political research. The data were originally collected by the SRC political behavior program. Neither the original collectors of the data nor the consortium bear any

evaluate the data by formulating two statistical models. The first specifies the turnout percentages one should observe under the Downsian expected utility model, namely a pure interaction effect of the following nature:

		Party Difference	
		No	Yes
Closeness	Not Close	$\gamma$	$\gamma$
	Close	$\gamma$	$\gamma + \beta$

That is, if there is no party difference, variations in closeness should produce no increase in the likelihood of voting. Similarly, for those who think that the election is not close, increasing party differences should have no impact.

The second model specifies the turnout percentages one should observe under the minimax regret model, namely, a pure column effect:

		Party Difference	
		No	Yes
Closeness	Not Close	$\gamma$	$\gamma + \beta$
	Close	$\gamma$	$\gamma + \beta$

Increasing party difference should increase the likelihood of voting independently of the perceived closeness of the election.

Unfortunately, our task is complicated by the fact that neither of the preceding models is a special case of the other: they are not "nested." In such cases there is no generally accepted method of constructing a test statistic such that if it exceeds a prespecified bound one model is accepted, and if not, the other is accepted. Consequently, we employ a methodology which we believe to be reasonable, although *ad hoc*. The procedure is to nest both models in a more general model, fit the general model to the data, then test hypotheses to see if we can reject one of the two specific models of interest.

The general model is

		Party Difference	
		No	Yes
Closeness	Not Close	$\gamma$	$\gamma + \beta_2$
	Close	$\gamma$	$\gamma + \beta_1$

In terms of this table the two hypotheses of interest are

$H_0: \beta_2=0$ , acceptance $\Rightarrow$ rejection of minimax regret.

$H_1: \beta_1=\beta_2$ , acceptance $\Rightarrow$ rejection of Downsian expected utility maximization.

If we can reject exactly one of the two hypotheses, we have a reasonable case for deciding in favor of one model rather than the other. Evidently, the problem with this procedure is that we might be unable to reject either model, or we might find grounds for rejecting both. Should either of these possibilities come to pass we can draw no firm conclusion.

Parameter estimation proceeds as follows. The dependent variable is

$$Y = \begin{cases} 1 & \text{if respondent is a voter.} \\ 0 & \text{if respondent is a nonvoter.} \end{cases}$$

And the independent variables are

$$X_1 = \begin{cases} 1 & \text{if respondent has a party differential.} \\ 0 & \text{otherwise.} \end{cases}$$
$$X_2 = \begin{cases} 1 & \text{if respondent perceives the election} \\ & \text{as close} \\ 0 & \text{otherwise.} \end{cases}$$

The general model is

$$Y = \gamma + \beta_1 X_1 X_2 + \beta_2 X_1 (1 - X_2) + \epsilon$$
$$= \gamma + (\beta_1 - \beta_2) X_1 X_2 + \beta_2 X_1 + \epsilon$$
$$= \gamma + \alpha_1 X_1 X_2 + \alpha_2 X_1 + \epsilon, \tag{1}$$

where  $\alpha_1 = (\beta_1 - \beta_2)$   
 $\alpha_2 = \beta_2$ .

Using ordinary least squares, we estimated the parameters of (1) and utilized these estimates to form a consistent estimate of

$$\hat{Y} = \hat{\gamma} + \hat{\alpha}_1 X_1 X_2 + \hat{\alpha}_2 X_1.$$

The latter can be interpreted as the probability that  $Y=1$  conditional upon the observed value of  $X_1$  and  $X_2$ . Because  $Y$  takes on only two values,  $\epsilon$  is distributed as a Bernoulli variable with variance estimated consistently by  $\hat{Y}(1-\hat{Y})$ . These variance estimates were utilized to approximate the variance-covariance matrix of  $\epsilon$ . Then equation (1) was re-estimated. This generalized least-squares procedure provides consistent and unbiased estimates of the coefficients  $\alpha_1$ ,  $\alpha_2$ , and their standard errors.

In Table 1 we present the basic turnout data for the four presidential elections examined. A cursory examination of these tables produces at least two observations. First, one notices immediately that turnout rates among registered voters are very high for all values of the independent variables. Nearly everyone *intends* to vote, and ap-

Table 1. Percentage Voting as a Function of Party Differential (PD) and Closeness of Election

		Pre-Election Intention		Post-Election Report	
		Low PD	High PD	Low PD	High PD
1952	NC	94% (81)	99+ (291)	85 (72)	93 (268)
	C	95 (179)	99 (446)	87 (171)	92 (416)
1956	NC	95 (81)	98 (197)	82 (81)	87 (200)
	C	95 (198)	98 (515)	86 (203)	92 (517)
1960	NC	94 (36)	100 (124)	88 (34)	92 (123)
	C	99+ (216)	99 (749)	94 (207)	95 (711)
1964	NC	95 (146)	99 (369)	86 (140)	92 (353)
	C	98 (146)	98 (326)	87 (138)	92 (326)

proximately 90 per cent of the respondents report *having voted*, although this figure presumably overestimates actual turnout.<sup>12</sup> Second, although the independent variables do not seem to produce much variance in the turnout rate (given the mean, of course, they cannot), there does seem to be some systematic variation in turnout which is related to the party differential but not to closeness of the election.

More exactly, in Table 2 we show the coefficient estimates for equation (1), with standard errors in parentheses. Our test procedure is as follows: for each data set we reject both models if the general model is rejected at the .05 level using a  $\chi^2$  test. If the general model is not rejected, we reject  $H_0$  (i.e., do *not* reject minimax regret) if  $\hat{\alpha}_2 > 0$  at the .05 level; and we reject  $H_1$  (i.e., do *not* reject Downsian expected utility) if  $\hat{\alpha}_1 \neq 0$  at the .05 level.

Of the eight data sets, we fail to reject the general model in seven cases. The general model fails for the 1960 pre-election data. Interestingly, the general model fails in this case because of the presence of a closeness of the election effect in the zero party differential column. The minimax regret model cannot explain this, nor can the Downsian expected utility model in which the

expected party differential equals  $p \cdot 0 = 0$ .

Of the seven cases in which the general model could not be rejected, we found that  $H_0(\hat{\alpha}_2 = 0)$  could be rejected in five cases but  $H_1(\hat{\alpha}_1 = \hat{\alpha}_2)$  could be rejected in only one case.<sup>13</sup> Thus, of seven cases in which the general model is not rejected, the Downsian model of expected party differential survives only once (1956 post reports) while the minimax regret model survives five times (1952 pre- and post-, 1956 pre-, 1964 pre- and post-). Note the perfect minimax regret pattern for the 1956 pre-election data, and the near-perfect patterns for the 1952 pre- and 1964 post-election data. In sum, for these data the minimax regret model has a 5 to 1 advantage over the Downsian expected utility model. The obvious conclusion is that the Downsian model of *expected* party differential turns in a dismal performance as a descriptive turnout model, while the minimax regret model fares considerably better. According to the data, the party differential affects turnout *independently* of expected closeness.<sup>14</sup> While this relationship is inconsistent with the Downsian suggestion, it is consistent with the minimax regret model and with *numerous other models* one might construct in which the party differential affects turnout independently of the probability of affecting the outcome.

A final caveat: in principle one might accept the notion that voters maximize expected utility while one simultaneously rejects the Downsian formulation of an expected utility model. If one is disposed to this view, our analysis could be interpreted as showing that while the Riker-Ordeshook modification of Downs successfully rescues the Downsian model from the prediction that no one votes, still further modifications are required. A natural candidate is a model in which the private benefits of voting (i.e., those like the  $D$  term which are not discounted by  $p$ ) vary with the party differential. (We might note here that Strom's expected utility model is not such a model—in it, party differential terms still are multiplied by  $p$  terms—see Appendix 2.) One

<sup>13</sup> In the remaining case (1960 postelection reports) neither hypothesis can be rejected. Thus although the general model is not rejected for this case, both particular models are.

<sup>14</sup> At least if one considers the whole sample of registered voters. We might note that Fiorina has developed a "hybrid" expected utility model of voting behavior which produces the curious result that a subclass of the citizenry is *less likely* to vote the closer they perceive the election to be. Now, if the world were evenly divided between those whose turnout was encouraged and those whose turnout was discouraged by a high  $p$ , one would expect no aggregate effect. Empirically, however, the unusual subclass to which Fiorina's prediction applies typically appears to be small. See Morris Fiorina, "The Voting Decision: Investment and Consumption Aspects," (California Institute of Technology: Social Science Working Paper No. 46).

<sup>12</sup> Aage Clausen, "Response Validity: Vote Report," *Public Opinion Quarterly*, 32 (Winter, 1968-69), 588-606.

Table 2. Estimates of Equation 1

Data Set	$\hat{\alpha}_1$	$\hat{\alpha}_2$	$\gamma$
1952 pre	-.006 (.006)	.050 (.014)*	.946
1952 post	-.001 (.020)	.069 (.027)*	.864
1956 pre	-.005 (.012)	.030 (.016)*	.950
1956 post	.047 (.027)*	.021 (.032)	.849
1960 pre <sup>n.s.</sup>	-.015 (.009)	.012 (.010)	.988
1960 post	.032 (.026)	-.015 (.029)	.934
1964 pre	-.013 (.010)	.023 (.012)*	.966
1964 post	-.001 (.021)	.055 (.025)*	.863

\*  $p < .05$ .

n.s. general model rejected,  $p > .05$ .

or more theories constructed along these lines would survive our statistical test, for given the data at hand they are indistinguishable from the minimax regret model. In such situations there is only one way to proceed: build the alternative models, then collect the data which will allow discrimination between them and the minimax regret model of the voting decision.

# Conclusions

Our critics have attacked the minimax regret model primarily on the grounds that they do not *feel good* about it, that it is *unreasonable*, that voters just *shouldn't* behave that way. Such attitudes are understandable among statisticians and operations researchers. In political science, however, good feelings in one's tummy are not considered persuasive empirical evidence. As we have tried to demonstrate, the available data provide no empirical basis for rejecting the minimax regret model as a descriptive model of the turnout decision, regardless of its weaknesses as a prescriptive theory of decision making. One cannot be so generous in one's evaluation of the central component of the traditional Downsian model. We would be the first to recognize the extreme crudeness of our data and the necessarily tentative nature of the empirical test. But whatever its flaws, our data analysis is far more substantial than that provided by the critics, whose closest brush with the real world consists of an analysis of freeway driving decisions.

To re-emphasize the main point of our original article, what we tend to accept as rational may blind us both to alternative standards of reasonable behavior and to what is actually happening in the political world. The Downsian turnout model has lived on not because it has any descriptive value, but rather because theorists who accept the prescriptive model underlying it refuse to let it die. In proposing a minimax regret model we are not attempting to erect a new graven image. We

are only attempting to fan the fires of agnosticism.

# Appendix 1: On Tullock

As one would expect, Professor Tullock offers an ingenious criticism. We offer a two-part reply. First, there is the purely technical point that in our article we assumed that the cost of voting for candidates 1, 2, or 3 is a constant. But to cast a write-in vote in an otherwise two-candidate election presumably entails a higher cost than pulling a lever. Hence, the result to which Tullock objects does not generally hold in the write-in context. Second, we would dispute Tullock's basic premise that every citizen is his own first choice for president. Rather, we wager that the citizen who ranks himself as his most preferred candidate is rare. Our mini-poll of Caltech faculty (not noted for poor self-images) produced no cases. Thus, we suggest that Professor Tullock is a deviant case and can safely be ignored for purposes of analysis.

# Appendix 2: On Strom

Strom's piece constitutes a rather desperate attempt to save the Downsian expected utility model. The dénouement of Strom's argument is condition 9:

$$\text{Is } P_{2j}(U_{2j}^v) + P_{3j}(U_{3j}^v) + Q_{3j}(U_{3j}^a) + Q_{4j}(U_{4j}^a) > C + P_{4j}(U_{4j}^v) + Q_{2j}(U_{2j}^a)?$$

Now, Strom would have us believe that the right-hand side tends to  $C$  while the left-hand side adds to some significantly positive quantity. Nonsense. We cheerfully admit that the right-hand side tends to  $C$ , but just as cheerfully contend that the left-hand side tends to 0. Strom can't have his cake and eat it too. He must demonstrate empirically that the utility terms on the left-hand side are so large that even when discounted by  $p$ -terms, the resulting sum of products is non-trivial. We await such demonstration.

## The Ethical Voter

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No rational egoist should bother voting because, as Skinner's Dr. Frazier notices, the probability of any one man casting the decisive ballot "is less than the chance he will be killed on the way to the polls."<sup>1</sup> No matter how deeply he cares about the electoral outcome, a man must realize that his vote is only one among very many. The larger the electorate, the smaller the probability of any one vote's changing the outcome, so in most modern polities the politically rational thing to do is to conserve on shoe leather.

Real-world voters, of course, do flock to the polls, which is usually explained in terms of a feeling of "civic duty." The fact that men get some satisfaction from discharging their civic duty by voting might answer the question of why it is rational for them to go to the polls. Unfortunately, it leaves another—how do they vote once they get there? Presumably, they go ahead and vote according to the dictates of their egoistic interests.<sup>2</sup>

Ferejohn and Fiorina make a valuable contribution by initiating the search for an alternative motivational basis for the phenomenon of voting.<sup>3</sup> However, their own solution, "minimax regret" (choose the alternative having the least unbearable worst possible outcome), is seriously flawed. As Ferejohn and Fiorina argue, voting must be either an investment or a consumption decision if it is to be rational for Economic Man. But if it is an investment decision one would expect a rational actor to use the same decision rule when voting as when taking other investment decisions such as portfolio choice; and from the

study of these other decisions it is clear that investors do not aim at minimax regret. Indeed, there is no rational reason they should. Ferejohn and Fiorina rely on the argument, "My God, what if I didn't vote and my preferred candidate lost by one vote? I'd feel like killing myself." But before putting in the knife, the non-voter should recall that "what if" arguments are decisively refuted by "seemed like a good idea at the time" rejoinders. A multitude of decisions which one comes to regret *ex post facto* were *ex ante* the only reasonable courses of action. Most of us wish we had bought Xerox stock when it was first issued; J. P. Morgan always regretted rejecting young Henry Ford's request for him to finance the original Model T venture; etc. What are clearly mistakes with the benefit of hindsight were nevertheless good moves at the time the decision had to be taken. Everyone regrets that he is not prescient, that is all. These regrets are unaltered by minimaximizing.

The remaining possibility is that voting is a consumption decision. Of course, consumer choice is an important topic in economic theory. While emphasizing the fact, however, Ferejohn and Fiorina overlook important differences in the focus of consumer and investment theory. The economist can say why an investor likes his portfolio (it is instrumental; it will pay off in terms of other goods) but he cannot say why a consumer fancies his purchases. The theory of consumer choice predicts how Economic Man will trade off one good for another under various market and budgetary conditions, and it does so by taking as given the preferences of consumers. In the voting application, consumer theory might help to explain why people fail to vote when the price is high (e.g., on a rainy day), but there must be a prior assumption that people think voting is a good, i.e., that they prefer voting to nonvoting *ceteris paribus*. This leaves the question of why they like to vote, which presumably takes us back to civic duty or irrationality.

The main thrust of this comment is meant to be constructive rather than destructive. It follows in the spirit if not in the footsteps of Ferejohn and Fiorina. Essentially, the purpose is to suggest another reason that voters should go to the polls and, more significantly, another analysis of what they do once they get there. The argument has

<sup>1</sup> B. F. Skinner, *Walden II* (New York: Macmillan, 1948), p. 265. See also G. F. W. Hegel, *The Philosophy of Right*, trans. T. M. Knox (Chicago: Encyclopedia Britannica, 1952), p. 104; Brian Barry, *Political Argument* (London: Routledge & Kegan Paul, 1965), pp. 328–30 and *Sociologists, Economists and Democracy* (London: Collier-Macmillan, 1970), pp. 19–23; and Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: University of Michigan Press, 1967), chap. VII.

<sup>2</sup> Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), chap. 14. Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963).

<sup>3</sup> John A. Ferejohn and Morris P. Fiorina, "The Paradox of Not Voting: A Decision Theoretic Analysis," *American Political Science Review*, 68 (June, 1974), 525–36.

implications both for party strategies and for the interpretation of electoral results.

The keystone of the analysis is the assumption that individuals have both egoistic and ethical preferences, that their welfare functions have both egoistic and ethical components. Nothing very precise need be said about the relative strengths of these motives. Ordinarily egoistic preferences ride roughshod over ethical ones, but no matter. All that is required for present purposes is that ethical preferences carry *some* weight, that they will be decisive at least where everything else is equal. Clearly, such a lexicographical relationship seriously underestimates the strength of ethical preferences. They are not mere tie-breakers. Rather, strong ethical preferences can often override weak egoistic ones. The assumption which this argument requires, then, is a very weak one.

Such a description of human nature is reminiscent of the theories of David Hume, who holds that an individual is capable of displaying benevolence, at least when his own interests are not at stake. The ethical theories of Hume's friend Adam Smith are identical on this point as evidenced by his discussion of the "impartial spectator." More recently John Harsanyi has worked with similar assumptions.<sup>4</sup>

An important distinction between egoistic and ethical preferences is that the former are strictly effect-oriented, whereas the latter applaud trying as well as succeeding. The vote of a single man does no more to enact his ethical preferences than it would to enact his egoistic preferences, but the demonstration that his motives are pure and his heart is in the right place counts for something ethically speaking. Thus, one reason men go to the polls might be to take a moral stand. Not very much depends on this alternative explanation of why men vote, however. The balance of the argument is equally valid if men go to the polls purely out of a sense of civic duty. There is certainly nothing inconsistent in arguing that a deontological tug gets citizens into the poll booth and that ethical preferences then take over.

Precisely the same factor that makes it irrational for an egoist to go to the polls makes it rational for a less than thoroughly self-centered man to vote his ethical preferences once there. As one in a multitude, his vote cannot much affect the outcome. Since there is very little he can do inside the poll booth to further his egoistic interests, a

man is freed to vote his ethical preferences. The larger the electorate, the more pronounced this tendency. When the curtains of the poll booth are drawn, then, a voter assumes the air of Adam Smith's impartial spectator. It should also be noted that this is an extraordinarily cheap way for a man to assuage his conscience. Casting a ballot for the forces of The Good is far safer than casting a stone.

Put into a more general form, this analysis suggests that political man behaves ethically whenever the losses he expects from so doing are low enough to be outweighed by the rewards. This implies at least two dimensions—one, the stakes in the decision (the cost if it goes against him or the reward if he wins); and the other, his chances of altering the outcome. A man is safe in behaving

		Stake	
		high	low
Efficacy	high	I egoistic	II ethical
	low	III ethical	IV ethical

Figure 1. Sources of Ethical Voting Behavior

ethically when there is not much in it for him (cells II and IV in Figure 1). When he has much at stake he certainly longs to tip the outcome in his favor, and he will try to do so if he has the power (cell I) but lacking the power he simply sighs while choosing the ethical course of action (cell III). In this form the theory is amenable to empirical examination. Provisional hypotheses would be that, controlling for his stake in the project, the stronger the respondent's sense of political efficacy, the less likely he is to opt for the policy he perceives to be the fairest; and, controlling for efficacy, the higher the respondent's stake in the program, the less likely he is to favor the fairest policy. A further refinement might involve controlling for the ethical significance of the issue; some are central to a man's sense of fairness, whereas other issues might be very peripheral. Even in testing the hypothesis as stated, two additional considerations must be kept in mind. The logic of the first hypothesis obviously implies a curvilinear correlation. The difference between a one-in-a-million and a one-in-ten-million chance of altering the outcome is clearly much less significant for the rational voter than the difference between a one-in-a-hundred and a one-in-a-thousand chance, although the difference in each case amounts to a factor of ten. And, when costs are allowed to vary, as in the second hypothesis, an individual's attitude toward risk-taking be-

<sup>4</sup> David Hume, *A Treatise of Human Nature* (London: John Noon, 1739), especially Book III, and *An Enquiry Concerning the Principles of Morals* (London: T. Cadell, 1777). Adam Smith, *The Theory of Moral Sentiments*, 6th ed. (London: A. Strahan and T. Cadell, 1790). John C. Harsanyi, "Cardinal Welfare, Individualistic Ethics and Interpersonal Comparisons of Utility," *Journal of Political Economy*, 63 (August, 1955), 309-21.

comes important. The hypothesis as stated assumes that his taste for risks is a constant, but it is easy enough to build into the hypothesis any other theory about how it might vary.

Two politically significant conclusions follow from this recognition of the importance of the ethical component in electoral choice. First, political parties stand to gain more popular support through a moralizing crusade (provided, of course, that they choose popular causes) than by focusing debates on questions of who gets what when, and how. Second, electoral results should be interpreted as reflecting essentially ethical preferences insofar as partisans have structured the debate in such a way as to allow these to emerge. Both these conclusions, it should be emphasized, are predicated on the explicit assumption about human nature and on the implicit assumption that voters pursue their goals as rational means-ends calculators.

These principles are rather more familiar to politicians than to political scientists. Lyndon Johnson's Great Society, George McGovern's schemes for income redistribution and Harold Wilson's Social Contract are all appeals for mandates from those who are comfortably placed in society for the commitment of social resources to help those in desperate straits; and two of the three succeeded in persuading enough voters to forsake self-interest to form a government. Something approaching recognition of these principles is found in the Banfield and Wilson "ethos theory," although one hastens to add that both public- and private-regardingness can qualify as ethical behavior. What matters is not the substance of the rule of conduct but rather how it is justified, and so long as private-regarding citizens argue in terms of universalizable prescriptions (e.g., the Puritan Ethic), their selfishness must be considered an ethical principle.<sup>5</sup>

<sup>5</sup> Edward C. Banfield and James Q. Wilson, "Public Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 63 (December, 1964), 876-87. Another example might be John H. Fenton, *The Catholic Vote* (New Orleans: Hauser Press, 1960); religious-group voting surely has both ethical and reference group aspects. R. M. Hare,

The argument of this comment is best summarized as Niebuhr's slogan set on its head: immoral men, moral society.<sup>6</sup> This, however, only refers to the inclinations of actors. While individuals are more tempted to behave selfishly in their personal dealings, formal legal restraints or informal social ones may deny them the opportunity. In their public roles, men can vote their ethical preferences only if these alternatives are listed on the ballot.

This argument is not to deny Niebuhr's observation that modern mass society has some unsavory characteristics, among the foremost of which is intolerance. The present analysis suggests that intolerance does not proceed from a lack of idealism but rather from an excess of it. In *Freedom and Reason* Hare shows that the moral fanatic is just one special variety of moralist—one which is repugnant from most moral perspectives, admittedly, but a moralist nonetheless.<sup>7</sup> Most men, while far from being fanatics, are less flexible on ethical issues than they would be on material demands. This is surely because their ethical preferences are more intensely held. Hence, even in a well-integrated society in which men are very close together on ethical matters, such differences as remain might matter far more to them than those arising out of egoistic desires. The judgment must be a discouraging one: the fact that ethical preferences are reflected in electoral results is not necessarily a cause for celebration. One cannot help wishing that Hitler had forsaken ethical ideals and pursued narrow material gain instead.

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*The Language of Morals* (Oxford: Clarendon Press, 1952) argues that the essential characteristic of moral statements is that they are universalizable prescriptions.

<sup>6</sup> Reinhold Niebuhr, *Moral Man and Immoral Society, A Study in Ethics and Politics* (New York: Scribner's, 1932), pp. xi-xii, writes, "In every human group there is less reason to guide and check impulse, less capacity for self-transcendence, less ability to comprehend the needs of others and therefore more unrestrained egoism than the individuals, who compose the group, reveal in their personal relationships."

<sup>7</sup> R. M. Hare, *Freedom and Reason* (Oxford: Clarendon Press, 1963).



# The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities\*

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## Introduction

Vote trading, or logrolling, is a concept familiar to students of legislative bodies. Such students of the formal properties of voting bodies—James M. Buchanan, Gordon Tullock and James S. Coleman, in particular—have extolled the strategy of exchanging votes in a legislative arena as a means by which individual members are able to maximize their total utility.<sup>1</sup> Their scenario is straightforward: a legislator who does not feel very strongly about an issue he would oppose should be willing to trade votes with another member in order to secure the passage of a motion he strongly favors, but which would otherwise be defeated. Exchanging of votes thus seems analogous to exchanging of commodities for money in a market economy. William H. Riker and Steven J. Brams, however, have demonstrated that, for a legislature of three or more members and at least six motions, some trades which appear to be individually rational do not maximize the total utility of the traders.<sup>2</sup> Instead, there is a “paradox

of vote trading” in which the incentive for every pair of legislators to trade is present, but the effects of externalities (costs imposed on the non-trader for each pair of motions) reduces the total utility to each player from what he could expect if no trades were made.

Riker and Brams argue that the advocates of vote exchanges have failed to consider the effects of externalities.<sup>3</sup> An externality is simply any cost which is imposed on an individual as a result of actions not under his control. In the case of the paradox of vote trading, such externalities considered by Riker and Brams are always “external costs”—i.e., the trading behavior of two members of a three-person legislature always adversely affects the nontrader.<sup>4</sup> This is a feature of the system of majority voting, as we shall demonstrate below. Indeed, externalities may involve potential benefits to the nonparticipant. We shall present an extension of the paradox of vote trading to unanimity decision rules below in which externalities are indeed positive. Actually, the extension of the Riker-Brams paradox to unanimity rules is part of a broader theoretical question which we shall consider: under what conditions is a more stringent decision rule or a change in voting strategies Pareto optimal? Accordingly, after a consideration of the Riker-Brams argument, we shall extend their analysis and attempt a resolu-

\* We are grateful for the comments of Steven J. Brams, William H. Riker, David H. Koehler, and two anonymous referees for the Review.

<sup>1</sup> See James M. Buchanan and Gordon Tullock, *The Calculus of Consent: Logical Foundations of Constitutional Democracy* (Ann Arbor: University of Michigan Press, 1962), chaps. 7–12; Tullock, “A Simple Algebraic Logrolling Model,” *American Economic Review*, 60 (June, 1970), 419–426; James S. Coleman, “The Possibility of a Social Welfare Function,” *American Economic Review*, 56 (December, 1966), 1105–1122; Coleman, “The Possibility of a Social Welfare Function: Reply,” *American Economic Review*, 57 (December, 1967), pp. 1311–1317; Edwin T. Haeefe, “Coalitions, Minority Representation, and Vote-Trading Probabilities,” *Public Choice*, 8 (Spring, 1970), 75–90; and Dennis C. Mueller, Geoffrey C. Philpotts, and Jaroslav Vanek, “The Social Gains from Exchanging Votes: A Simulation Approach,” *Public Choice*, 13 (Fall, 1972), 55–80.

<sup>2</sup> Riker and Brams, “The Paradox of Vote Trading,” *American Political Science Review*, 67 (December, 1973), 1235–1247. Cf. R. E. Park, “The Possibility of a Social Welfare Function: Comment,” *American Economic Review*, 57 (December, 1967), 1300–1304; Mueller, “The Possibility of a Social Welfare Function; Comment,” *Ibid.*, pp. 1304–1311; Robert Wilson, “An Axiomatic Model of Logrolling,” *American Economic Review*, 59 (June, 1969), pp. 331–341; David H. Koehler, “Vote-Trading and the Voting

Paradox: A Proof of Equivalence,” *American Political Science Review*, 69 (1975); and, for a view which tends to support Riker and Brams but does not rule out the positions of Buchanan, Tullock and Coleman, see Peter Bernholz, “Logrolling, Arrow Paradox and Cyclical Majorities,” *Public Choice*, 15 (Summer, 1973), 87–95 (but especially p. 94). But cf. the exchange among Tullock, Bernholz, and Riker and Brams in “Communications,” *American Political Science Review*, 68 (December, 1974), 1688–1692. Also see Joe A. Oppenheimer, “Relating Coalitions of Minorities to the Voters’ Paradox, or Putting the Fly in the Democratic Pie,” University of Texas mimeo, 1973. An earlier informal statement of the possible negative effect of vote trading is found in William A. Niskanen, Jr., *Bureaucracy and Representative Government* (Chicago: Aldine, 1971), p. 143.

<sup>3</sup> Riker and Brams, “The Paradox of Vote Trading,” p. 1240.

<sup>4</sup> *Ibid.*, p. 1242.

Table 1. Utilities of Members for Outcomes of Voting

	Preferred Outcome	Motion $x$ $u_i(X)$	$u_i(\bar{X})$	Preferred Outcome	Motion $y$ $u_i(Y)$	$u_i(\bar{Y})$
	Col 1	Col 2	Col 3	Col 4	Col 5	Col 6
*Member 1	$X$	1	-2	$Y$	1	-2
Member 2	$X$	1	-1	$\bar{Y}$	-2	2
Member 3	$\bar{X}$	-2	2	$Y$	1	-1
		Motion $w$ $u_i(W)$	$u_i(\bar{W})$		Motion $z$ $u_i(Z)$	$u_i(\bar{Z})$
Member 1	$W$	1	-1	$\bar{Z}$	-2	2
Member 2	$\bar{W}$	-2	2	$Z$	1	-1
*Member 3	$W$	1	-2	$Z$	1	-2
		Motion $t$ $u_i(T)$	$u_i(\bar{T})$		Motion $v$ $u_i(V)$	$u_i(\bar{V})$
Member 1	$\bar{T}$	-2	2	$V$	1	-1
*Member 2	$T$	1	-2	$V$	1	-2
Member 3	$T$	1	-1	$\bar{V}$	-2	2

\* Nontrader on pair of motions.

tion on the supposed effects of vote trading between the Buchanan-Tullock-Coleman approach and that of Riker and Brams.

The paradox of vote trading under a majority decision rule is based upon a simple set of criteria for "naive" or "sincere" voting (always revealing one's actual preferences) and "sophisticated" voting (concealing actual preferences in a way which nevertheless will yield a net gain in expected utility).<sup>5</sup> The Riker-Brams model stipulates two conditions for "switching" (or, sophisticated voting): (1) a switcher must be in the decisive set for a given motion; and (2) he must also be a pivotal member of such a set—in the sense that his change in votes would be sufficient to make a formerly losing coalition a winning one.<sup>6</sup> The second condition contains the further premise that there is some positive utility in trading of votes, so that an incentive to employ sophisticated rather than sincere voting does exist. Vote trading is simply "a barter system of switching" in which each legislator who agrees to trade his vote on one motion receives the vote of another member in return.<sup>7</sup> What Riker and Brams show, then, is that in an  $n$ -member legislature voting on a set of motions, trades which appear to be individually rational among each pair of voters are not collectively rational (i.e., Pareto optimal) because of the effects of externalities on the nontraders. An

example drawn from the Riker-Brams article will demonstrate the paradox.

In Table 1, the utilities for each legislator on each motion which Riker and Brams stipulate are reproduced.<sup>8</sup> In the table we have six motions:  $x, y, w, z, t$ , and  $v$  and twelve possible outcomes:  $X, \bar{X}, Y, \bar{Y}, W, \bar{W}, Z, \bar{Z}, T, \bar{T}, V$ , and  $\bar{V}$ , where  $u_i(X)$  is the utility to voter  $i$  of the passage of motion  $x$  and  $u_i(\bar{X})$  is the utility to  $i$  for the defeat of the motion. Utility is defined over outcomes, since the motions are merely means to the end of maximizing total utility for each individual. It is clear from the table that under simple majority rule without vote trading, the net utilities over the six motions are zero for each player.<sup>9</sup> While all six motions pass, variations in the intensity of preferences affect the net utility such that no one is any better or worse off than had no votes been cast at all. If each legislator ignores the actions of others on votes on which he himself is not in a position to trade, logrolling appears to be a rational strategy. The net expected utility of the six votes is +4 for each member, allowing each to make the best possible trades for himself.<sup>10</sup> If, on the other hand, Member 1 resigns himself to his "security level"—his evaluation of what happens to him if he refrains from trading but others do not (what he can guarantee himself regardless of the actions of other players)—his net utility would be -6.<sup>11</sup> In the former case, the member considers only the possible gains from vote trading, and in the latter case, only what happens to

<sup>5</sup> On the distinction between "sincere" and "sophisticated" voting, see Robin Farquharson, *Theory of Voting* (New Haven: Yale University Press, 1969), pp. 18 and 39-40.

<sup>6</sup> Riker and Brams, "The Paradox of Vote Trading," pp. 1237-1238.

<sup>7</sup> *Ibid.*, p. 1238.

<sup>8</sup> This is Table 5, p. 1241.

<sup>9</sup> *Ibid.*, Table 6.

<sup>10</sup> *Ibid.*, Table 7.

<sup>11</sup> *Ibid.*, Table 9.

Table 2. Utilities of Outcomes Without Trades Under a Unanimity Rule

Members	Outcomes						$\Sigma u_i$
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	-2	-2	-1	2	2	-1	-2
2	-1	2	2	-1	-2	-2	-2
3	2	-1	-2	-2	-1	2	-2

him if he refuses to trade while the other players do trade. An evaluation of the potential gains from trading which excludes external costs convinces each member that all individual trades are rational. Thus, there is an incentive for every possible trade to occur. As a result of the log-rolling, all six motions in Table 1 fail, yielding a net utility to each player of -2. This is the "paradox of vote trading." Each member fails to consider the external costs on the motions on which he does not trade and accepts any trade which appears to be individually rational. Rational trading thus yields a worse net situation than the failure to trade. Since no single member can be guaranteed that the others would agree not to trade if he refrains from doing so, each is put in a "kind of  $n$ -person Prisoners' Dilemma in which each member is forced by individual rationality to choose the socially and individually destructive alternative" and "... each member comes to a worse end than if he ... always voted sincerely or naively."<sup>12</sup>

Riker and Brams have indeed revealed a paradox of rationality—one which they claim is "inescapable."<sup>13</sup> Advocates of the strategy of log-rolling to maximize total utility—including Buchanan, Tullock, and Coleman—thus seem to be faced with a direct challenge to their position. In the next section of this paper, we shall extend the work of Riker and Brams to unanimity voting rules, providing further support for their finding of a paradox of vote trading.

#### Unanimity Decision Rules and the Paradox

The occurrence of the paradox of vote trading under simple majority rule is not sufficient to warrant the statement of Riker and Brams that the paradox is inescapable. As Buchanan and Tullock have argued, simple majority rule is not well suited to handle the problems posed by varying intensities of preference. An examination of Table 1 clearly reveals that the members of the legislative body are not equally affected by the outcomes on each motion. We assume that utilities are measured cardinally for each individual.<sup>14</sup>

In this sense, our approach differs from that of Riker and Brams, who argue that only ordinal measurement of utilities is required to establish the paradox.<sup>15</sup> We shall justify this approach in the next section. For the present purposes, we need to note only that for each member, the net difference in utility which would result from the passage or failure of any given motion is maximized for those motions on which the member is in the minority position. Under such conditions of varying intensities of preference, Buchanan and Tullock argue the case for a system of qualified majority rule and go so far as to suggest that the optimal decision rule might be one of unanimity, allowing for vote trading.<sup>16</sup> On motion  $x$  for example, Members 1 and 2 each stand to gain one unit of utility if the motion passes, while Member 3 loses two units of utility with the same result. We might suspect that a more stringent voting rule would increase the net utility accruing to the three players more than would simple majority rule, either with or without vote trading.

The arguments of Buchanan and Tullock are, however, just as susceptible to the paradox of vote trading as is simple majority rule, although for quite different reasons. Under a unanimity rule without logrolling, the net utility over the six motions in Table 1 to each member is -2, with all motions failing. This result (see Table 2 below) is no better than majority rule with vote trading and inferior to majority rule without trading. If vote trading is allowed under unanimity, the net utility to each player is once again zero. (See Table 3). As in the case of simple majority rule, all six motions pass. Therefore, a unanimity rule allowing for vote trading is no better than simple majority rule with a prohibition on logrolling.<sup>17</sup> Furthermore, an examination of the effects of the alternative strategies of naive and sophisticated voting on the unanimity rule and utility values in Table 1 indicates that we have another occurrence of the paradox of vote trading. In the Riker-Brams example for majority rule, the individual trades were rational, but the aggregation of the

<sup>12</sup> *Ibid.*, pp. 1244 and 1242.

<sup>13</sup> *Ibid.*, p. 1242.

<sup>14</sup> This amounts to accepting interpersonal comparisons of utilities. See Jerome Rothenberg, *The Measurement of Social Welfare* (Englewood Cliffs, N.J.: Prentice-Hall, 1961), pp. 137-143.

<sup>15</sup> Riker and Brams, "The Paradox of Vote Trading," pp. 1242 ff.

<sup>16</sup> Buchanan and Tullock, *The Calculus of Consent*, pp. 85ff. and pp. 276-280.

<sup>17</sup> For a contrary point of view, see J. Roland Pennock, "The Pork Barrel and Majority Rule," *Journal of Politics*, 32 (August, 1970), 709-716.

Table 3. Utilities of Outcomes with Trades and with External Costs Under a Unanimity Rule

Members	Outcomes						$\Sigma u_i$
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	1	1	1	-2	-2	1	0
2	1	-2	-2	1	1	1	0
3	-2	1	1	1	1	-2	0

Table 4. Utilities of Outcomes with Trades When Possible and Without Anticipating External Costs Under a Unanimity Rule

Members	Outcomes						$\Sigma u_i$
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	-2	-2	1	-2	-2	1	-6
2	1	-2	-2	1	-2	-2	-6
3	-2	1	-2	-2	1	-2	-6

trading outcomes produced a sub-optimal societal result. For a unanimity rule, the trading of votes would produce a Pareto optimal societal (and, of course, individual) result, but, as we shall see, the individual trades are not themselves rational.

In the Riker-Brams example, the utility of the outcome to each legislator who trades but does not consider external costs is +4. For a unanimity decision rule, however, the failure to consider the impact of the trading behavior of others yields a utility of -6 to each member across the six motions (see Table 4). Thus, if each legislator considered only those trades in which he takes an active part, he would rationally refuse to trade. Consider the case of a potential exchange between members 2 and 3 on motions  $x$  and  $y$ . The only feasible trade would be for member 2 to give up his decisive opposition to motion  $y$  in return for member 3's pivotal support on  $x$ . In each case, however, the result would be a loss in utility. Voting sincerely across the two motions, each member would receive +1 units of utility: -1 on the defeated issue he supports and +2 on the defeated issue he opposes. An exchange of votes would yield each a net utility of -1: -2 on the

passed motion he opposes and only +1 on the passed motion which he supports. It would appear, then, that there are no reasons why members should trade votes at all.

Such an appearance, however, is misleading because of the altered effects of externalities. Under a more exclusive voting rule, as Buchanan and Tullock correctly argue, the costs imposed by externalities indeed will be reduced.<sup>18</sup> In the case of the current voting situation, logrolling will produce external benefits rather than costs for the traders. If each legislator were to refuse to trade and attempt to "await" the benefits accruing to him from the trading behavior of the other two legislators, he would receive (if such a situation were actually to occur) +8 units of utility—in comparison with the Riker-Brams security level of -6 for majority rule (see Table 5). While such trades will not take place—since they are individually irrational—the analogy with the Riker-Brams security level is instructive since it demonstrates that logrolling under a unanimity rule has a very different effect upon externalities than does vote trading under majority rule. This ideal situa-

<sup>18</sup> Buchanan and Tullock, *The Calculus of Consent*, pp. 63-68.

Table 5. Utilities of Outcomes When No Trades Occur But Each Member Anticipates External Costs Under a Unanimity Rule

Members	Outcomes						$\Sigma u_i$
	$u_i(X)$	$u_i(Y)$	$u_i(W)$	$u_i(Z)$	$u_i(T)$	$u_i(V)$	
1	1	1	1	2	2	1	8
2	1	2	2	1	1	1	8
3	2	1	1	1	1	2	8

Table 6. The Paradox of Vote Trading as a Three-Person Prisoners' Dilemma

Member 3		Member 2	
		Alternative 1 Not Trade	Alternative 2 Trade
Member 1	Alternative 1 Not Trade	-2, -2, -2	-2, -2, -2
	Alternative 2 Trade	-2, -2, -2	-4, -4, 8

Member 3		Member 2	
		Alternative 1 Not Trade	Alternative 2 Trade
Member 1	Alternative 1 Not Trade	-2, -2, -2	8, -4, -4
	Alternative 2 Trade	-4, 8, -4	0, 0, 0

Note: This is an adaptation of Table 10 in Riker and Brams, "The Paradox of Vote Trading," p. 1243. In cell (1, 1, 1), the entry is "-2, -2, -2" because no one trades. In cells (1, 2, 1), (2, 1, 1) and (1, 1, 2), the entries are also "-2, -2, -2" because, if any pair consistently votes sincerely, then all three must do so. In cell (2, 2, 2), the entry is "0, 0, 0" because all players trade. In cells (2, 2, 1), (1, 2, 2) and (2, 1, 2), the entries are appropriate combinations of -4 units for the traders and +8 units for the nontraders. The +8 units are calculated as in Table 5; the -4 units result from receiving, first the payoff to sincere voting (a) when one is not a trader (-2-2) and (b) when one's potential trade is with a sincere voter (1-2); and second, the payoff to sophisticated voting when one's partner is a sophisticated voter (2-1). If trading is viewed as "cooperative" behavior and not trading as "defecting," then the desired outcome in a Prisoners' Dilemma game—total cooperation—is represented in cell (2, 2, 2); this strategy is preferred by all players to complete defection—as represented in cell (1, 1, 1)—thus producing the dilemma.

tion which each player would like to obtain is thus simply the sum of all of the positive utilities for the six motions for each legislator: each member can guarantee himself a utility of +2 on those motions he opposes by refusing to trade; the trading patterns on the motions he favors guarantee that an outcome favorable to him will occur.

We thus have the reverse of the kind of  $n$ -person Prisoners' Dilemma found by Riker and Brams: each player would be better off if he could refuse to trade himself but would benefit from the external effects of the trades of the other players. Each member has an incentive to be a free rider on the others. By his single negative vote on a given motion he opposes, he can guarantee himself a utility of +2, while no trades will be consummated which will reject the motions he favors. Since, by construction, the nontrading member in Table 1 favors the motion on which he does not trade, he cannot incur any losses on such votes. Yet, if Members 1 and 3 were to realize that Member 2 did not intend to cooperate on other motions, they would rationally refuse to trade at all. The resulting paradox is that no players would ever trade, leaving each member worse off than

he would be if the (individually irrational) trades were consummated. Table 6 presents the Prisoners' Dilemma for a unanimity rule.<sup>19</sup> The paradox does indeed seem "inescapable" and the entire situation is symmetric. The net utility to each legislator under simple majority rule without vote trading is equal to the net utility under a unanimity rule with vote trading; while the net utility obtained under both majority rule with vote trading and unanimity without logrolling is identical. These findings make more explicit a statement by E. E. Schattschneider about the bargaining system in American politics: "... if everybody got into

<sup>19</sup> Riker and Brams (see the note to Table 10 on p. 1243 of "The Paradox of Vote Trading") argue that their result is "strictly a Prisoners' Dilemma. . . ." If, however, we consider the general discussion of the dilemma, the preferred strategy of both players is cooperating (i.e., trading), but such a strategy is dominated by that of defecting (not trading). This is the case in our example; for a discussion of the dilemma, see R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957), pp. 88-113. The Riker-Brams example is indeed a Prisoners' Dilemma, but not the classical type.

the act, the unique advantage of this form of organization would be destroyed. . . ."<sup>20</sup>

As in any Prisoners' Dilemma game, the assumption of Riker and Brams that "motions are made and voted upon serially, so that future motions are not necessarily anticipated when current motions are considered"<sup>21</sup> does provide the potential for learning behavior over time. In repeated legislative sessions, members may begin to trust each other and pursue a strategy of cooperation rather than defection under a unanimity rule (as opposed to cooperation under majority rule) yielding the best possible outcome.<sup>22</sup> Yet the logical properties of the paradox remain intact and there is nothing to prevent a greedy member from attempting to maximize his own utility at

the expense of the others, resulting again in a Prisoners' Dilemma and an occurrence of the paradox of vote trading. Thus, we have shown that the paradox is quite general, but that its effects are not neutral with respect to decision rules. Who bears the external costs depends upon who has the power to inflict them—the majority (in the case of majority rule) or the minority (under a unanimity rule).<sup>23</sup> On a more general level, however, we ask: Are the positions of Riker and Brams, on the one hand, and Buchanan, Tullock, and particularly Coleman on the other hand, necessarily in total conflict? It is to this question—and its implications for the relationship between vote trading and decision rules—that we now turn.

### Paradox Gained and Paradox Lost

The initial discussion of the paradox by Riker and Brams suggests that they have uncovered an anomaly in the structure of voting procedures: the initial assumption is that vote trading should produce Pareto optimal results, but the paradox is established when the condition of vote trading is not met. Riker and Brams argue, however:

. . . the paradox of vote trading is closely related to the paradox of voting. . . . [In the above example] no trade is preferred to all other trades, which is equivalent to our earlier demonstration that no coalition can defeat all others. Since there is no preferred set of trades on which to base a stable coalition, no stable coalition is possible. The paradox of vote trading cannot, therefore, be simply solved by waving it away with a coalition. Rather it is inherent in the nature of the legislative process and, given an appropriate distribution of tastes and external costs, cannot be avoided.<sup>24</sup>

Similarly, David H. Koehler has presented a proof that a cyclical majority will occur if and only if

<sup>20</sup> E. E. Schattschneider, *The Semisovereign People* (New York: Holt, Rinehart and Winston, 1960), p. 35. The paradox—at least as developed so far—follows rather directly from the fact that each voter can be said to be in either a winning coalition (for each motion or for the entire set of motions) or a losing coalition. Thus, the set of motions,  $J$ , constitutes a simple game: formally, "[a] game,  $v$ , in  $(0, 1)$  normalization is said to be simple if, for each  $S \subset N$ , we have either  $v(S) = 0$  or  $v(S) = 1$ " (Guillermo Owen, *Game Theory* [Philadelphia: W. B. Saunders, 1968], p. 163), where  $N$  is the set of players in the game,  $S$  is a coalition, and  $v(S)$  is the value of the coalition. Owen notes (*ibid.*) that "simple games . . . include voting 'games' in elections and legislatures"—precisely the situation we have. The result that at least one member of the total number of players must be excluded from some coalition follows from an extension of L. S. Shapley's comment that "[s]olutions that exclude one or more players . . . are found . . . in almost all simple games, and they probably exist in all constant-sum games." See Shapley, "On Solutions that Exclude One or More Players," in *Essays in Mathematical Economics in Honor of Oskar Morgenstern*, ed. Martin Shubik (Princeton: Princeton University Press, 1967), p. 57.

<sup>21</sup> Riker and Brams, "The Paradox of Vote Trading," p. 1236.

<sup>22</sup> Cf. Anatol Rapoport and Albert M. Chammah, *Prisoners' Dilemma* (Ann Arbor: University of Michigan Press, 1965), chap. 5; and Lester B. Lave, "An Empirical Approach to the Prisoner's Dilemma Game," *Quarterly Journal of Economics*, 76 (August, 1962), pp. 424–436. Nigel Howard has proposed an approach to the Prisoners' Dilemma in which cooperative strategies would be rational. In his "metagames," players choose their strategies dependent upon those of the other players. See his *Paradoxes of Rationality: Theory of Metagames and Political Behavior* (Cambridge: MIT Press, 1971), pp. 44–48 and 55–60. Note the strong criticism of this approach by John C. Harsanyi, in his review of Howard's book in the *American Political Science Review*, 67 (June 1973), 599–600. In experiments with repetitive play, individuals have tended to behave cooperatively. Rapoport and Chammah as well as Lave found that the proportion of cooperative responses approach seventy per cent. For an interesting argument that the Prisoners' Dilemma is not a genuine dilemma or even a paradox, see R. L. Cunningham, "Ethics and Game Theory: The Prisoners' Dilemma," *Papers on Non-Market Decision Making*, 2 (1967) pp. 11–26.

<sup>23</sup> The logical nexus between unanimity and majority voting rules vanishes once the deterministic nature of the argument is relaxed. The probability that an individual will agree to trade votes with one or more others is dramatically affected by the fact that, under the former decision rule, a single negative vote will suffice to defeat a motion. Under majority rule, however, the probability that a member will be able to affect the outcome of a collective decision is substantially reduced—and, indeed, is a function of the size of the voting body. Another factor which is also a function of the size of the voting body—and which seems to us to be more serious for a unanimity than a majority rule—is "transaction costs," associated with the necessity of reaching a collective decision. Our discussion of pairwise vs.  $n$ -wise trading of votes below bears on this problem. We are grateful to T. Nicolaus Tideman and James M. Buchanan for calling this general problem to our attention, although we emphasize that our approach is not always consistent with theirs (in private communications). Cf. also note 34 below.

<sup>24</sup> Riker and Brams, "The Paradox of Vote Trading," pp. 1245–1246.

the conditions for vote trading established by Riker and Brams and discussed above are met.<sup>25</sup> Note, however, the statement by Riker and Brams that "an appropriate distribution of tastes and external costs" must occur. Perhaps a reconciliation of the position espoused by Buchanan, Tullock, and Coleman and that of Riker and Brams and Koehler can be achieved. A critical first step in this process is to realize that the Riker-Brams discussion of the paradox of vote trading when preferences are ordinarily ranked is itself based upon an "appropriate" distribution of tastes and external costs. The salience measure they propose for their ordinal treatment of the paradox,  $s_i(x)$ , is itself a function of the associated cardinal utilities attached by an individual to the passage and failure of a given motion. Let  $u_i(P_x)$  be the utility to Member  $i$  derived from the outcome associated with his preferred position on motion  $x$  and let  $u_i(\bar{P}_x)$  be a similar utility for  $i$  derived from the outcome for his not preferred position on  $x$ . Then the salience of the motion is defined as:  $s_i(x) = |u_i(P_x) - u_i(\bar{P}_x)|$ , which is clearly a function of the cardinal properties of the associated utilities.<sup>26</sup>

Alterations in these cardinal utilities thus may entail alterations in the relative salience of the various motions, with the prospect that an "appropriate" distribution of tastes is not obtained. To take two examples,<sup>27</sup> in Table 1 above (a) consider the impact of changing the values of each motion on which the cardinal utilities are currently (1, -2) to (2, -1); or (b) consider a change from each entry of (-2, 2) to (-3, 3). Under the first change, a majority voting rule would yield each member a net utility of zero without vote trading and +2 if all players traded votes. The second proposed alteration of utilities in Table 1 would produce a unanimity rule outcome of -2 to each member without vote trading and of zero with logrolling. In both cases, the paradox is seemingly avoided. How can this occur?

The reason the paradox might be avoided under certain circumstances is that the distribution of preferences has a direct and identifiable impact on the vote trading situation. A second factor which must not be overlooked is that the ordinal properties of the paradox of voting can be maintained even if there are changes in the cardinal numbers associated with the specific outcomes in a chart of preferences such as Table 1. The change from utilities of (-2, 2) to (-3, 3) does not affect the ordinal rankings of the outcomes at

all with respect to producing a paradox of voting. Coleman's critical point is that there is a difference between merely noting that one is in a situation in which the paradox of voting occurs and attempting to devise a strategy to extricate oneself from such a situation.<sup>28</sup> On the other hand, we must realize that at a fundamental level the paradox of vote trading is indeed inescapable. As long as individual trades are rational, then we have a cyclical pattern of vote trading which can never be stable, as the Riker-Brams analysis demonstrates.<sup>29</sup> This result is *not* a function of the associated cardinal utilities which members attach to outcomes. It thus should be cautioned that we are not proposing a logical way out of the paradox (although a very restrictive one will follow from a theorem we propose), but rather a practical way out. In particular, we argue that there are basically three types of situations: those in which sincere voting is Pareto optimal, those in which vote trading (sophisticated voting) is Pareto optimal, and those in which no decision rule and no voting strategy can improve upon a preordained societal result. In any of these situations, strategies which yield less than an optimum may indeed be reached. Our concern, however, is to distinguish the conditions under which the paradox of vote trading will occur and those under which vote trading will yield a Pareto optimal solution. In order to make this distinction, we need a set of cardinal utility measures.

We thus propose such a set:  $u(x_{ij})$  for a set of voters  $i \in I$  and a set of motions  $j \in J$  where  $u(x_{ij})$  is the utility of the passage of the  $j$ th motion to the  $i$ th voter: and  $u(\bar{x}_{ij})$  where the various subscripts are as before and  $u(\bar{x}_{ij})$  is the utility of the defeat of such motions. We offer the following theorem to provide necessary and sufficient conditions for the optimality of vote trading:

#### Theorem

- (1) Sincere voting is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij}).$$

- (2) The strategy adopted (sincere vs. sophisticated voting) is neutral under majority rule if and only if:<sup>30</sup>

<sup>28</sup> Coleman, "The Possibility of a Social Welfare Function," especially p. 1107.

<sup>29</sup> Riker and Brams, "The Paradox of Vote Trading," p. 1244.

<sup>30</sup> Cf. Buchanan and Tullock, *The Calculus of Consent* (p. 145): "Potentially, the voter should enter into bargains until the marginal 'cost' of voting for something of which he disapproves but about which his feelings are weak exactly matches the expected marginal benefits of the vote or votes secured in return support for issues in which he is more interested."

<sup>25</sup> Koehler, "Vote-Trading and the Voting Paradox." See Appendix II.

<sup>26</sup> Riker and Brams, "The Paradox of Vote Trading," p. 1239.

<sup>27</sup> We are grateful to David H. Koehler (private communication) for suggesting the first example and to an anonymous referee for suggesting the second.

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

- (3) Vote trading is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

In the first appendix to this article, we present the proofs of the above optimality conditions. As formulated above, these proofs depend upon the assumption either that all motions pass or that all motions fail. We shall continue to employ this assumption in our discussion of the optimality conditions below because of its interesting consequences for "porkbarrel" projects on which legislators may trade votes. This restriction, however, can easily be lifted and our results generalized to situations in which some motions pass and others fail. What is critical here is that the relationships we derive are based upon the positions taken by the majority of legislators, on the one hand, and the minority of legislators, on the other hand.<sup>31</sup> Note that in part (1) of the theorem, Pareto optimality is itself defined by

$$\sum_i \sum_j u(x_{ij})$$

since this summation provides the boundaries for the production possibility curve in terms of the total utility for the society. Similarly, Pareto optimality for condition (2) can be represented by either

$$\sum_i \sum_j u(x_{ij}) \quad \text{or} \quad \sum_i \sum_j u(\bar{x}_{ij}),$$

while for condition (3), the optimality condition obviously is represented by

$$\sum_i \sum_j u(\bar{x}_{ij}).$$

We note that in the theorem, we do not restrict the trading situation to pairwise trades, as do Riker and Brams. The set of trading partners can be as large as the entire membership of the legis-

lative body. This lack of restriction to pairwise trades will have interesting consequences for analyzing the paradox of vote trading under different situations of Pareto optimality.

We state a corollary of the theorem without proof: Under condition (1) vote trading is Pareto optimal under a unanimity rule; under condition (2) the strategy adopted under a unanimity rule is neutral; and under condition (3) sincere voting is Pareto optimal under a unanimity rule. The last part of the corollary is perhaps the least obvious. In our discussion above, we noted that the effects of vote trading under a unanimity rule are indeed just the opposite of those under majority rule. Therefore, if vote trading is optimal under majority rule for condition (3), sincere voting would be expected to be (and indeed is) optimal under a unanimity rule.

### Conclusions

The appropriate distribution of tastes and externalities for the optimality of sincere voting, then, is simply that the voters collectively have more to gain by the passage of all the motions than they do by defeating them. This result is heuristic: majority rule without vote trading will produce a Pareto optimal result under such conditions. If the voters attach equal utility across the set of motions to their passage or their defeat, then no decision rule or strategic premise is going to change the net utility of the outcome. Finally, if in the aggregate, the voters attach more utility to the defeat of all motions than they do to passing all of them, then majority rule with vote trading (or a unanimity rule without logrolling) can yield a Pareto optimal result. Only, however, in the very restrictive case in which the size of the trading set is equal to the number of legislators will the paradox of vote trading be avoided in a formal sense. In this situation, we have a totally cooperative  $n$ -person game in which the enforcement power has been agreed upon by the players in advance. More likely situations involve either pairwise trades only—a noncooperative game—as developed by Riker and Brams and also by Gerald H. Kramer, or at best only a partially cooperative game (in which trading coalitions may be greater than pairwise in size but smaller than the entire chamber), as John A. Ferejohn has argued.<sup>32</sup> Thus, the optimality of vote trading under condition (3) does not extricate us from the paradox of vote

<sup>31</sup> This condition together with the condition for vote trading introduced in the proofs below, is based upon situations in which (under majority rule) all motions will pass. While seemingly restrictive, the same logic underlying the three proofs can easily be extended to "corollaries" assuming that all motions will fail or that some will pass and some will fail. A less cumbersome approach, however, can extend the theorem we have presented rather easily. Let  $u(x_{ij})$  represent the utility to legislator  $i$  of the majority position on issue  $j$  and  $u(\bar{x}_{ij})$  represent the utility to  $i$  of the minority position on  $j$  (irrespective of the nature of the actual outcomes). Then the theorem we propose is completely general and subsumes not only our special case but all of the possible "corollaries" representing alternative cases.

<sup>32</sup> See Riker and Brams, "The Paradox of Vote Trading," pp. 1238ff.; Kramer, "Sophisticated Voting over Multidimensional Choice Spaces," *Journal of Mathematical Sociology*, 2 (July, 1972), 125-180; and Ferejohn, "Sour Notes on the Theory of Vote Trading," California Institute of Technology, Social Science Working Paper Number 41 (June 1, 1974). We are particularly grateful to Steven J. Brams for his comments on this aspect of the present work.



trading unless we make the very strong assumption that the trading game is totally cooperative. On the other hand, all is not lost because *in actual legislative situations*, we would expect that "learning behavior" could easily provide the basis for legislators to extricate themselves from the paradox of vote trading. And, indeed, to the extent that legislators are rational actors, we should expect that they will indeed do so.

It is not terribly difficult for a rational legislator to realize when to stop trading and when the Pareto optimal position has been reached (particularly if he has been "stung" once by an occurrence of the paradox of vote trading). While Riker and Peter C. Ordeshook have argued that in most large legislatures members are likely to realize the potentially harmful effects of vote trading (and, hence, restrict any trading activity to a smaller proto-coalition such as fellow party members),<sup>33</sup> evidence from the American Con-

gress suggests that "learning behavior" is indeed evident on the optimality of vote trading on "porkbarrel" votes. As James T. Murphy has commented:

[On the House Public Works Committee] . . . the two parties *distrust* each other. After all, public works projects are inherently political and, as such, are presumed to have substantial electoral significance. Since each party values majority status, each expects the other to porkbarrel . . . when the other is in the majority. Because they foresee the possibility of partisan allocations and because they take the allocation of public works benefits so seriously, congressmen insist on a fair allocation of the goodies. Hence, they have agreed upon permanent ways to reduce electoral risks when a majority in each party is affected by an allocation. . . . *Only when a majority in each party benefits from an allocation can party cooperation be expected.* Since committee Democrats and Republicans are confronted with exactly the same situation as House Democrats and Republicans, the electoral risk-reducing devices explain party cooperation in the committee as well as in the House.<sup>34</sup>

<sup>33</sup> Riker and Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), pp. 113-114. Note especially their reference to James Murphy, *The Empty Porkbarrel* (Lexington, Mass.: D. C. Heath, forthcoming), which (according to Riker and Brams) argues that very little logrolling actually takes place even on public works legislation. Also, cf. David R. Mayhew, *Party Loyalty among Congressmen* (Cambridge: Harvard University Press, 1966: ch. 6) on the optimality of intra-party as opposed to inter-party cooperation. And, cf. the comments made in President Gerald R. Ford's inaugural speech to a joint session of Congress on August 12, 1974. Addressing in particular House Speaker Carl Albert (D., Okla.), Ford stated: "I have sometimes voted to spend more taxpayers' money for worthy federal projects in Grand Rapids [Michigan] while vigorously opposing wasteful federal boondoggles in Oklahoma" [cited in *Congressional Quarterly Weekly Report*, 32 (August 17, 1974), 2209]. Also cf. the comments of Martin and Susan Tolchin (*To the Victor* [New York: Vintage, 1972], p. 218): ". . . it was no surprise that when Representative Charles Joelson, a New Jersey Democrat, wrote a letter to each member of Congress asking 'Where can we economize in your district?' not one reply was returned to his office." Many members probably thought that the letter was just another example of the humor for which Joelson was known (and to which the first-named author of this article can attest). Further evidence for the claim that legislators may be aware of the paradox of vote trading can be inferred from Herbert B. Asher, "The Learning of Legislative Norms," *American Political Science Review*, 67 (June, 1973), 499-513. Asher notes (p. 508) that 68 per cent of a sample of 22 freshmen in the 91st House stated that they would be willing to trade votes with a colleague. While the effects of learning behavior do seem to be apparent in the House—more nonfreshmen (81 per cent of a sample of 21) expressed a willingness to trade than freshmen (p. 501)—the comment of a senior member is instructive (p. 503): "Yes, I'd trade votes, but this does not happen often. It is not a specific trade, but more a matter of good will. I never had a specific trade; this happens more often in the Senate." (On the utility of trading through "good will," or implicit logrolling, see Appendix II.)

<sup>34</sup> Murphy, "Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision-Making," *American Political Science Review*, 68 (March 1974), 179-180 (first emphasis in original; second emphasis added). We thus find ourselves in the interesting situation of citing a single author on two different sides of the same question (cf. note 33 above). We await publication of his book before pursuing the interpretation of Riker and Ordeshook any further. For similar comments, see Robert F. Fenno, Jr., *Congressmen in Committees* (Boston: Little, Brown, 1973), pp. 58, 156-159, and 165-166; and Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974), pp. 86-91. For a somewhat different perspective on the effects of vote trading for porkbarrel projects from that presented in the text, see Ferejohn, "Sour Notes on the Theory of Vote Trading," p. 11. For a statement which is more in accord with our view of the logrolling problem and also treats the problem of transaction costs (cf. note 23 above), see Mancur Olson, Jr., "The Optimal Allocation of Jurisdictional Responsibility: The Principle of 'Fiscal Equivalence'," in United States Congress, Joint Economic Committee, 91st Congress, First Session, *The Analysis and Evaluation of Public Expenditures: The PPB System*, I (Washington: Government Printing Office, 1969), pp. 321-331. Olson argues (p. 326): "If all mutually advantageous bargains were struck, logrolling would insure that all collective goods that it was Pareto-optimal to provide would be provided. But . . . especially where large groups of people are at issue, it will very often be the case that logrolling will not happen, and that there will not be a Pareto-optimal supply of public goods." He adds (p. 326, n. 14): "In the United States Congress, logrolling probably leads to a greater expenditure when projects of a 'pork barrel' type are at issue. In most of these cases the projects are of a tangible, if not monumental type, and a Congressman is more likely to be identified in his district with such a project than with a general tax increase, which could not in any case usually be traced to any one package of local projects." For an argument similar to that of Olson on government expenditures, see J. Ronnie Davis and Charles W. Meyer, "Budget Size in a Democracy," *Southern Economic Journal*, 36 (July, 1969), 10-17.

Thus, on issues in which individual rationality might lead a member to vote against a porkbarrel project for another member (and, particularly, one from the opposition party), there still might be a collective rationality in voting for the project—and members seem to be able to “learn” how to resolve the formal problem of pairwise trading instability.

The result is traceable to the fact that straightforward majority rule nevertheless will produce majorities for all six motions under condition (3); this voting procedure is insensitive to variations in individual cardinal utilities. The conditions for the optimality of vote trading, however, are built around assumptions about cardinal utilities. When the decision reached by counting votes is the same as that reached by counting units of utility—as in condition (1)—there is no conflict between the majority principle and the salience of individual issues. On the other hand, when the two criteria come into direct conflict, the mechanism of vote trading (or of a unanimity rule without vote trading, which also is constructed so as to protect “intense minorities”) has the potential to avoid a paradox of vote trading.

In particular, we are intrigued by the failure of a unanimity rule with vote trading to dominate the results obtained for such a rule without logrolling. In the first case, where a supposed domination does occur, we encounter the paradox of vote trading. However, the advocates of a unanimity rule with vote trading—Buchanan and Tullock—were particularly concerned with situations in which an intense minority might be tyrannized by a relatively apathetic majority. It is precisely in this situation—where

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij})$$

that vote trading is Pareto optimal under majority rule but inferior to sincere voting for a unanimity rule. The paradox with which we are confronted, then, is over the effects of alternative decision rules and voting strategies on the problem of externalities. We do not have a unidirectional linkage between the rules and the strategies, but a more complicated web than we had imagined. Indeed, the only situation in which a unanimity decision rule with vote trading is Pareto optimal involves “intense majorities” rather than “intense minorities.”

The relationship between the operative decision rule and the optimal voting strategy (if such a strategy exists) is itself a function of the distribution of preferences—and thereby affects the distribution of externalities. As Riker and Brams comment, the critical distinction is between the “market for votes” and the “market for goods”:

vote trading is an imperfect barter system in which only whole commodities (votes) rather than infinitely divisible ones (e.g., money) must be exchanged.<sup>35</sup> This feature of the imperfect vote market increases the effects of both the decision rule and the voting strategy on the magnitude of external costs and benefits. Only when externalities are effectively nil

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij})$$

does the relationship between voting strategies and decision rules cease to affect the total utility over the set of outcomes.

In the case of the paradox of vote trading, the imperfections of the market produce a dynamic relationship between decision rules and voting strategies which effectively prevents the attainment of Pareto optimality. Under the final set of conditions

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

either majority rule with vote trading or a unanimity rule without logrolling can yield a Pareto optimal outcome. Thus, the two mechanisms suggested by Buchanan and Tullock both may work to reduce external costs, but not simultaneously. When a unanimity rule yields an optimal distribution of utilities, vote trading can at best fail to improve upon this distribution and at worst produce a markedly suboptimal distribution of external costs, thereby leaving everyone worse off than if no votes had been cast at all.

Thus our analysis has demonstrated that the positions of Riker and Brams, on the one hand, and that of Buchanan, Tullock, and Coleman, on the other hand, are not necessarily in total conflict (although for rather different reasons from those Buchanan and Tullock posited). Except in the extreme case in which *all* legislators agree to trade (for majority rule), the paradox of vote trading cannot be avoided as a logical problem. This does not mean, however, that sincere voting is Pareto optimal under all circumstances. What we emphasize here is that Riker and Brams are concerned with a formal problem of voting games, whereas Buchanan, Tullock, and Coleman have stressed optimality conditions. Clearly, as we have demonstrated, the two situations are not logically equivalent. Which solution is more satisfactory depends upon the dynamics of an actual social situation and the answer to the fundamental question: how high are the costs of reaching a

<sup>35</sup> Riker and Brams, “The Paradox of Vote Trading,” pp. 1235 and 1238. Cf. Coleman, “The Possibility of a Social Welfare Function: Reply,” pp. 1315–1317.

collective decision (transactions costs) among all players?

### Appendix I

In this appendix, we present a proof of the theorem (but not the corollary) stated in the text. We restate the definitions of  $u(x_{ij})$  as the utility that the  $i$ th legislator receives from the passage of the  $j$ th motion and  $u(\bar{x}_{ij})$  as the utility he receives from the defeat of such motions. Denote the total set of voters (on any motion) as  $I$  and partition the set  $I$  into the following disjoint subsets:  $A$ , the set of traders (on any motions) and  $B$ , the set of nontraders on all motions. We also restate the theorem:

#### Theorem

(1) Sincere voting is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij}),$$

(2) The strategy adopted (sincere vs. sophisticated voting) is neutral under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

(3) Vote trading is Pareto optimal under majority rule if and only if:

$$\sum_i \sum_j u(x_{ij}) < \sum_i \sum_j u(\bar{x}_{ij}).$$

We shall also assume, without loss of generality, that

$$\sum_i \sum_j u(x_{ij}) = 0.$$

Given the definitions of Pareto optimality in the text, we immediately turn to the proof, which like the theorem itself, is in three parts.

First consider condition (1):

Let

$$\sum_i \sum_j u(x_{ij}) = 0 \text{ and } \sum_i \sum_j u(\bar{x}_{ij}) < 0$$

For trades to occur, we have:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0.$$

For nontraders, by stipulation:

$$\sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

Since the total utility to all players is assumed to be zero,

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) + \sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

To prove sufficiency, let

$$\sum_i \sum_j u(\bar{x}_{ij}) < \sum_i \sum_j u(x_{ij}) = 0.$$

If all members trade votes (i.e.,  $B$  is empty):

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij}).$$

But this contradicts the initial premise that

$$\sum_i \sum_j u(\bar{x}_{ij}) < \sum_i \sum_j u(x_{ij})$$

since, if  $B$  is empty

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0$$

$$\forall i \in A \rightarrow \sum_j u(\bar{x}_{ij}) > 0.$$

But

$$\sum_i \sum_j u(\bar{x}_{ij}) < 0$$

by stipulation. Thus,  $B$  is non-empty and sufficiency is established.

The proof of necessity is not as direct because

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij})$$

does not follow directly from the fact that  $B$  is non-empty. As condition (2) of the theorem demonstrates, the non-emptiness of  $B$  is also consistent with

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}).$$

If  $B$  is non-empty, then either (1) vote trading yields a net loss of utility from sincere voting; or (2) vote trading among all  $i \in I$  produces no net change of utility across the motions  $j \in J$ ; or (3) only a subset of  $I$ ,  $i \in A$ , can trade profitably and

$$\forall i \in B, \quad u(x_{ij}) < 0.$$

The condition for vote trading,

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0,$$

effectively rules out non-profitable trades among all  $i \in I$ , but it does not eliminate profitable trading among some subset of  $A \subset I$ . If such a subset can trade profitably,  $B$  may be non-empty and

$$\sum_i \sum_j u(x_{ij}) \geq \sum_i \sum_j u(\bar{x}_{ij}).$$

The suboptimality of vote trading, however implies that:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) < \sum_{i \in B} \sum_j u(x_{ij})$$

which in turn implies that  $A$  is empty and  $B$  is non-empty (specifically, the condition for vote trading is violated). Since

$$\begin{aligned} \sum_{i \in B} \sum_j u(x_{ij}) &> \sum_{i \in A} \sum_j u(\bar{x}_{ij}), \\ \sum_{i \in B} \sum_j u(x_{ij}) &> \sum_{i \in B} \sum_j u(\bar{x}_{ij}). \end{aligned}$$

Otherwise, vote trading would be profitable  $\forall i \in B$ . Since

$$\begin{aligned} \forall i \in I, i \in B, \sum_{i \in B} \sum_j u(x_{ij}) &> \sum_{i \in B} \sum_j u(\bar{x}_{ij}) \\ \rightarrow \sum_i \sum_j u(x_{ij}) &> \sum_i \sum_j u(\bar{x}_{ij}), \end{aligned}$$

establishing necessity.

The proof for the second condition is as follows:

Let

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}) = 0.$$

For trades to occur, we have:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0.$$

For the non-traders, by stipulation:

$$\sum_{i \in B} \sum_j u(x_{ij}) \leq 0$$

Thus

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) + \sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

To prove sufficiency, let

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}) = 0.$$

Assume that  $B$  is empty. Then, by the condition for vote trading,

$$\begin{aligned} \sum_{i \in A} \sum_j u(\bar{x}_{ij}) &> \sum_i \sum_j u(x_{ij}) = 0 \\ \rightarrow \sum_i \sum_j u(\bar{x}_{ij}) &> \sum_i \sum_j u(x_{ij}). \end{aligned}$$

since  $\forall i \in I, i \in A$ . But this yields a contradiction since

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij})$$

by assumption. Now assume that  $A$  is empty.

Then, by the condition for vote trading,

$$\sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

If the strict inequality holds,

$$\sum_{i \in B} \sum_j u(x_{ij}) < 0 \rightarrow \sum_i \sum_j u(x_{ij}) < 0$$

since  $\forall i \in I, i \in B$ . But, again, this yields a contradiction, implying that  $A$  is non-empty. Thus sufficiency is established.

To establish necessity, we demonstrate that: (1) if  $B$  is empty, vote trading is not profitable and (2) if  $A$  is empty, sincere voting would not improve upon the results of vote trading. Assume that  $B$  is empty. Then

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) \leq 0.$$

But this implies that

$$\sum_i \sum_j u(\bar{x}_{ij}) \leq 0$$

since  $\forall i \in I, i \in A$ . However, the condition for vote trading is

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > 0$$

and we have

$$\sum_i \sum_j u(\bar{x}_{ij}) > 0.$$

But, by assumption

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) + \sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

Thus, if  $B$  is empty, the condition for vote trading must be violated. Hence,  $B$  is non-empty. Now assume that  $A$  is empty. Then,

$$\begin{aligned} \sum_{i \in B} \sum_j u(x_{ij}) &\geq \sum_i \sum_j u(\bar{x}_{ij}) \\ \rightarrow \sum_i \sum_j u(x_{ij}) &\geq \sum_i \sum_j u(\bar{x}_{ij}) \end{aligned}$$

since  $\forall i \in I, i \in B$ . If the strict inequality holds, we have:

$$\sum_i \sum_j u(x_{ij}) > \sum_i \sum_j u(\bar{x}_{ij})$$

which implies that sincere voting would improve upon the results of vote trading. However,

$$\sum_i \sum_j u(\bar{x}_{ij}) = 0$$

by assumption and thus

$$\sum_i \sum_j u(x_{ij}) > 0.$$

This contradicts our assumption that

$$\sum_{i \in B} \sum_j u(x_{ij}) \leq 0,$$

since  $\forall i \in I, i \in B$ . Thus,

$$\sum_i \sum_j u(x_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}),$$

establishing necessity.

Finally we have:

Let

$$\sum_i \sum_j u(\bar{x}_{ij}) = 0 \text{ and } \sum_i \sum_j u(x_{ij}) < 0.$$

For trades to occur, we have:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij}).$$

For nontraders, by stipulation:

$$\sum_{i \in B} \sum_j u(x_{ij}) < 0.$$

To establish that vote trading is profitable under these conditions—the proof of sufficiency—we must show: (1)  $A$  is non-empty and (2)  $B$  may be empty. We need not demonstrate that  $B$  is always empty since that would imply that the only profitable trading situation occurs when all players trade, which is not maintained here.

Thus, the conditions for trading and nontrading give:

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) + \sum_{i \in B} \sum_j u(x_{ij}) \leq 0.$$

Let

$$\sum_i \sum_j u(\bar{x}_{ij}) = 0 \text{ and } \sum_i \sum_j u(x_{ij}) < 0.$$

Assume first that  $A$  is empty. Then

$$\sum_{i \in B} \sum_j u(x_{ij}) = \sum_i \sum_j u(x_{ij}) < 0$$

since  $\forall i \in I, i \in B$ . If  $A$  is empty, however, the condition for vote trading

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij})$$

must be violated. The violation of the condition for vote trading, however, contradicts our initial premise that

$$\sum_i \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij}).$$

Thus,  $A$  is non-empty. Now suppose that  $B$  is empty. Since  $A$  is non-empty,

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) \leq 0$$

and

$$\begin{aligned} \sum_{i \in A} \sum_j u(\bar{x}_{ij}) &> \sum_i \sum_j u(x_{ij}) \\ &\rightarrow \sum_i \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij}) \end{aligned}$$

if  $B$  is empty. Since this result does not contradict our initial premise,  $B$  may be empty and sufficiency is established.

To prove necessity we must show that

$$\sum_i \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij})$$

if: (1)  $A$  is non-empty and (2)  $B$  may be empty. If  $B$  is empty, then

$$\sum_{i \in A} \sum_j u(\bar{x}_{ij}) = \sum_i \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij})$$

by the condition for vote trading. Assume that  $A$  is empty. Then, the condition for vote trading must be violated and:

$$\begin{aligned} \sum_{i \in A} \sum_j u(\bar{x}_{ij}) &\leq \sum_i \sum_j u(x_{ij}) \\ &\rightarrow \sum_{i \in B} \sum_j u(x_{ij}) \\ &= \sum_i \sum_j u(x_{ij}) \geq \sum_i \sum_j u(\bar{x}_{ij}). \end{aligned}$$

Thus,

$$\sum_i \sum_j u(\bar{x}_{ij}) > \sum_i \sum_j u(x_{ij})$$

only if  $A$  is non-empty. Necessity is established.

## Appendix II

The distribution of tastes which Riker and Brams discuss (see note 24 above) is one which yields a cyclical majority. It can readily be confirmed that a cycle is found in Table 1 above. Particularly in the case of cyclical majorities, the strategy of implicit vote trading (always trading with the same partner) is not likely to yield optimal results. Implicit vote trading on a large scale implies a cohesive voting system (and the potential for responsible parties). Such a system is realizable only if the distribution is single-peaked. Riker and Brams ("The Paradox of Vote Trading," pp. 1244–1246) discuss this problem. A specific set of illustrations will help. Let  $u_i(1, 2)$  denote the utility to voter  $i$  obtained from an implicit vote trading agreement between members 1 and 2, etc. Then, for simple majority rule, we have:

$$\begin{array}{lll} u_1(1, 2) = 3 & u_1(1, 3) = 3 & u_1(2, 3) = -10 \\ u_2(1, 2) = 3 & u_2(1, 3) = -10 & u_2(2, 3) = 3 \\ u_3(1, 2) = -10 & u_3(1, 3) = 3 & u_3(2, 3) = 3 \end{array}$$

For a unanimity voting rule, we have:

$$\begin{array}{lll} u_1(1, 2) = -1 & u_1(1, 3) = -1 & u_1(2, 3) = 8 \\ u_2(1, 2) = -1 & u_2(1, 3) = 8 & u_2(2, 3) = -1 \\ u_3(1, 2) = 8 & u_3(1, 3) = -1 & u_3(2, 3) = -1 \end{array}$$

In the case of majority rule, implicit trading is profitable for both trading partners, but no single coalition dominates any of the others. Since the expected value of the three coalitions is equal to  $-4$ , the three coalitions are not even preferable to the results obtained from the paradox of vote trading. None of these coalitions, however, is dominated by the results from simple majority rule without vote trading, so that all three possible coalitions remain in the core of the voting game. For a unanimity rule, note that the nontrader still benefits from the trading activity of the others. The  $+8$  entries for the nontrader in each coalition are simply the values of his security level (see Tables 5 and 6 above). In each case, the traders are worse off than if no logrolling had occurred—but actually are better off than if they had at-

tempted to employ “explicit” sophisticated voting strategies. But, the outcomes for sophisticated voting are themselves not in the core for the voting game under a unanimity rule, whereas a unanimity rule with vote trading dominates each of the three solutions for “implicit” vote trading. Thus, the coalitions from implicit vote trading are not in the core for a unanimity voting rule—and neither is the “actual” solution of the voting game: a refusal to trade by each player. This “solution” is in turn dominated by each incidence of implicit vote trading. The paradoxical result is that the only solution considered which is in the core is not attainable. Hence, we have the paradox of vote trading and the contradiction is even more clearly defined for a unanimity rule than it is for a majority decision rule (in which implicit vote trading solutions are indeed in the core). This is the case because, as we argue above (see note 19), the vote trading situation under a unanimity rule is a better illustration of the traditional Prisoners’ Dilemma than is the situation under majority rule.

# The Instability of Minimum Winning Coalitions\*

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"In  $n$ -person, zero-sum games, where side payments are permitted, where players are rational, and where they have perfect information, only minimum winning coalitions occur."<sup>1</sup>

This is William Riker's statement of the size principle as it first appeared in his ground-breaking book, *The Theory of Political Coalitions*. It has been perhaps the most widely accepted and recognized finding of the new school of deductive political science. Yet recently, other political scientists have begun to express doubts about its validity and to adduce arguments calling it into question.<sup>2</sup>

Robert Butterworth has demonstrated that rational individuals might form larger than minimum winning coalitions in symmetric zero-sum games. Using a five-person game as a counterexample he has demonstrated that if a three-person minimum coalition is about to form it can be to the advantage of one of the potential losers to bribe his way into the coalition. He also shows that it is to the advantage of the coalition members to allow themselves to be bribed and that the one remaining individual who expects to be left outside the expanded coalition cannot rationally expect to entice the briber back into the losing coalition by offering to bribe the briber. His conclusion is, therefore, that larger than minimum winning coalitions are possible.

In a recent article and series of exchanges with

Butterworth, Kenneth Shepsle has challenged some aspects of this argument.<sup>3</sup> He points out that Butterworth failed to take into account the possibility that the one individual who is in danger of being left out of the coalition also has the option of entering into competitive bribery with the initial briber for the favor of being allowed into the minimum winning coalition. Shepsle concludes that such competitive bribery would place the two potential losers in such an unfavorable position that they would find it advantageous to recoallesce together thereby allowing the minimum coalition of three to form. He acknowledges, however, that another round of competitive bribery could start at any time and that the minimum coalition configuration is unstable.

The purpose of this paper is to generalize and extend the findings of the authors discussed above beyond five-person games to  $n$ -person games. It will be shown that for a general class of essential, symmetric zero-sum games with side payments, larger than minimum winning coalitions are compatible with rational behavior on the part of individuals.<sup>4</sup> Moreover, it will be demonstrated that only in a highly restricted class of games will minimum winning coalitions be immune to the bribery of the form identified by Butterworth.

To prove these assertions I will examine an essential symmetric zero-sum game and specify the conditions under which some subset of players not in the minimum winning coalition have an incentive to make an offer to the winners to be let into the coalition which the winners will find profitable to accept. Under these conditions it will be demonstrated that the residuum of losers (those not attempting to get in) will have an incentive to make a counteroffer to the potential defectors to keep them out of the winning coalition. Moreover it will be shown that the best possible return to the potential defectors will be the same for the two alternatives available, (i.e. join-

\* I have been the beneficiary of a great deal of helpful inspiration and criticism in the preparation of this paper. The impetus for it was provided by an idea in a preliminary draft of Oran Young's paper, "The Size of Winning Coalitions: A Note on Riker's Size Principle." The argument has been refined and expanded as a result of truly helpful comments from Lawrence Dodd, Russell Hardin, Joe Oppenheimer, Kenneth Shepsle, Jeff Smith, and Harrison Wagner. I am deeply grateful for their help. An earlier version of this paper was presented at the annual meeting of the Public Choice Society, New Haven, Connecticut, March 1974. Research on this paper was supported by National Science Foundation Grant #GS-33490.

<sup>1</sup> William Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), p. 32.

<sup>2</sup> Robert Lyle Butterworth, "A Research Note on the Size of Winning Coalitions," *American Political Science Review*, 65 (September, 1971), 741-745, and Russell Hardin, "Hollow Victory: The Minimum Winning Coalition," Fels Center of Government, Discussion Paper #30. Butterworth calls Riker's findings into question via a theoretical argument, whereas Hardin presents a different theoretical argument as well as a discussion of the empirical problems inherent in any attempt to test Riker's thesis. Although both make telling points the argument here concentrates on extending Butterworth's line of reasoning.

<sup>3</sup> Kenneth Shepsle, "On the Size of Winning Coalitions," *American Political Science Review*, 68 (June, 1974), 505-518, Robert Lyle Butterworth, "Comment on Shepsle's 'On the Size of Winning Coalitions,'" and Shepsle, "Minimum Winning Coalitions Reconsidered: A Rejoinder to Butterworth's 'Comment,'" *Ibid.*, 519-524.

<sup>4</sup> Riker's argument was articulated in terms of symmetric games, as can be seen from his use of a graphical representation of the characteristic function. This paper discusses only symmetric games of the type treated by Riker.

ing the minimum winning coalition or being paid to stay out of the minimum winning coalition). Thus if bidding proceeds in this fashion, larger than minimum winning coalitions are possible, but the situation is indeterminate.

It will be further demonstrated that even if the residuum of losers try competitively to bid their way into the winning coalition (rather than attempting to bribe the defectors into remaining in the losing coalition) the situation is no more determinate. A cyclical bribery situation would result which could arbitrarily terminate at any size leaving larger than minimum winning coalitions.

Finally it will be shown that the conditions which allow for the onset of a potentially cyclic and indeterminate round of bribery are general. But the conditions necessary to preclude the possibility of such bribery, and so to preclude the formation of larger than minimum winning coalitions, will be shown to be quite restrictive. Only in games in which the potential set of losers has no incentive to form any countercoalitions is this form of bribery ruled out. This special class of games will be called "games inessential over coalitions of losers." Only in these games can Riker's principle be supported. The conclusion therefore is that in most symmetric zero-sum games larger than minimum winning coalitions can form.

To see how this result follows consider the following game. Let  $v(i)$  represent the value of a coalition of  $i$  members. Then to specify the game as an essential  $n$ -person, zero-sum symmetric game we require that:

- (1)  $v(i)$  be defined over the integers  $0 \leq i \leq n$  ( $n \geq 5$ );<sup>6</sup>
- (2)  $v(i) = -v(n-i)$ ;
- (3) for all positive integers  $i$  and  $j$  such that  $i+j \leq n$ ,  $v(i)+v(j) \leq v(i+j)$ ;
- (4) for some  $i$  and  $j$ ,  $v(i)+v(j) < v(i+j)$ .

Condition 1 is simply the requirement that the payoffs to each size coalition be specified, i.e. that the game is well defined and symmetric. Condition 2 is the zero-sum condition. Condition 3, superadditivity, is the requirement that any coalition can do at least as well as the sum of its parts; no loss is incurred to the members of two coalitions if they coalesce. Condition 4 is simply the requirement that there is at least one coalition which actually makes the members better off than they could be if they failed to coalesce but remained in smaller coalitions. In the absence of Condition 4 there is never any incentive for any coalitions to form.

<sup>6</sup> In this paper I examine only games involving five or more players. This allows us to examine competitive bribery when there are at least two potential losers. All of the results not involving competitive bribery can be easily demonstrated for games with fewer than five members.

To develop the argument, let  $S$  represent the minimum winning coalition of  $s$  members where  $s \leq n-2$ . There are therefore, at least two potential losers. Let  $-S$  be the losing coalition. Then let  $r$  be any integer such that  $1 \leq r < n-s$  and call some set of  $r$  individuals in  $-S$ ,  $R$ . Thus  $R$  is a subset of losers but not all of the losers. Call the remaining subset of losers in  $-S$ ,  $Q$ . For convenience let us refer to the number of individuals in  $Q$  as  $q$ , bearing in mind that  $q+r=n-s$ . Then we have a minimum winning coalition  $S$  and a partition of the losers into two proto-coalitions  $R$  and  $Q$ .

Assume that the minimum winning coalition  $S$  has tentatively formed, and in keeping with the symmetry of the game has agreed on a symmetric distribution of the rewards of  $v(s)$ . Suppose that the members of  $R$  were to attempt to offer the members of  $S$  some bribe to be split among them in a symmetric fashion. What would be the necessary conditions for the members of  $R$  to be able to make an acceptable bid to the members of  $S$  to be let into the winning coalition? Such a condition is the existence of an "a"—i.e., the amount that the members of  $R$  would give to the members of  $S$ , (or get from them if  $a$  were negative), which allows the following inequality to hold:<sup>6</sup>

$$\begin{aligned} S's \text{ new payoff} &= v(s+r) + a > v(s) \\ &= S's \text{ old payoff or} \quad (1) \\ v(s+r) - v(s) &> -a. \end{aligned}$$

But, of course another necessary condition for members of  $R$  to make such a bid is the requirement that they would be better off if it is accepted than they would be if the losing coalition symmetrically shared the cost of the losers.<sup>7</sup> This condition is:

$$\begin{aligned} R's \text{ new payoff} &= -a > -\frac{r}{r+q} v(s) \\ &= R's \text{ old payoff.} \quad (2) \end{aligned}$$

<sup>6</sup> Here for the sake of convenience I treat the division of payoffs between  $S$  and  $R$  as if  $S$  appropriated to itself all of the winnings of  $v(s+r)$  and in addition got a bribe of amount "a" from  $R$  for the formation of  $S \cup R$ . The use of the term "bribe," however, may be misleading. If, for example, the game in question were of positive slope over the range  $s < r$  then the members of  $S$  might actually offer the members of  $R$  a "bribe" to secure the formation of  $S \cup R$ . In that event "a" would be a negative quantity. In either event other ways of representing the division of payoffs are possible. One could posit that the members of  $S \cup R$  divided the payoffs symmetrically and that in addition the members of  $R$  gave the members of  $S$  a bribe of size "b". The analysis would proceed in the same way as it does in this representation. Only the algebra would be different. The conclusions would be the same.

<sup>7</sup> The assumption that the "losers" initially expect to share the losses symmetrically is not necessary for the argument which follows. It is a convenience, adopted for ease of exposition. All the conclusions that follow can be derived without that assumption.



Conditions (1) and (2) can be combined to yield condition (3) which is necessary for a mutually acceptable bid:

$$v(s+r) - v(s) > -a > -\frac{r}{r+q}v(s) \quad \text{or} \quad (3)$$

$$v(s+r) > \frac{q}{r+q}v(s).$$

Notice that (3) is simply a restriction on the slope of the characteristic function over the range  $s$  to  $s+r$ . Condition (3) always holds if the slope over that range is positive and it specifies a maximum rate of drop off which allows  $R$  to bid successfully.

By a similar argument we could derive the condition on the characteristic function that would allow  $Q$  to make a mutually acceptable bid to  $S$  to be let into the minimum winning coalition. That condition is:

$$v(s+q) > \frac{r}{r+q}v(s) \quad (3')$$

Now suppose that condition (3) held and hence that  $R$  could offer  $S$  a bribe. Could this result in a larger than minimum winning coalition? If  $R$  offered  $S$  a bribe one possibility is that  $Q$  might offer  $R$  a bribe *not* to enter into the coalition with  $S$ . The coalition  $Q$  could offer  $R$  any amount  $a'$  consistent with the inequality:

$$Q's \text{ position if } R \text{ accepts } Q's \text{ bribe}$$

$$= -a' + v(r+q) > v(q) \quad (4)$$

$$= Q's \text{ position if } R \text{ joins } S$$

or  $-a' > v(q) - v(r+q).$

Using the zero-sum condition on (4) it becomes:

$$a' < v(s+r) - v(s). \quad (5)$$

Thus  $a'$  is  $R$ 's net payoff position as offered by  $Q$ . From (5) it follows that if  $Q$  is interested in getting  $R$  to stay out of a coalition with  $S$  they can offer  $R$  a net payoff position which approaches  $v(s+r) - v(s)$ . The best return  $R$  could get from  $Q$  would approach that amount. But from (1) the net return that  $R$  could get from entering into a coalition with  $S$ ,  $-a$ , also has an upper limit of  $v(s+r) - v(s)$ . Thus, if  $S$  and  $Q$  compete for  $R$  as coalition partners the best bids that each could make to  $R$  would have the same upper limit of  $v(s+r) - v(s)$ . One cannot say whether  $R$  will join with  $S$  or remain in  $-S$ . Under these bidding conditions one cannot say whether the winning coalition which will form is the minimal  $S$  or  $S \cup R$ .

But as Shepsle has pointed out, the options

open to the set  $Q$  are not necessarily restricted to attempting bribery of the members of  $R$  to keep them from coalescing with  $S$ . The members of  $Q$  also have open the option of attempting to competitively outbid  $R$  for the privilege of coalescing with  $S$ . Suppose that  $Q$  were to attempt such a counterbid? Since  $Q$  faces the possibility of receiving  $-v(s+r)$  if  $R$  succeeds in its bid,  $Q$  would conceivably be willing to offer up to  $v(s+r)$  to be let into the coalition with  $S$ . Were  $S$  to accept such a bid and form  $S \cup Q$  the net return to  $S$  would have an upper bound of:

$$v(s+q) + v(s+r).$$

But such an outcome would leave the members of  $R$  in the lurch. Their net return under such an arrangement would be  $-v(s+q)$ . To forestall such an eventuality they might be tempted to up the amount they would be willing to offer  $S$ . They conceivably would be willing to offer up to  $v(s+q)$  for the privilege of forming  $S \cup R$ . Under their maximum rational offer the net return to  $S$  would approach:

$$v(s+r) + v(s+q).$$

Now each of these maximal bids to  $S$  are of the same size. There is no way of concluding that one or another of the two coalitions  $S \cup R$  or  $S \cup Q$  will be more attractive to  $S$  and will, therefore, form. Moreover, following Shepsle's argument, one can notice that  $Q$  and  $R$ , by bidding for  $S$ 's favor on the basis of the worst that might befall them if the other were to join with  $S$ , may well put themselves in a position that could be improved to their mutual benefit were they to fall back into the coalition  $R \cup Q$ . To see this, notice that their joint return when they outbid one another amounts to  $-v(s+q) - v(s+r)$  or, by the zero-sum condition,  $v(r) + v(q)$ , which is the worst they possibly could do acting on their own as coalitions. If the game in question is essential, to the extent that  $v(r) + v(q) < v(r+q)$ , then they would have an incentive to form  $R \cup Q$  to cut their losses and to get  $v(r+q)$ . In such a case when  $S$  is a minimum winning coalition one might find the tendency identified by Shepsle to move toward a minimum winning coalition again, but it would not necessarily follow. For  $S$  would have an incentive to prevent the defection of the winner of the competitive bidding.  $S$  would conceivably enter into a bidding contest to stop the possible regrouping of  $R$  and  $Q$ . This situation would then resemble one of the bidding situations identified above, and the size of the resultant coalition would not necessarily be the minimum winning size.

But of course this argument was predicated on the assumption that condition (3) held for some  $r$  and  $q$ . If the condition holds then larger than minimum winning coalitions are possible. Simi-

larly, if condition (3') holds, larger than minimum coalitions could form due to  $Q$ 's initial bidding. Only if neither (3) nor (3') hold for any  $r$  and  $q$  is such bidding precluded and one can expect only minimum winning coalitions to form.

Let us then examine the characteristics of games for which neither inequality (3) or (3') hold for any  $r$  and  $q$ . If (3) does not hold then for any  $r$  and  $q$

$$v(s+r) \leq \frac{q}{r+q} v(s), \quad (6)$$

and if (3') does not hold, then

$$v(s+q) \leq \frac{r}{r+q} v(s). \quad (6')$$

But it can be shown that the strict portions of the inequalities in (6) and (6') cannot hold. To see this notice that from (6) by the application of the super-additivity condition one can derive:

$$v(s) + v(r) \leq v(s+r) \leq \frac{q}{r+q} v(s),$$

which implies

$$\frac{r}{r+q} v(s) \leq -v(r),$$

and by applying the zero-sum condition we get

$$\frac{r}{r+q} v(s) \leq v(s+q). \quad (7)$$

Now comparing (6') and (7), it is apparent that strict equality must hold in both for them to be consistent. Thus the negation of conditions (3) and (3') imply that:

$$v(s+q) = \frac{r}{r+q} v(s). \quad (8)$$

Similarly it can be shown that:

$$v(s+r) = \frac{q}{r+q} v(s). \quad (8')$$

Adding (8) and (8') we obtain the restriction on the characteristic function necessary to preclude the cyclic bribery. It is

$$v(s+q) + v(s+r) = v(s).$$

Applying the zero-sum condition to this equation yields the condition:

$$v(r) + v(q) = v(r+q). \quad (9)$$

Equation (9) must hold for all partitions of losers, i.e., all partitions of  $-S$ , if only minimum winning coalitions are to be expected. If we call games for which (9) holds for all  $r$  and  $q$  such that  $r+q=n-s$  "inessential over coalitions of losers," we can restate our result succinctly. Only in games which are inessential over coalitions of losers can minimum winning coalitions be expected to form exclusively.

Once identified, this result is quite intuitive. If a game does not conform to (9) then the losers have an incentive to form a countercoalition. The incentive consists of a potential cut in their losses. But the saving to be gained by forming a countercoalition would be accomplished at the expense of the minimum winning coalition. Consequently, members of the minimum winning coalition have an incentive to stop the formation of this countercoalition, or to break it if it has formed. They could do so by offering to bring some of the losers into their coalition and by sharing with them some of the losses thereby saved. The losers left out in the cold by such a maneuver could attempt to lure back the defectors or to outbid them for the favors of the minimum winning coalition. In either case the size of the resulting coalition is indeterminate and potentially larger than minimum winning size. Only if there are no potential savings to losers from forming a countercoalition is there no incentive for the bribery to begin.

This analysis has implications for future empirical research into the formation of minimum winning coalitions. Although minimum winning coalitions cannot be expected to form in general, there is a theoretically restricted class of cases in which we would expect to find only such coalitions. Moreover, this set may still be empirically interesting. For example, the formation of multi-party cabinets in some legislatures may be situations in which those excluded stand to gain nothing by coalescing among themselves. To the extent these are zero-sum games one should expect only minimum winning coalitions to form in these cases. On the other hand if, in some other legislatures, losers stand to gain from coalescing, then larger than minimum winning cabinet coalitions can be expected.<sup>8</sup> The payoff structure for losers, therefore, is an important variable to be considered in any prediction of coalition behavior and any future testing of hypotheses dealing with the formation of minimum winning coalitions.

<sup>8</sup> Differences in the payoff structures in legislatures in different countries might explain some of the variance in cabinet size found by researchers such as Lawrence Dodd, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis," *American Political Science Review*, 68 (September, 1974), 1093-1117.

# Evaluation of a Presidential Election Game

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## 1. Introduction

There has been considerable discussion, over the past decade, of the merits of the presidential election system in the United States. In particular, discussion has frequently centered on the excessive power which this system seems to give one group or another (the large states, the small states, organized minorities within one or another of these kinds of states, etc.), though there is also frequent disagreement about the identity of these favored groups.

In this paper we carry out an evaluation of the presidential election game. The Shapley value<sup>1</sup> is computed approximately by the method of multilinear extensions.<sup>2</sup> A comparison with the Banzhaf value<sup>3</sup> will also be made.

The following simplifying assumptions will be made:

- (a) The number of voters within a state is directly proportional to that state's population, according to the last census.
- (b) The presidential electors for each state are pledged to the candidate who carries that state, and have no choice but to abide by their pledge.
- (c) The election will be decided by the electors rather than reaching the House of Representatives.

The reason for assumption (a) is that it allows us to work directly with census figures rather than having to count voter rolls, and having to figure out the number of potential but unregistered voters. The assumption is reasonable, in the sense that we might expect voters to be approximately proportional to total population. There may, however, be certain states with large alien populations for which this assumption is not very accurate. Additionally, we will find that toward the end of the decade, a fast-growing state (e.g., Florida or California) is underrepresented in the Electoral College (i.e., by the 1980 election, Florida's population may be much larger than represented by the apportionment which was based on 1970 census figures).

Assumption (b) also seems to be reasonable.

<sup>1</sup> Lloyd S. Shapley, "A Value for n-Person Games," in *Annals of Mathematics Studies* No. 28 (Princeton: Princeton University Press 1953), 307-327.

<sup>2</sup> Guillermo Owen, "Multilinear Extensions of Games," *Management Science*, 18 (1972), 64-79.

<sup>3</sup> John F. Banzhaf III, "One Man, 3,312 Votes: A Mathematical Analysis of the Electoral College," *Villanova Law Review*, 13 (1968), 304-332.

Admittedly the electors are (constitutionally) free to vote for anyone, and in the last few elections some have actually done so. Nevertheless it seems doubtful that this independence would be exercised in a truly close election.

Assumption (c), of course, does not always hold. Nevertheless, referral to the House of Representatives occurs so infrequently (this last happened after the 1824 election) that we consider its probability negligible. (It should be noted that in 1968, when this possibility seemed to be somewhat probable, Mr. Wallace seemed more interested in making a deal *within* the Electoral College than in letting the election be decided in the House.)

## 2. Game-Theoretic Basis

A game, as understood here, is merely a pair  $(N; v)$  where  $N$  is a (finite) set of players, and  $v$  is a function which assigns to each subset  $S \subset N$ , a number  $v(S)$ . The subsets are called coalitions; the elements  $i$  of  $N$  are players.

The presidential election can be thought of as a game  $(N; u)$ , where  $N$  is the set of all voters in the country, and  $u$  assigns, to each coalition  $S$  of voters, the number 0 or 1, depending on whether  $S$  is "large" enough to win the election against its complement,  $N-S$ . Thus,  $u(S)=1$  if  $S$  contains a majority of the voters within each of a set  $T$  of states, these states (in  $T$ ) having at least 270 electoral votes. Otherwise,  $u(S)=0$ .

For an arbitrary game  $(N; v)$ , the Shapley value is a vector  $\mathcal{J}[v]$ , whose components  $\mathcal{J}_i[v]$  are given by

$$2.1 \quad \mathcal{J}_i[v] = \sum_{\substack{S \subset N \\ i \in S}} \frac{s!(n-s-1)!}{n!} [v(S \cup \{i\}) - v(S)]$$

where  $s$  and  $n$  are the number of players in  $S$  and  $N$ , respectively. This component  $\mathcal{J}_i[v]$  is known as the *value to player  $i$  of the game  $v$* , and represents (in some sense) his power within that game. It is this value which we shall study for the presidential election game.

As can be seen, the formula (2.1) requires the sum of a very large number of terms (i.e., one for each coalition which does not contain  $i$ ) and is therefore quite difficult to compute when  $N$  is large. In particular, for a presidential election, where  $N$  is of the order of  $10^8$ , a direct evaluation seems out of the question. Recourse is therefore

frequently had to the multilinear extension (MLE) of the game. Essentially, the MLE of game  $(N; v)$  is the function  $f(x_1, \dots, x_n)$  defined by

$$2.2 \quad f(x_1, \dots, x_n) = \sum_{S \subset N} \prod_{i \in S} x_i \prod_{i \notin S} (1 - x_i) v(S).$$

It is shown in an earlier paper by the author<sup>4</sup> that, if  $f$  is the MLE of  $v$ , then

$$2.3 \quad g_i[v] = \int_0^1 f_i(t, t, \dots, t) dt$$

where  $f_i$  is the  $i$ -th partial derivative  $\partial f / \partial x_i$ .

The advantage of using the MLE is that it can be given a probabilistic interpretation. Thus,  $f(x_1, \dots, x_n)$  is the mathematical expectation

$$E[v(\bar{S})],$$

where  $S$  is a random coalition, determined probabilistically by saying that there is probability  $x_i$  that player  $i$  will belong to  $\bar{S}$ , and that the probabilities of different players belonging are independent. This interpretation will allow us to give approximations based on the central limit theorem.

### 3. The State Game

If we disregard the process by which the electors are themselves chosen, we can think of the electoral college as itself representing a game. By assumption 1(b) above, all the electors from a given state will vote for the same candidate, and so each state can be thought of as a single player. We have, then, here, a *weighted majority game*  $(M; v)$ , where  $M$  is the set of all states (plus the District of Columbia), and, for any  $T \subset M$ ,

$$3.1 \quad v(T) = \begin{cases} 0 & \text{if } \sum_T w_j \leq 269 \\ 1 & \text{if } \sum_T w_j \geq 270 \end{cases}$$

$w_j$  being the number of electoral votes that state  $j$  has.

For this 51-person game, a direct computation of the Shapley value (2.1) still seems out of the question. Use can, however, be made of certain properties of the game (e.g., that  $v$  has only the values 0 or 1, and that  $v(T)$  depends only on  $\sum_T w_j$ ) to obtain an exact result through a method of generating functions. The exact details are given in Shapley and Mann<sup>5</sup> as calculated for the

Electoral College after the 1950 and 1960 census reapportionments.

As against this exact method, we may approximate the Shapley value by the method of MLE's. In fact, the partial derivative  $f_i(x_1, \dots, x_n)$  can be expressed as

$$3.2 \quad f_i(x_1, \dots, x_n) = E[v(\bar{S} \cup \{i\}) - v(\bar{S})],$$

where  $\bar{S}$  is a random coalition, such that player  $j (j \neq i)$  has probability  $x_j$  of belonging to  $\bar{S}$ , and that these probabilities are independent. (Here,  $\bar{S}$  is assumed not to contain  $i$ .) Now, in 3.2, each of the terms  $v(\bar{S} \cup \{i\})$  and  $v(\bar{S})$  equals either 0 or 1, and  $v(\bar{S}) \leq v(\bar{S} \cup \{i\})$ , so that the variable  $v(\bar{S} \cup \{i\}) - v(\bar{S})$  can have only the two values 0 or 1. Its expectation, then, is the probability that it equal 1, and this happens only if  $v(\bar{S}) = 0$  but  $v(\bar{S} \cup \{i\}) = 1$ . By (3.1), this happens if

$$\sum_{\bar{S}} w_j \leq 269$$

and

$$w_i + \sum_{\bar{S}} w_j \geq 270.$$

Thus,

$$3.3 \quad f_i(x_1, \dots, x_n) = \text{Prob} \left\{ 270 - w_i \leq \sum_{\bar{S}} w_j \leq 269 \right\}$$

Let, now,  $Y = \sum_{\bar{S}} w_j$ . Then  $Y$  is a random variable, equal to the sum of 50 variables  $Z_j$ , where

$$Z_j = \begin{cases} 0 & \text{if } j \notin \bar{S} \\ w_j & \text{if } j \in \bar{S}. \end{cases}$$

As we have seen above, the variables  $Z_j$  are independent.  $Z_j$  has the values 0 and  $w_j$  with probabilities  $1 - x_j$  and  $x_j$ , respectively. Thus  $Z_j$  has mean and variance

$$3.4 \quad \mu(Z_j) = x_j w_j \quad \sigma^2(Z_j) = x_j(1 - x_j)w_j^2$$

and  $Y$ , being the sum of these, has mean and variance

$$3.5 \quad \mu(Y) = \sum_{j \neq i} x_j w_j$$

$$3.6 \quad \sigma^2(Y) = \sum_{j \neq i} x_j(1 - x_j)w_j^2.$$

Since  $Y$  is the sum of many (50) independent random variables, all with similar distributions, it is possible to approximate  $Y$  by a normal random variable having the mean (3.5) and variance (3.6). Since  $Y$  has only integer values, but the

<sup>4</sup> Owen, "Multilinear Extensions."

<sup>5</sup> Lloyd S. Shapley and Irwin Mann, "Values of Large Games, VI: Evaluating the Electoral College Exactly," RM-3158-PR, The RAND Corporation, May 1962.

normal variable is continuous, a correction is made, setting 269.5 (rather than 270) as the necessary number of votes. Then we have the approximation

3.7 
$$f_i(x_1, \dots, x_n) \cong \text{Prob} \{ 269.5 - w_i \leq Y' \leq 269.5 \},$$

where  $Y'$  is a normal variable with the mean (3.5) and variance (3.6).

Applying formula (2.3), now we notice that, if  $x_1 = x_2 = \dots = x_n = t$ , then (3.5) and (3.6) take the form

3.8 
$$\mu(Y) = t \sum_{j \neq i} w_j$$

and

3.9 
$$\sigma^2(Y) = t(1 - t) \sum_{j \neq i} w_j^2.$$

The righthand side of (3.7) can now be evaluated (for given values of  $t$ ) by means of a table of the normal distribution; it is then necessary to integrate this (numerically), to obtain the power of state  $i$ .

In Table 1, values are given both by the exact method and by the MLE approximation, for the 1970 apportionment. The approximation, while not exact, is really not bad. (Maximum error 0.367 per cent.)

4. The Composed Game

We proceed now to the presidential election

game. This can best be described by saying that it is the composition  $v[u_1, u_2, \dots, u_{51}]$  of the game  $v$ , described in section 3, with the 51 games  $u_j$  which are played within the individual states, i.e., a game is played within each state to determine the state's electors, who then vote for the presidential candidate according to the game  $v$ .

Each of the games  $u_j$  is very easy to handle—in fact,  $u_j$  is a simple majority game for  $m_j$  players— $m_j$  being the number of voters in the state, which we assume proportional to the state's population. Thus

$$u_j(S) = \begin{cases} 0 & \text{if } s \leq \frac{m_j}{2} \\ 1 & \text{if } s > \frac{m_j}{2} \end{cases}$$

where  $s$  is the number of players in  $S$ .

It is, of course trivial—by symmetry—to compute the Shapley value for each of the games  $u_j$ . Unfortunately, composition of games does not generally give rise to a composition of values, and so it is not possible to obtain exact values for the composed game.

Turning to the MLE's, we find, however, that MLE's do compose when the games compose. Thus, if the game  $v$  has MLE  $f$ , while the games  $u_1, \dots, u_{51}$  have MLE's  $g_1, \dots, g_{51}$  respectively, then the compound game  $v[u_1, \dots, u_{51}]$  has the MLE  $h$ , where

Table 1

Electoral Votes	Number of States	Exact Value $\mathcal{G}_i$	Rescaled Index 538 $\mathcal{G}_i$	MLE Approximation $\mathcal{G}_i'$	Approximation Ratio $\mathcal{G}_i'/\mathcal{G}_i$
45	1	.08830938	47.510446	.08852076	1.00239
41	1	.07972734	42.893309	.07975829	1.00039
27	1	.05096311	27.418153	.05113219	1.00332
26	2	.04897682	26.349529	.04915304	1.00360
25	1	.04699893	25.285424	.04715678	1.00336
21	1	.03916926	21.073062	.03927665	1.00274
17	2	.03146563	16.928509	.03157240	1.00339
14	1	.02576694	13.862614	.02586151	1.00367
13	2	.02388196	12.848494	.02396389	1.00343
12	3	.02200413	11.838222	.02207416	1.00318
11	1	.02013337	10.831753	.02019398	1.00301
10	4	.01826959	9.829039	.01833326	1.00349
9	4	.01641273	8.830049	.01647300	1.00367
8	2	.01456270	7.834733	.01461150	1.00335
7	4	.01271944	6.843059	.01276382	1.00349
6	4	.01088284	5.854968	.01092260	1.00365
5	1	.00905301	4.870519	.00907755	1.00271
4	9	.00722957	3.889509	.00724257	1.00180
3	7	.00541245	2.911898	.00543059	1.00335
Total	51	0.99999997	537.999803	1.00296983	

Table 2

	Population $m_i$	Electoral Votes $w_i$	Total Value $\Psi_i$	Value Per Inh. $\Psi_i/m_i$ ( $\times 10^{-9}$ )	Ratio $v_i/v_{d.e.}$	Banzhaf Ratio $\beta_i$
1960						
Alabama	3,266,740	10	.013971	4.2767	1.615	1.632
Alaska	226,167	3	.0011068	4.8937	1.845	1.838
Arizona	1,302,161	5	.0044025	3.3809	1.275	1.281
Arkansas	1,786,272	6	.0062101	3.4766	1.313	1.315
California	15,717,204	40	.130848	8.3251	3.143	3.162
Colorado	1,753,947	6	.0061309	3.4955	1.321	1.327
Connecticut	2,535,234	8	.0098302	3.8774	1.464	1.477
Delaware	446,292	3	.0015535	3.4809	1.313	1.308
Dist. of Columbia	763,956	3	.0020251	2.6508	1.000	1.000
Florida	4,951,560	14	.024200	4.8873	1.845	1.870
Georgia	3,943,116	12	.018404	4.6674	1.762	1.789
Hawaii	632,772	4	.0024692	3.9022	1.472	1.468
Idaho	667,191	4	.0025366	3.8019	1.434	1.429
Illinois	10,081,158	26	.065190	6.4665	2.430	2.491
Indiana	4,662,498	13	.021761	4.6672	1.762	1.786
Iowa	2,757,537	9	.011564	4.1936	1.581	1.596
Kansas	2,178,611	7	.0079966	3.6705	1.385	1.392
Kentucky	3,038,156	9	.012143	3.9968	1.509	1.521
Louisiana	3,257,022	10	.013967	4.2882	1.619	1.635
Maine	969,265	4	.0030605	3.1575	1.192	1.186
Maryland	3,100,689	10	.013609	4.3890	1.657	1.675
Massachusetts	5,148,578	14	.024652	4.7881	1.808	1.834
Michigan	7,823,194	21	.046039	5.8849	2.223	2.262
Minnesota	3,413,864	10	.014300	4.1888	1.581	1.597
Mississippi	2,178,141	7	.0079966	3.6712	1.385	1.392
Missouri	4,319,813	12	.019273	4.4615	1.683	1.710
Montana	674,767	4	.0025455	3.7724	1.423	1.421
Nebraska	1,411,330	5	.0045350	3.2133	1.211	1.231
Nevada	285,278	3	.0012467	4.3701	1.649	1.636
New Hampshire	606,921	4	.0024238	3.9936	1.506	1.499
New Jersey	6,066,782	17	.032595	5.3727	2.026	2.063
New Mexico	951,023	4	.0030342	3.1905	1.204	1.197
New York	16,782,304	43	.146181	8.7104	3.287	3.312
North Carolina	4,556,155	13	.021538	4.7272	1.781	1.807
North Dakota	632,446	4	.0024692	3.9024	1.472	1.468
Ohio	9,706,397	26	.064013	6.5949	2.487	2.539
Oklahoma	2,328,284	8	.0094188	4.0454	1.528	1.541
Oregon	1,768,687	6	.0061779	3.4929	1.317	1.321
Pennsylvania	11,319,366	29	.078088	6.8986	2.604	2.638
Rhode Island	859,488	4	.0028828	3.3541	1.264	1.259
South Carolina	2,382,594	8	.0095422	4.0050	1.509	1.524
South Dakota	680,514	4	.0025588	3.7601	1.419	1.415
Tennessee	3,567,089	11	.016093	4.5115	1.702	1.721
Texas	9,579,677	25	.061003	6.3680	2.404	2.452
Utah	890,627	4	.0029358	3.2963	1.245	1.237
Vermont	389,881	3	.0014597	3.7440	1.415	1.400
Virginia	3,966,949	12	.018454	4.6519	1.755	1.784
Washington	2,853,214	9	.011754	4.1196	1.555	1.569
West Virginia	1,860,421	7	.0073532	3.9524	1.491	1.506
Wisconsin	3,951,777	12	.018415	4.6599	1.758	1.788
Wyoming	330,066	3	.0013411	4.0631	1.532	1.521

Table 2. (Continued)

	Population $m_i$	Electoral Votes $w_i$	Total Value $\Psi_i$	Value Per Inh. $\Psi_i/m_i$ ( $\times 10^{-9}$ )	Ratio $v_i/v_{d.c.}$	Banzhaf Ratio $\beta_i$
1970						
Alabama	3,444,165	9	.012003	3.4849	1.406	1.421
Alaska	302,173	3	.001186	3.9253	1.584	1.582
Arizona	1,772,482	6	.005740	3.2385	1.307	1.314
Arkansas	1,923,295	6	.005977	3.1079	1.254	1.261
California	19,953,134	45	.156584	7.8476	3.166	3.177
Colorado	2,207,259	7	.007474	3.3861	1.366	1.376
Connecticut	3,032,217	9	.011271	3.7171	1.500	1.515
Delaware	548,104	3	.001597	2.9131	1.175	1.175
Dist. of Columbia	756,510	3	.001875	2.4783	1.000	1.000
Florida	6,789,443	17	.032070	4.7326	1.906	1.941
Georgia	4,589,575	12	.018515	4.0341	1.628	1.651
Hawaii	769,913	4	.002521	3.2804	1.324	1.325
Idaho	713,008	4	.002428	3.4073	1.375	1.376
Illinois	11,113,976	26	.063718	5.7332	2.313	2.361
Indiana	5,193,669	13	.021349	4.1106	1.659	1.684
Iowa	2,825,041	8	.009662	3.4202	1.380	1.392
Kansas	2,249,071	7	.007544	3.3542	1.353	1.363
Kentucky	3,219,311	9	.011609	3.6062	1.455	1.470
Louisiana	3,643,180	10	.013730	3.7686	1.521	1.538
Maine	993,663	4	.002863	2.8860	1.164	1.166
Maryland	3,922,399	10	.014238	3.6300	1.464	1.482
Massachusetts	5,689,170	14	.024084	4.2333	1.708	1.736
Michigan	8,875,083	21	.045549	5.1322	2.071	2.113
Minnesota	3,805,069	10	.014027	3.6864	1.487	1.505
Mississippi	2,216,912	7	.007490	3.3787	1.363	1.373
Missouri	4,677,399	12	.018688	3.9954	1.612	1.635
Montana	694,409	4	.002397	3.4516	1.393	1.394
Nebraska	1,483,791	5	.004375	2.9488	1.190	1.194
Nevada	488,738	3	.001508	3.0854	1.245	1.244
New Hampshire	737,681	4	.002470	3.3486	1.351	1.353
New Jersey	7,168,164	17	.032935	4.5946	1.854	1.889
New Mexico	1,016,000	4	.002897	2.8516	1.151	1.153
New York	18,190,740	41	.134168	7.3756	2.976	3.004
North Carolina	5,082,059	13	.021122	4.1563	1.677	1.702
North Dakota	617,761	3	.001695	2.7435	1.107	1.107
Ohio	10,652,017	25	.059855	5.6191	2.267	2.314
Oklahoma	2,559,253	7	.008042	3.1423	1.268	1.278
Oregon	2,091,385	5	.006231	2.9793	1.202	1.209
Pennsylvania	11,793,909	27	.068299	5.7910	2.337	2.385
Rhode Island	949,723	4	.002802	2.9498	1.190	1.192
South Carolina	2,590,516	8	.009257	3.5734	1.442	1.454
South Dakota	666,257	4	.002347	3.5260	1.423	1.424
Tennessee	3,924,164	10	.014242	3.6292	1.464	1.482
Texas	11,196,730	26	.063950	5.7115	2.305	2.352
Utah	1,059,273	4	.002958	2.7924	1.127	1.129
Vermont	444,732	3	.001438	3.2362	1.306	1.305
Virginia	4,648,494	12	.018631	4.0080	1.617	1.640
Washington	3,409,169	9	.011942	3.5030	1.413	1.428
West Virginia	1,744,237	6	.005695	3.2648	1.317	1.324
Wisconsin	4,417,933	11	.016631	3.7645	1.519	1.539
Wyoming	332,416	3	.001244	3.7423	1.510	1.509

$$4.1 \quad h = f \circ (g_1, \dots, g_{s1})$$

is the composite function. (See Owen<sup>6</sup> for details of this.)

Now, the MLE  $f$  was discussed in section 3 above. To construct the MLE  $g_j$ , we note that  $g_j(x_1, \dots, x_{m_j})$  is the probability that the coalition  $S$  will have more than  $m_j/2$  members, given that the several players have probability  $x_1, x_2, \dots, x_{m_j}$  of belonging, and that these are independent. We are interested, however, only in values of  $g_j$  along the "diagonal"  $x_1 = \dots = x_{m_j} = t$ , and there we find that

$$4.2 \quad g_j(t, t, \dots, t) = \text{Prob} \left\{ Z > \frac{m_j}{2} \right\},$$

where  $Z$  is a binomial random variable with parameters  $m_j$  and  $t$ . This can be approximated (with truly negligible error) by a normal variable with mean  $tm_j$  and variance  $t(1-t)m_j$ . Thus,

$$4.3 \quad g_j(t, t, \dots, t) \cong \Phi \left[ \left( t - \frac{1}{2} \right) \sqrt{\frac{m_j}{t(1-t)}} \right],$$

where  $\Phi$  is the cumulative normal distribution function.

Now, as is shown in Owen,<sup>7</sup> the value to all voters in the  $i$ -th state together is given by

$$4.4 \quad \Psi_i = \int_0^1 f_i(x_1, x_2, \dots, x_{s1}) \frac{dx_i}{dt} dt,$$

where the integration is taken over the path

$$4.5 \quad \begin{aligned} x_j(t) &= g_j(t, \dots, t) \\ &= \Phi \left[ \left( t - \frac{1}{2} \right) \sqrt{\frac{m_j}{t(1-t)}} \right]. \end{aligned}$$

For the sake of simplicity, it is easier to change the parameter from  $t$  to  $T$ , where

$$4.6 \quad T = \frac{t - \frac{1}{2}}{\sqrt{t(1-t)}}.$$

Then  $T$  varies from  $-\infty$  to  $+\infty$  as  $t$  goes from 0 to 1. Thus we will also have

$$4.7 \quad \Psi_i = \int_{-\infty}^{+\infty} f_i(x_1, \dots, x_n) \frac{dx_i}{dT} dT,$$

where the path of integration is

$$4.8 \quad x_j(T) = \Phi(T\sqrt{m_j}).$$

<sup>6</sup> Owen, "Multilinear Extensions."

<sup>7</sup> Owen, "Multilinear Extensions."

It is now possible to approximate  $f_i(x_1, \dots, x_n)$  as in section 3 above; we apply the formulae (3.5), (3.6) and (3.7), as described there. Note that, although (4.7) is formally an improper integral, it is in practice possible to neglect all values of  $T$  outside some small interval so that only a finite integration is necessary. Once  $\Psi_i$ , the value to all voters in the  $i$ -th state, has been computed, this can be divided by  $m_i$  to obtain the value to each individual voter from that state (clearly all voters in the same state have equal powers).

A certain difficulty arises here; namely, that we do not know  $m_i$ , the number of voters. We know the population, and have an assumption that  $m_i$  is proportional to it, but we lack a constant of proportionality. Since, however, we are not so much interested in the actual power of each voter, but only in the disparity of power between voters from different states, the problem is not a serious one. We shall use the population figures for  $m_i$ ; the eventual power obtained per individual can then be divided by the power per individual in the most disadvantaged state (really, the District of Columbia) to obtain power ratios.

Table 2 shows the results of these computations, together with a further approximation obtained by dividing  $\mathcal{G}_i$  (as given in Table 1) by  $\sqrt{m_i}$ . (This is, in effect, the Banzhaf ratio.)

As in any approximative method, one should worry somewhat about the errors introduced by the approximation. Briefly speaking, there are three sources of error here: (a) the error introduced by the approximation in formula (4.3); (b) the error introduced by the approximation in formula (3.7); (c) the error arising from numerical integration.

Of these three, (a) can safely be neglected;  $m_i$  is clearly large enough so that the normal approximation to the binomial will be extremely accurate. Similarly, (c) can be made very small if the steps of integration are small enough, as indeed they are here. The main source of error, then, is (b). It is difficult to estimate this error a priori; we might, however, expect that the error here will be approximately the same as that obtained for the state game (section 3). Comparison of the exact figures with the MLE approximations, as seen in Table 1, suggests that the error here is not large either. Thus we feel these results are reasonably accurate.

## 5. Conclusions

Generally speaking, four factors seem to contribute to the disparity of power in presidential elections. Two of these, of course, have always been known. The others have been more or less suspected, but it is only with game-theoretic analysis that they can be measured.

a. The first factor is what might be called the



integrity factor, which is best explained by saying that the number of electors from each state must be an integer. The obvious consequence is that there may be states with sizable differences of population but the same number of electors, e.g., Alaska with 302,173 inhabitants and North Dakota with 617,761. Both have one U.S. Representative, and hence both have 3 electors. This factor clearly benefits the voters from states whose population is low among those with equal numbers of U.S. Representatives.

- b. The second factor might be called the Senate factor, and is explained by saying that the number of electors is equal to a state's total congressional representation. Now the state's delegation to the House is (more or less) proportional to population, but all states have two senators. Thus, this factor tends to favor states with low population.
- c. The third factor, which was first noticed by Shapley and Mann (see note 5), might be called the unit rule factor. Essentially, we see in Table 1 that, if the values to the states in the state game are multiplied by 538, the power for the larger states is slightly more than their electoral vote; that for the smaller states is slightly less than their electoral vote. This factor favors the large states.
- d. The fourth factor can best be described as the composition factor. This can best be seen mathematically from formula (4.7), where the factor  $dx_i/dT$  appears. From (4.8), we will have

$$5.1 \quad \frac{dx_i}{dT} = \sqrt{\frac{m_j}{2\pi}} e^{-\frac{1}{2}m_j T^2},$$

which, for small values of  $T$ , is roughly proportional to  $\sqrt{m_j}$ . (For larger values of  $T$ , of course, the exponential term decreases rapidly as  $m_j$  increases.) Since most of the integral (4.7) will be concentrated near  $T=0$ , it follows that there will be an additional factor, not quite equal to  $\sqrt{m_j}$ , favoring the larger states.

Of these four factors, it is clear that (a) and (b) were expected by the original framers of the Constitution. Factor (a) was probably considered

minor, a necessary evil to contend with, while (b) was part of a compromise which made the smaller states more willing to join the union. The other two factors, however, are probably quite unexpected. Factor (c) was certainly not expected by the Constitution's framers, since the present method of pledged electors was not then foreseen; in any case it is minor, as Table 1 shows. The strongest factor, from Table 2, is clearly factor (d). Somehow, people have been aware of this factor's existence, as evidenced by the traditional rule which says that the more populous states are usually "swing" states. It is only with game-theoretic analysis, however, that the extent of this factor has been measured, first by Banzhaf,<sup>8</sup> and now in this paper.

In closing, it might be worthwhile to comment on a recent controversy over allocation of resources in a presidential election.

In one paper<sup>9</sup>, Brams and Davis recommend a "3/2 power rule" for allocation, and suggest that this is empirically tested by strategies actually used in the last four presidential elections. In another paper<sup>10</sup>, Colantoni, Levesque and Ordeshook disagree.

It should be pointed out that our model is quite different, treating the election as an  $n$ -person game among the voters, while the authors in both the above papers treat an election as a two-person game between the candidates. Moreover, it is dangerous to try to find empirical validation of a non-obvious and complicated mathematical model. Nonetheless, it is interesting to note that the Shapley value as here obtained is very similar to the allocation recommended by Brams and Davis.

<sup>8</sup> Banzhaf, "One Man, 3.312 Votes."

<sup>9</sup> Steven J. Brams and Morton D. Davis, "The 3/2's Rule in Presidential Campaigning," *American Political Science Review*, 68 (1974), 113-134.

<sup>10</sup> Claude S. Colantoni, Terrence J. Levesque, and Peter C. Ordeshook, "Campaign Resource Allocations Under the Electoral College," *American Political Science Review*, 69 (1975), 141-154; followed by a "Comment" from Brams and Davis, 155-156, and a "Rejoinder" by Colantoni et al, 157-161.

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# Vote Trading and the Voting Paradox: A Proof of Logical Equivalence\*

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In a recent paper concerned with legislative vote trading, Riker and Brams demonstrated "... the paradox of logrolling: that rational trades by all members [may] make everyone worse off."<sup>1</sup> In discussing this effect the authors note that "There seems ... to be an inherent disequilibrium in vote trading which is occasioned by the assumption of individual rationality."<sup>2</sup> This instability associated with vote trading has been noted by a number of others. R. E. Park, considering the set of possible legislative outcomes, proved a theorem which states:

If the introduction of vote trading is 'beneficial' in the sense that some majority set can, with vote trading, improve the payoffs to all of its members over those without vote trading, there is no stable outcome with vote trading."<sup>3</sup>

Tullock has also indicated that instability exists at least in the case of "implicit" logrolling (formation by candidates or parties of platforms: "... complex mixes of different measures").<sup>4</sup> He notes, however, that "explicit" logrolling, ordinary exchanges of votes among legislators, is stable.<sup>5</sup> Further, both Tullock and Coleman in different articles have indicated that logrolling, by taking intensities of preference into account, eliminates irrationality of the sort described by Arrow.<sup>6</sup>

In this paper I begin with an example of a vote-trading situation taken from Riker and Brams

\* I wish to extend my gratitude to the many individuals who read and commented on this work. Among them I owe a particular debt to Professor Steven J. Brams for his detailed criticism of several drafts and his continued encouragement.

<sup>1</sup> William H. Riker and Steven J. Brams, "The Paradox of Vote Trading," *American Political Science Review*, 67 (December, 1973), 1241.

<sup>2</sup> Riker and Brams, p. 1246.

<sup>3</sup> R. E. Park, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, 57 (December, 1967), 1304.

<sup>4</sup> Gordon Tullock, "A Simple Algebraic Logrolling Model," *American Economic Review*, 60 (June, 1970), 422.

<sup>5</sup> *Ibid.*, p. 424.

<sup>6</sup> Tullock makes the point in an appendix to James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan, 1962), p. 332, as does James Coleman, "The Possibility of a Social Welfare Function," *American Economic Review*, 56 (1966), 1105-22. These remarks are addressed to Kenneth J. Arrow, *Social Choice and Individual Values*, Cowles Foundation Monograph No. 12, 2nd ed. (New York: Wiley, 1965), pp. 1-33.

and demonstrate that an endless sequence of trades can be expected under conditions of individual rationality. Then the utility numbers are replaced with ordinal notation, and a proof is given which demonstrates that the individual preference patterns required for vote trading are necessary and sufficient conditions for the voting paradox: that vote trading and the voting paradox are logically equivalent.<sup>7</sup> Following this result, a few concluding remarks are directed to the previous considerations of logrolling.

## The Instability of Vote Trading

Riker and Brams have constructed a model which specifies the conditions which must exist if rational individuals are to exchange votes. These may be summarized briefly as follows:

- (1) "... the traders must be on opposite sides on two motions."
- (2) Each trader must be in a majority on one motion and a minority on the other.
- (3) Each trader's salience must be higher on his minority motion than his majority motion.
- (4) Each trader must be pivotal in his majority.<sup>8</sup>

These conditions must be met if trading is to

<sup>7</sup> Since the completion of this work, several other papers dealing with the subject have come to my attention. Peter Bernholz, "Logrolling, Arrow Paradox and Cyclical Majorities," *Public Choice*, 16 (Summer, 1973), 87-102 and "Logrolling, Arrow Paradox and Decision Rules: A Generalization," *Kyklos*, 27 (November, 1973), 49-62, has demonstrated that vote trading implies the voting paradox. He does not prove that they are logically equivalent. Also Joseph Kadane, "On Division of the Question," *Public Choice*, 13 (Fall, 1972) 47-54, and Nicholas R. Miller, "Logrolling and the Arrow Paradox: A Note," unpublished manuscript (December, 1973) have addressed the equivalence by considering the situation where there is no undominated platform confronting a set of voters. This implies that members will have an incentive to trade votes, and that a cyclical majority is present.

Joe A. Oppenheimer, in "Relating Coalitions of Minorities to the Voters' Paradox or Putting the Fly in the Democratic Pie," a paper delivered at the Annual Meeting at the South West Political Science Association Meeting, San Antonio, Texas, March 30-April 1, 1972, has shown that in legislatures or elections "an efficacious coalition of minorities can exist if, and only if, there is an underlying voters' paradox" (p. 5). Oppenheimer's logic is similar to that presented here; the structure of the two arguments, however, is quite different. If the voting paradox is equivalent to both the "coalition of minorities" and logrolling, it follows logically that these two are one and the same.

<sup>8</sup> Riker and Brams, pp. 1236-40.

Table 1. Utilities of Members for Motions on X and Y

Member	Preferred Outcome	Motion x		Preferred Outcome	Motion y	
		$u(x)$	$u(\bar{x})$		$u(y)$	$u(\bar{y})$
$m_1$	$x$	1	-2	$y$	1	-2
$m_2$	$x$	1	-1	$\bar{y}$	-2	2
$m_3$	$\bar{x}$	-2	2	$y$	1	-1

occur, and they form the basis for several examples constructed by the authors to illustrate the process. The examples provide the starting point for this analysis, and though they are limited to a three member legislature confronting six issues, or for purposes of this paper only two issues, it is anticipated that they are generalizable. As Riker and Brams point out, "... the three member example can, of course, be embedded in any legislature; and of course, the three members can be three factions."<sup>9</sup>

Consider a legislature with three members ( $m_1, m_2, m_3$ ) deciding two issues ( $X$  and  $Y$ ). The situation is summarized in Table 1 where the numbers represent utilities which individual members attribute to the possible collective choices on motions  $x$  and  $y$ .<sup>10</sup> These have obviously been chosen in such a fashion that the members will be able to make individually beneficial exchanges of votes; as such, they are consistent with the four conditions for vote trading stated above.

The salience,  $s$ , for member  $i$  on issue  $X$  is defined as the absolute utility difference between the possible outcomes  $x$  and  $\bar{x}$ .<sup>11</sup> For example, the salience for member  $m_2$  is

$$s_2(x) = |u_2(x) - u_2(\bar{x})| = |1 - (-1)| = 2$$

The same calculation for issue  $Y$  gives  $s_2(y)=4$  and illustrates the meaning of "higher" salience referred to in Condition 3. The voting outcomes include the combinations of possible decisions on individual motions and may be given as follows:  $\{x \wedge y, x \wedge \bar{y}, \bar{x} \wedge y, \bar{x} \wedge \bar{y}\}$ . The listing implies that the motions are considered jointly by the members. This is intended since it is clearly required for a rational exchange of votes.

Consider first the outcome resulting from sincere or naive voting; that is, "... voting in accord with one's true tastes ..." on each successive motion.<sup>12</sup> From the utilities or preferences of each member (Table 1) it is apparent that in the absence of vote trading the outcome will be  $x \wedge y$

( $x$  is preferred to  $\bar{x}$  by  $m_1$  and  $m_2$ , and  $y$  to  $\bar{y}$  by  $m_1$  and  $m_3$ ). The members' utilities for  $x, y$  and  $x \wedge y$  are given in Section A of Table 2, which also lists all the other possible outcomes and which illustrates the process of trading and its associated instability.

Now when  $m_2$  and  $m_3$  are confronted with the prospective outcome  $x \wedge y$ , vote-trading models assert that they will realize that an exchange of votes will be mutually beneficial. Member  $m_2$  will offer to vote against his own interest on his less salient issue  $X$  ( $s_2(x)=2, s_2(y)=4$ ) thereby insuring  $m_3$  a favorable outcome  $\bar{x}$  on his more salient issue ( $s_3(x)=4, s_3(y)=2$ ) if in return  $m_3$  will vote against his own interest on his less salient issue  $Y$  thereby insuring  $m_2$  a favorable outcome  $\bar{y}$  on his more salient issue. If agreement is reached, a new outcome,  $\bar{x} \wedge \bar{y}$ , is expected or actually realized if the vote is taken.<sup>13</sup> This is shown in Section B of Table 2 where the members' payoffs are given for  $\bar{x} \wedge \bar{y}$ . The column headed "total  $u$ " gives the sum of the component utilities, while " $\Delta u$ " refers to the change in total utility from the previous "naive" state. Note that in Section B both  $m_2$  and  $m_3$  have experienced a positive increase in total utility relative to the naive voting outcome  $x \wedge y$ .

For purposes of this example, assume that the vote is not taken—that the members after the first trade are confronted by a new *potential* outcome  $\bar{x} \wedge \bar{y}$ . In this case, one of two further exchanges may take place:  $m_1$  will either approach  $m_2$  or  $m_3$ . It does not matter which occurs first since either may make a mutually beneficial agreement with  $m_1$ . Arbitrarily beginning with  $m_2$ , one can see that it is rational for  $m_1$  to switch his vote from  $y$  to  $\bar{y}$ , which  $m_2$  favors, if  $m_2$  will break his agreement with  $m_3$  and change his vote from  $\bar{x}$  to  $x$ , which both  $m_1$  and  $m_2$  prefer.<sup>14</sup> The result is the

<sup>13</sup> The primary concern is not the actual occurrence of a particular outcome following a trade. The important point is that if either votes or intentions to vote are exchanged, there is no equilibrium.

<sup>14</sup> This trade sounds somewhat strange since ordinarily one member does not promise another a vote on an issue which the latter has already won. What this does, of course, is enable  $m_2$  to break his previous agreement with  $m_3$ , who has promised to vote  $\bar{y}$ , and be assured that  $\bar{y}$  still wins. The reason for using this device is that it allows the number of issues in the example to be kept to a minimum.

<sup>9</sup> *Ibid.*, p. 1241.

<sup>10</sup> Except for minor notational differences this is taken directly from Table 5, Riker and Brams, p. 1241.

<sup>11</sup> This concept is developed in some detail in Riker and Brams, p. 1239.

<sup>12</sup> Riker and Brams, p. 1237.

Table 2. The Trading Cycle

## A. Naive voting

	Members' Utilities for Outcome $x \wedge y$			$\Delta u$
	$u(x)$	$u(y)$	Total $u$	
$m_1$	1	1	2	
$m_2$	1	-2	-1	
$m_3$	-2	1	-1	

B. Trade between  $m_2$  and  $m_3$ 

	Members' Utilities for Outcome $\bar{x} \wedge \bar{y}$			$\Delta u$
	$u(\bar{x})$	$u(\bar{y})$	Total $u$	
$m_1$	-2	-2	-4	-6
$m_2$	-1	2	1	2
$m_3$	2	-1	1	2

C. Trade between  $m_1$  and  $m_2$ 

	Members' Utilities for Outcome $x \wedge \bar{y}$			$\Delta u$
	$u(x)$	$u(\bar{y})$	Total $u$	
$m_1$	1	-2	-1	3
$m_2$	1	2	3	2
$m_3$	-2	-1	-3	-4

D. Trade between  $m_1$  and  $m_3$ 

	Members' Utilities for Outcome $x \wedge y$			$\Delta u$
	$u(x)$	$u(y)$	Total $u$	
$m_1$	1	1	2	3
$m_2$	1	-2	-1	-4
$m_3$	-2	1	-1	2

potential outcome  $x \wedge \bar{y}$ , with both  $m_1$  and  $m_2$  unambiguously better off (see Section C, Table 2, where both  $m_1$  and  $m_2$  experience a positive  $\Delta u$  over the previous state).

The next step is clear:  $m_3$  approaches  $m_1$  with a promise to switch his vote from  $\bar{x}$  to  $x$ , which  $m_1$  favors, if  $m_1$  will break his agreement with  $m_2$  and change his vote from  $\bar{y}$  to  $y$ , which both  $m_1$  and  $m_3$  prefer to  $\bar{y}$ . Since it is rational for  $m_1$  and  $m_3$  to make such a trade, the outcome will be  $x \wedge y$ , which is the same as would occur with naive voting. Now the process is ready to begin again.<sup>15</sup>

The question of the effect of broken agreements on continued vote trading is not a primary concern given the perspective of this paper. The matter is discussed by Dennis C. Mueller, "The Possibility of a Social Welfare Function: Comment," *American Economic Review*, 57 (December, 1967), 1304-11, and by James Coleman, "The Possibility of a Social Welfare Function: Reply," *American Economic Review*, 57 (December, 1967), 1311-17 in a response to R. E. Park, "Comment."

<sup>15</sup> The purpose of this argument is not to suggest that whenever votes are exchanged in the real world a frantic process of cycling commences. Rather it is to show that when votes can be exchanged, instability follows logically, whether or not it takes place in practice.

While this example illustrates the cyclical nature of vote trading, it does not establish that cycling is logically associated with vote trading; the utility numbers could constitute a special case. To eliminate this possibility, the members' preferences can be expressed in ordinal-utility terms subject only to such constraints as are required for individually rational trades to occur (i.e. the preferences are consistent with the four conditions stated above).

Consider a situation comparable to that portrayed in Table 1 where three members are deciding issues  $X$  and  $Y$ . There is for each member an ordinal preference on each issue and a salience relation between the issues (indifference is not of concern since vote trading implies that the members express preferences on each motion). If, for example,  $m_1$  preferred  $x$  to  $\bar{x}$  it would be written  $u_1(x) > u_1(\bar{x})$ ; if motion  $y$  relative to  $\bar{y}$  were more important to him than motion  $x$  relative to  $\bar{x}$ , it would be written  $s_1(y) > s_1(x)$ . In setting forth the members' preferences as required for vote trading,  $m_1$  is designated a trader and a set of preference relations is arbitrarily selected for him. Following that, all of the remaining preferences for the second trader ( $m_2$ ) and the third member (who is the innocent bystander) are fixed by the vote-trading Conditions 1-4. There are of course eight possible combinations of preference and salience relations for  $m_1$ ; however, the different possibilities simply amount to differences in labeling, and the deductions which follow from one configuration are true for all the others.

Assume that trader  $m_1$  has the following preferences:

$$u_1(x) > u_1(\bar{x}) \text{—given,}$$

$$u_1(y) > u_1(\bar{y}) \text{—given,}$$

$$s_1(y) > s_1(x) \text{—given.}$$

Then if trade is to take place, the second trader,  $m_2$ , must have a complimentary set of preferences:

$$u_2(\bar{x}) > u_2(x) \text{—Condition 1,}$$

$$u_2(\bar{y}) > u_2(y) \text{—Condition 1,}$$

$$s_2(x) > s_2(y) \text{—Condition 3.}$$

Further, the bystander,  $m_3$ , plays a crucial role in filling out the appropriate majorities:

$$u_3(x) > u_3(\bar{x}) \text{—Conditions 2 and 3,}$$

$$u_3(\bar{y}) > u_3(y) \text{—Conditions 2 and 3,}$$

$$s_3(x) = s_3(y) \text{—arbitrary.}$$

(For  $m_3$ , the relative salience of  $x$  and  $y$  has no effect on the steps which follow. By setting them

equal it is possible to make the most general statement of the argument.)

Once the members' preference and salience relations are established, one may determine the preference orderings for each over the set of outcomes:

Member 1 (Trader)

1.  $u_1(x) > u_1(\bar{x}), u_1(y) > u_1(\bar{y})$  and  $s_1(y) > s_1(x)$   
 $\rightarrow u_1(x \wedge y) > u_1(\bar{x} \wedge y) > u_1(x \wedge \bar{y}) > u_1(\bar{x} \wedge \bar{y})$

Member 2 (Trader)

2.  $u_2(\bar{x}) > u_2(x), u_2(\bar{y}) > u_2(y)$  and  $s_2(x) > s_2(y)$   
 $\rightarrow u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y) > u_2(x \wedge \bar{y}) > u_2(x \wedge y)$

Member 3 (Bystander)

3.  $u_3(x) > u_3(\bar{x}), u_3(\bar{y}) > u_3(y)$  and  $s_3(y) = s_3(x)$   
 $\rightarrow u_3(x \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$   
or

$$u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge y)$$

Implications 1-3 state the individual orderings over the outcome set (proofs are given in the Appendix). Since there are only four possible outcomes, the notation can be simplified before demonstrating the existence of cyclical majorities: Let

$$\begin{aligned} A &= u_i(x \wedge y) \\ B &= u_i(x \wedge \bar{y}) \\ C &= u_i(\bar{x} \wedge y) \\ D &= u_i(\bar{x} \wedge \bar{y}) \end{aligned}$$

It may be recalled that in the numerical example, outcome  $\bar{x} \wedge y$  was not involved. This is because, after the first trade,  $m_1$  was arbitrarily selected to approach  $m_2$  before  $m_3$  (see Section C of Table 2). Had the opposite choice been made,  $\bar{x} \wedge y$  would have appeared in the cycle instead of  $x \wedge \bar{y}$ ; however, in any complete trading cycle, only one of the two will be present. In the ordinal utility example just above, either  $x \wedge y$  or  $\bar{x} \wedge \bar{y}$  will be present and these correspond to  $A$  and  $D$  in the simplified notation. Thus from Implications 1-3 the members' preference orderings are listed and the resulting intransitivities demonstrated (Table 3), dealing first with one ( $D$ ) and then the other ( $A$ ). Next for the sake of completeness, intransitivity is shown to exist over the entire set of outcomes. It is assumed that all pair-wise combinations of outcomes are decided by majority rule.

In considering all four outcomes, there are two possible cases:  $m_3$  may either express a preference

Table 3. Social Choice Under Vote-Trading Conditions

Preference Orders Excluding $A$	Preference Orders Excluding $D$
$m_1 C > B > D$	$m_1 A > C > B$
$m_2 D > C > B$	$m_2 C > B > A$
$m_3 B > D > C$	$m_3 B > A > C$
Social Choice: $C > B > D > C$	Social Choice: $A > C > B > A$

for  $A$  as compared to  $D$  or vice versa. (In effect, the equal salience assumption for  $m_3$ , which leads to his indifference between  $A$  and  $D$  as expressed in Implication 3, is now discarded.) Table 4 presents both possibilities and demonstrates that the instability is unaffected.

Table 4. Social Choice Under Vote-Trading Conditions

Member $m_3$ Prefers $A$ to $D$	Member $m_3$ Prefers $D$ to $A$
$m_1 A > C > B > D$	$m_1 A > C > B > D$
$m_2 D > C > B > A$	$m_2 D > C > B > A$
$m_3 B > A > D > C$	$m_3 B > D > A > C$
Social Choice: $D > C > B > A > D$	Social Choice: $B > D > A > C > B$

It has been shown that if three members meet the conditions for vote trading, the cyclical majority follows. Still, this falls short of proving the logical equivalence of the two processes since it has not been demonstrated that the cyclical majority occurs *only if* the conditions for vote trading are present. The latter statement is in fact true. It can be proved by selecting preference configurations which generate cyclical voting and showing that when these preferences exist, the conditions for trade follow.

In the set of 216 possible  $3 \times 3$  preference matrices, twelve result in the cyclical majority.<sup>18</sup> Two of these, listed in Table 5 involve different preference patterns. The remaining cyclical preference matrices are formed by the five alternative

Table 5. Cyclical Preference Orders

I	II
$m_1 S > R > T$	$S > T > R$
$m_2 T > S > R$	$T > R > S$
$m_3 R > T > S$	$R > S > T$

<sup>18</sup> The preference configurations which lead to the cyclical majority are discussed in William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, New Jersey: Prentice-Hall, 1973), pp. 94-96.

permutations of the members for each preference pattern. In this case,  $R$ ,  $S$  and  $T$  may be considered either a set of complex outcomes such as those discussed above (e.g.  $x \wedge \bar{y}$ ), or simply a set of three competing alternatives in the context commonly used to demonstrate the voting paradox. The results are applicable regardless of the interpretation. Only the first of the two matrices is considered, since the second produces the same effect. In the matrix, the notation  $R > S$  indicates that the specified member prefers  $R$  to  $S$ .

First, it may be stated without proof that orderings of the sort indicated for the three members in Table 5 are required to produce cycling. Simply, each outcome or motion must be given a different rank by each member. Any deviation from this pattern results in a majority winner.<sup>17</sup>

Next, examination of Matrix I in Table 5 indicates that the conditions for trade are fulfilled.

1. *Traders must be on opposite sides of two motions.* Arbitrarily selecting  $m_1$  and  $m_2$  as traders, the preference orders show that they are on opposite sides on  $R$  vs.  $T$  and on  $S$  vs.  $T$ .

2. *Each trader must be in a majority on one motion and a minority on the other.* Member  $m_1$  is in a majority on  $R$  vs.  $T$  and a minority on  $S$  vs.  $T$  while  $m_2$  is in a majority on  $S$  vs.  $T$  and a minority on  $R$  vs.  $T$ .

3. *Each trader's salience must be higher on his minority motion than his majority motion.* Member  $m_1$ 's salience is higher on  $S$  vs.  $T$  than  $R$  vs.  $T$ , while  $m_2$ 's salience is higher on  $R$  vs.  $T$  than  $S$  vs.  $T$ .

4. *Each trader must be pivotal in his majority.* This is implicit in Conditions 1 and 2 where there are only three members.

If the conditions for trade are met, exchanges of votes presumably follow. Examination of the members' preferences in Table 5 demonstrates that this is indeed the case. Despite the fact that the final result of the voting is in doubt due to the cyclical majority, members may at least improve their chances with respect to the individual pairwise decisions. That is,  $m_1$  would be willing to disregard his preference order, voting for  $T$  against  $R$ , if  $m_2$  will in turn vote for  $S$  against  $T$ . This results in the set of "apparent" preferences shown in Table 6 where the effect of the trade is

<sup>17</sup> Riker and Ordeshook, pp. 100-109.

Table 6. Members' Apparent Preferences After Initial Trade

$m_1$	$S > \textcircled{T > R}$
$m_2$	$\textcircled{S > T} > R$
$m_3$	$R > T > S$

Table 7. Members' Apparent Preferences After Second Trade

$m_1$	$S > R > T$
$m_2$	$T > S > R$
$m_3$	$\textcircled{T > R} > S$

indicated by the balloons. The exchange would result in a victory for  $m_1$ , on  $S$  vs.  $T$  and for  $m_2$  on  $R$  vs.  $T$ , while the collective choice would be  $S$ .

Looking back to the original orderings (the "real" preferences), one notes that both  $m_2$  and  $m_3$  prefer  $T$  to  $S$ . Therefore  $m_2$  would be willing to break his agreement with  $m_1$  and vote according to his "real" preference order if in return,  $m_3$  will vote for  $T$  against  $R$ . This leads to the set of "apparent" preferences shown in Table 7 with the trade again indicated by the balloon. Now  $T$  would be the collective choice except for the fact that, referring once more to the original preferences,  $m_1$  and  $m_3$  both prefer  $R$  to  $T$ . This time,  $m_3$  would break his agreement with  $m_2$  in return for  $m_1$ 's promise to vote for  $R$  against  $S$  and  $R$  would be the prospective winner.

Obviously the example can be carried on indefinitely. The important point, however, is that the conditions for trade follow from the preference configurations required to produce the cyclical majority. Thus the proof is completed showing that if and only if three members meet the conditions for vote trading, the cyclical majority follows. In effect, the two processes are logically equivalent.

### Conclusion

These results indicate that assertions which associate instability with logrolling are well founded. Park's theorem is clearly confirmed and extended here by elucidating the relationship between individual preferences and the cyclical social choice.

The equivalence of the two processes indicates that theorems dealing with either one should apply to the other. For example, Kenneth May has shown that consistent majority decisions are possible if members' choices are limited to a single pair of alternatives.<sup>18</sup> This result extended to vote trading yields the obvious conclusion that members confronting a single pair of alternatives will have no incentive to trade votes. Elsewhere Sen has demonstrated that if members have Value-Restricted Preferences, majority voting will

<sup>18</sup> Kenneth O. May, "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision," *Econometrica*, 20 (October, 1952), 680-684.

produce consistent social choices.<sup>19</sup> From this it may be inferred that Value-Restricted Preferences will eliminate vote trading among rational legislators. For a third example, Duncan Black has proven that if members' preferences are single-peaked, cyclical majorities will not occur.<sup>20</sup> By the equivalence demonstrated here, one may conclude that single-peaked preferences will likewise preclude the possibility of a mutually beneficial exchange of votes. While none of these theorems provides a practical means for eliminating the cyclical majority, they do elucidate the problem. The same is obviously true in respect to the elimination of vote trading.

The arguments by Tullock and Coleman asserting that logrolling may invalidate Arrow's General Possibility Theorem are somewhat less obvious. Their contention is based on the belief, encouraged by Arrow himself, that his Condition III does not admit voting systems which allow for the expression of intensities of preference, and hence, would exclude decisions based on logrolling. Plott has shown in a recent paper that this is not correct.<sup>21</sup> Further, his proper identification of relevant alternatives as those which constitute the "feasible" set means that vote trading requires changes in apparent preferences over *relevant* rather than *irrelevant* alternatives and, therefore, Condition III does not apply at all. In fact, vote trading would eliminate the paradox only if it could be shown to lead to Value-Restricted Preferences. The proof given above definitely indicates that does not occur.

A major point in Oppenheimer's paper is that the occurrence of the voting paradox is not the rarity some have suggested.<sup>22</sup> Rather than citing examples of it, however, he shows that the paradox inevitably underlies the formation of success-

ful coalitions of minorities. As a result a whole class of events may be taken as indication of the existence of potential voting irrationality. The same may be said for the equivalence demonstrated here. To the extent that vote trading is prevalent in a legislature, it is an indication that the voting paradox may be a far more ubiquitous and troublesome phenomenon than political scientists have thought heretofore.

Finally, regarding Riker and Brams, there is nothing in this work to contradict or limit the importance of the paradox of vote trading. If anything the *a priori* futility of exchanging votes is emphasized; a legislator is unequivocally trapped between the prospect of a never-ending series of trades, broken agreements and legislative uncertainty, and the prospect of even worse material losses if he refuses to participate.<sup>23</sup>

### Appendix

Purpose: To show that the individual members' preferences on issues  $X$  and  $Y$  lead to the ordering of social states given in Implications 1-3.

I. For member  $m_1$ , the givens are  $u_1(x) > u_1(\bar{x})$ ,  $u_1(y) > u_1(\bar{y})$  and  $s_1(y) > s_1(x)$ . From these it must be proved that  $u_1(x \wedge y) > u_1(\bar{x} \wedge \bar{y}) > u_1(x \wedge \bar{y}) > u_1(\bar{x} \wedge y)$ . This requires the demonstration of six preference relations: that  $u_1(x \wedge y)$  is preferred to all of the others, that  $u_1(\bar{x} \wedge \bar{y})$  is preferred to  $u_1(x \wedge \bar{y})$  and  $u_1(\bar{x} \wedge y)$ , and that  $u_1(x \wedge \bar{y})$  is preferred to  $u_1(\bar{x} \wedge y)$ . In so doing additivity is assumed; that for any motions  $p$  and  $q$ ,  $u_i(p) + u_i(q) = u_i(p \wedge q)$ .

The preference relations may be demonstrated as follows:

- a.  $u_1(x \wedge y) > u_1(\bar{x} \wedge y)$ ,  $u_1(x \wedge \bar{y})$ ,  $u_1(\bar{x} \wedge \bar{y})$ 
  - i. If  $u_1(x) > u_1(\bar{x})$ , then  $u_1(x \wedge y) > u_1(\bar{x} \wedge y)$
  - ii. If  $u_1(y) > u_1(\bar{y})$ , then  $u_1(x \wedge y) > u_1(x \wedge \bar{y})$
  - iii. If  $u_1(x) > u_1(\bar{x})$  and  $u_1(y) > u_1(\bar{y})$ , then  $u_1(x \wedge y) > u_1(\bar{x} \wedge \bar{y})$
- b.  $u_1(\bar{x} \wedge \bar{y}) > u_1(x \wedge \bar{y})$ ,  $u_1(\bar{x} \wedge y)$ 
  - iv. If  $s_1(y) > s_1(x)$ ,  $u_1(x) > u_1(\bar{x})$  and  $u_1(y) > u_1(\bar{y})$ , then  $u_1(y) - u_1(\bar{y}) > u_1(x) - u_1(\bar{x})$ , and then  $u_1(\bar{x} \wedge y) > u_1(x \wedge \bar{y})$
  - v. If  $u_1(y) > u_1(\bar{y})$ , then  $u_1(\bar{x} \wedge y) > u_1(\bar{x} \wedge \bar{y})$
- c.  $u_1(x \wedge \bar{y}) > u_1(\bar{x} \wedge \bar{y})$ 
  - vi. If  $u_1(x) > u_1(\bar{x})$ , then  $u_1(x \wedge \bar{y}) > u_1(\bar{x} \wedge \bar{y})$

Statement i-vi give the six relations required to establish  $m_1$ 's ordering of the social states.

II. For member  $m_2$ , the givens are  $u_2(\bar{x}) > u_2(x)$ ,  $u_2(\bar{y}) > u_2(y)$ , and  $s_2(x) > s_2(y)$ . From these it

<sup>19</sup> Amartya K. Sen, "A Possibility Theorem on Majority Decisions," *Econometrica*, 34 (April, 1966), 491-99, has shown that the cyclical majority can be avoided by assuming Value-Restricted Preferences: "... a set of individual preferences over a triple of alternatives such that there exist one alternative and one value with the characteristic that the alternative never has that value in any individual's preference ordering ..." This is the opposite of the condition stated above to insure cycling in the three member-three alternative case, that "... each outcome must be given a different rank by each member."

<sup>20</sup> Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958) pp. 1-51. The condition of single-peakedness assumes that all members' preferences over a set of motions can be ordered on a single dimension such that each preference function "... changes its direction at most once, from up to down."

<sup>21</sup> Charles R. Plott, "Rationality and Relevance in Social Choice Theory," Social Science Working Paper No. 5 (California Institute of Technology, August, 1971), pp. 10-13, discusses Condition III and the example from Arrow, pp. 26-28.

<sup>22</sup> Oppenheimer, "Relating Coalitions," pp. 5-6.

<sup>23</sup> Riker and Brams, p. 1242, show that the worst a member can do occurs when others trade and he refuses to join in.

must be proved that  $u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y) > u_2(x \wedge y)$ . Again six preference relations will be demonstrated.

- a.  $u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y), u_2(x \wedge \bar{y}), u_2(x \wedge y)$ 
  - i. If  $u_2(\bar{y}) > u_2(y)$ , then  $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(\bar{x} \wedge y)}$
  - ii. If  $u_2(\bar{x}) > u_2(x)$ , then  $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(x \wedge \bar{y})}$
  - iii. If  $u_2(\bar{y}) > u_2(y)$  and  $u_2(\bar{x}) > u_2(x)$ , then  $\underline{u_2(\bar{x} \wedge \bar{y}) > u_2(x \wedge y)}$
- b.  $u_2(\bar{x} \wedge y) > u_2(x \wedge \bar{y}), u_2(x \wedge y)$ 
  - iv. If  $s_2(x) > s_2(y)$ ,  $u_2(\bar{x}) > u_2(x)$  and  $u_2(\bar{y}) > u_2(y)$ , then  $\underline{u_2(\bar{x}) - u_2(x) > u_2(\bar{y}) - u_2(y)}$ , and then  $\underline{u_2(\bar{x} \wedge y) > u_2(x \wedge \bar{y})}$
  - v. If  $u_2(\bar{x}) > u_2(x)$ , then  $\underline{u_2(\bar{x} \wedge y) > u_2(x \wedge y)}$
- c.  $u_2(x \wedge \bar{y}) > u_2(x \wedge y)$ 
  - vi. If  $u_2(\bar{y}) > u_2(y)$ , then  $\underline{u_2(x \wedge \bar{y}) > u_2(x \wedge y)}$

Statements i-vi give the six relations required to establish  $m_2$ 's ordering of the social states.

III. For member  $m_3$ , the givens are  $u_3(x) > u_3(\bar{x})$ ,  $u_3(\bar{y}) > u_3(y)$  and  $s_3(y) = s_3(x)$ . From these it must be proved that  $u_3(x \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$  or  $u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y}) > u_3(x \wedge y) > u_3(\bar{x} \wedge y)$ . Since  $u_3(x \wedge y) \leq u_3(\bar{x} \wedge \bar{y})$  only five preference relations need be demonstrated.

- a.  $u_3(x \wedge \bar{y}) > u_3(x \wedge y), u_3(\bar{x} \wedge \bar{y}), u_3(\bar{x} \wedge y)$ 
  - i. If  $u_3(\bar{y}) > u_3(y)$ , then  $\underline{u_3(x \wedge \bar{y}) > u_3(x \wedge y)}$
  - ii. If  $u_3(x) > u_3(\bar{x})$ , then  $\underline{u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge \bar{y})}$
  - iii. If  $u_3(\bar{y}) > u_3(y)$  and  $u_3(x) > u_3(\bar{x})$ , then  $\underline{u_3(x \wedge \bar{y}) > u_3(\bar{x} \wedge y)}$
- b.  $u_3(x \wedge y) > u_3(\bar{x} \wedge y)$ 
  - iv. If  $u_3(x) > u_3(\bar{x})$ , then  $\underline{u_3(x \wedge y) > u_3(\bar{x} \wedge y)}$
- c.  $u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)$ 
  - v. If  $u_3(\bar{y}) > u_3(y)$ , then  $\underline{u_3(\bar{x} \wedge \bar{y}) > u_3(\bar{x} \wedge y)}$

Statements i-v give the five relations required to establish  $m_3$ 's ordering of the social states. This completes the proof.



# Logrolling and the Paradox of Voting: Are They Really Logically Equivalent? A Comment

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In his paper "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence" David Koehler<sup>1</sup> claims to have proved the equivalence of logrolling and the paradox of voting. Unfortunately, the proofs given by Koehler do not have the general validity he intends them to have. Indeed, it is easy to show that logrolling does not necessarily imply the paradox of voting, nor does the paradox of voting imply the existence of logrolling situations.

I begin by first demonstrating with the help of a counter-example that certain logrolling situations do not imply the paradox of voting. This is not very surprising, since to prove that logrolling implies the paradox, it has been necessary to assume the absence of a certain kind of complementarity among issues.<sup>2</sup> In the counter-example a group of three voters,  $V_1$ ,  $V_2$  and  $V_3$ , has to decide two issues  $x$  and  $y$ . The two issues each contain two alternatives, namely,  $x_1$ ,  $x_2$  and  $y_1$ ,  $y_2$ . In Matrix 1 the utility payoffs for voters  $V_1$ ,  $V_2$  and  $V_3$  are given for the four possible outcomes  $(x_1, y_1)$ ,  $(x_1, y_2)$ ,  $(x_2, y_1)$  and  $(x_2, y_2)$ .

Matrix 1

Moves available to all voters	$x_1$	$x_2$
$y_1$	8, 8, 8	2, 10, 2
$y_2$	6, 2, 10	10, 6, 6

With simple majority voting  $x_2$  and  $y_2$  will receive a majority of votes, so that  $(x_2, y_2)$  will be the outcome selected by the group. This result follows, because  $x_1$  is dominated by  $x_2$  for  $V_2$ , and  $y_1$  by  $y_2$  for  $V_3$ . With no knowledge about the behavior of the other voters  $V_2$  will, therefore, vote for  $x_2$  and  $V_3$  for  $y_2$ .  $V_1$  knows this situation and consequently casts his votes for  $x_2$  and  $y_2$ , since by doing so his most preferred outcome  $(x_2, y_2)$  will get a majority.

But now voters  $V_2$  and  $V_3$  prefer  $(x_1, y_1)$  to  $(x_2, y_2)$  because of the higher payoffs  $8 > 6$ . And they can obtain this outcome by making a logrolling agreement. According to this agreement  $V_2$  votes for  $x_1$  as against  $x_2$ , which is against his immediate interest, to get in exchange  $V_3$ 's vote for  $y_1$  as against  $y_2$ , which is contrary to  $V_3$ 's immediate advantage. This exchange of votes is, however, favorable to both, since  $y_1$  strongly dominates  $y_2$  for  $V_2$ , and  $x_1$  strongly dominates  $x_2$  for  $V_3$ . Consequently, a typical logrolling situation is present.

It is now quite obvious that in the case given above, logrolling does not imply the paradox of voting. For  $(x_2, y_1)$  as well as  $(x_1, y_2)$  are preferred to  $(x_1, y_1)$  by only one voter.  $(x_1, y_1)$  is a stable outcome. Note, moreover, that  $(x_2, y_2)P_1(x_2, y_1)$ ,  $(x_2, y_2)P_1(x_1, y_2)$  and  $(x_1, y_1)P_1(x_2, y_1)$ ,  $(x_1, y_1)P_1(x_1, y_2)$ , where  $P_1$  means "preferred to by  $V_1$ ." As a consequence just the kind of complementarity is present, the absence of which had to be assumed to prove that logrolling situations imply the paradox of voting.<sup>2</sup>

I proceed to show that cyclical group preferences do not generally imply the presence of logrolling situations. First, it has to be mentioned that Koehler has not given a proof for cycles containing more than three alternatives. But it is obvious that cyclical group preference orderings with more than three alternatives  $A_1$  can exist. In general, we have:

$$A_1PA_2PA_3P \cdots PA_{n-1}PA_nPA_1,$$

where  $P$  means "preferred to by a majority of the group." Koehler's proof is only concerned with the case, in which  $n=3$ , and no proof has been given for  $n>3$ .

Secondly, consider Koehler's case with  $n=3$ :

$$A_1PA_2PA_3PA_1.$$

Even in this case the paradox of voting need not imply logrolling situations. For assume that  $A_1$ ,  $A_2$  and  $A_3$  belong all to one issue  $A$ . For example, the  $A_i$  may represent three different bridges, out of which one has to be selected by the group, since only one bridge can be built because of budgetary

<sup>1</sup> David H. Koehler, "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," *American Political Science Review*, 69 (September, 1975), 954-960.

<sup>2</sup> Peter Bernholz, "Logrolling, Arrow-Paradox and Decision Rules: A Generalization," *Kyklos*, 22 (fasc. 1, 1974), 53, assumption A5.

restrictions. In this case substantive logrolling is obviously not possible, since there are no two issues concerning which votes can be exchanged by group members.

The above considerations prove not only that Koehler has failed to show the logical equivalence of logrolling and the paradox of voting but also that such a proof is impossible. What Koehler

has demonstrated—but only for three group members and for two issues—is that *under certain conditions* logrolling and the paradox of voting are equivalent. But his belief that he had given a general proof prevented him from trying to find these conditions. As mentioned above, one of these conditions is the absence of a certain kind of complementarity among the relevant issues.

# Some Political Implications of "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence:" A Comment\*

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Although the theorem implied by Koehler's "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence," (and by the accompanying exchange between Bernholz and Koehler) has been proven elsewhere<sup>1</sup> in a more general form, the political consequences of the theorem have not yet been drawn out. Before specifying these consequences, it would be worthwhile to specify the theorem in what I believe is its most general form:

*If unrestricted<sup>2</sup> vote trading is permitted with regard to a finite number of independent (i.e. separable),<sup>3</sup> M-sided issues<sup>4</sup> with any simple or special majority rule,<sup>5</sup> precisely those preferences which allow for an effective logroll,<sup>6</sup> lead to a voting cycle, or cyclic*

\* I wish to acknowledge the careful and thoughtful criticisms I have received from a number of people including Duncan Black, Norman Frohlich, Leonard Lamm, Rick Piltz, Samuel Popkin, Thomas Schwartz, Gordon Tullock, and Oran R. Young. Norman Frohlich's kind loan of his favorite 70-million voter illustration (Figure 1) is greatly appreciated.

<sup>1</sup> Peter Bernholz, "Logrolling, Arrow-Paradox and Decision Rules: A Generalization," *Kyklos* 27 (fasc. 1, 1974) 49-62, as well as an independently derived formulation: Joe A. Oppenheimer, *Relating Coalitions of Minorities to the Voters' Paradox or Putting the Fly in the Democratic Pie*, paper given at the 1972 meeting of the Southwest Political Science Association, San Antonio, Texas, March 30-April 1, 1972. The first indication of the relationship probably was that contained in Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), especially pp. 55-60 and 64-69.

<sup>2</sup> Works showing the consequences of restrictions on vote trading are John A. Ferejohn, *Sour Notes on the Theory of Vote Trading*, Social Science Working Paper, California Institute of Technology, 41 (June, 1974); Thomas Schwartz, "Collective Choice Separation of Issues, and Vote Trading," (Pittsburgh: Carnegie-Mellon, Feb., 1975) paper given at Foundations of Political Economy Conference, Austin, Texas, February 28-March 1, 1975; as well as Joseph B. Kadane, "On Division of the Question," *Public Choice*, 13 (Fall, 1972) 47-54.

<sup>3</sup> See Bernholz's "Comment," *American Political Science Review*, 69 (Sept., 1975).

<sup>4</sup> A generalized proof for *m*-sided issues (*M* finite) is in Bernholz, "Logrolling."

<sup>5</sup> The generalization to any majority rule is in Oppenheimer.

<sup>6</sup> Of course, not all vote trades are logrolls. For a definition of logrolling, see Bernholz, "Logrolling," p. 53. A most informative discussion of the restrictive consequences of this definition relative to other possible vote trades can be found in Schwartz, "Collective Choice," pp. 18-20.

*majority. Furthermore, if (in such situations) the individuals' preferences do not support a cycle then such a logrolling coalition is not efficacious.<sup>7</sup>*

This theorem has important substantive implications for political scientists,<sup>8</sup> to the extent that logrolling of non-complementary issues is a prevalent political phenomenon.

## Numerous Paths to Electoral Victory

The existence of cyclic majorities across a set of compound alternatives leads to a new critique of the description of political leaders as "seeking to win." This traditional characterization of leaders can be shown to be an underspecified definition of political rationality when there are cycles. In other words, the strategy alternatives available to politicians in cyclic situations contain numerous paths to victory. Thus, a definition of political rationality based on an assumption no more specific than "win-seeking" is not specific enough to yield definitive predictions. Furthermore, these alternative paths differ markedly from one another in terms of important political characteristics. To see how new degrees of freedom are available to politicians, and why there is need to further specify political motivation of leaders, consider the following example.

Assume an electorate of 70 million consisting of the following four disjoint subgroups of voters: 10 million blacks, 10 million farmers, 20 million labor union members, and 30 million "others," (See Table 1.) Imagine that each of the specific minority groups has one major "political concern" or issue about which its members feel passionately, i.e., they would rather win on that issue and lose on all the others than win on all the others and lose on that one issue. (Passionate feelings regarding any one issue therefore are defined relative to a larger set of issues.) In Table 1, the blacks passionately want the passage of civil rights legislation, the farmers want price supports, and the laborers want an end to "right-to-work laws." Further, assume that the members of each group are opposed to the preferred

<sup>7</sup> With special majority rules, vote trading can be undertaken to block rather than to win a vote, hence the introduction of the notion of efficacious, cf. Oppenheimer.

<sup>8</sup> Except as noted all of what follows comes directly from Oppenheimer.

Table 1. Illustration of Coalitions of Minorities: Cyclic Outcomes

Group	Size	Issue Positions		
		Price Supports for Farm Prod.	End Rt-to- Work Laws	Civil Rights
(F)—Farmers	10 million	very pro <sup>a</sup>	anti	anti
(L)—Labor	20 million	anti	very pro	anti
(B)—Blacks	10 million	anti	anti	very pro
(O)—Others	30 million	anti	anti	anti
Headcount for each issue:				
		1. farm price supports: 10 million pro vs. 60 million anti		
		2. end rt. to work laws: 50 million anti vs. 20 million pro		
		3. civil rts: 10 million pro vs. 60 million anti		

<sup>a</sup> "Very pro" means preferring to win on that issue and lose on the others than win on all the others and lose on that one issue, i.e., feeling passionately about that issue.

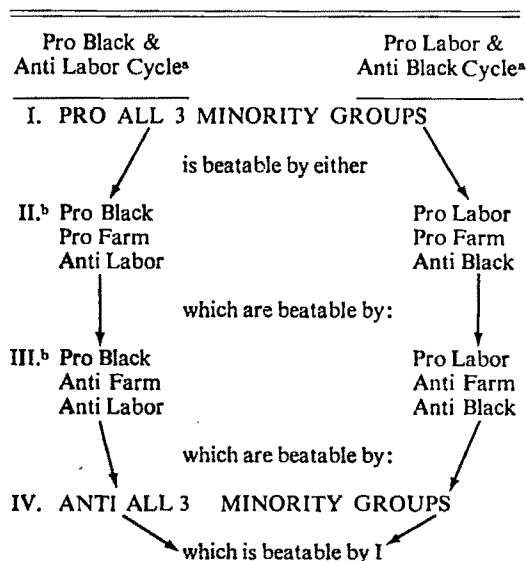
positions of the other groups. Thus, considered by itself, civil rights would fail (60 to 10), farm supports would fail (60 to 10), and pro-union legislation would fail (50 to 20).

But what would happen if coalitions formed to support multi-issued "platforms?" For example, consider the situation without coalitions. The minorities all lose, and the status quo becomes a "platform" which is opposed to all three of the identified minorities. Here the majorities on each of the issues win. (I shall designate this platform as MAJ.) On the other hand, if all of the minorities banded together and agreed to trade votes across issues to support their three passionately held issues, they could form a victorious coalition. That is, the coalition of minorities (designated as BLF) could beat the anti-minorities "platform": MAJ. But BLF could, in turn, be defeated by a platform which was in part pro, and in part anti, minorities. To illustrate, consider a platform supporting two minority positions and one majority position: pro-labor, pro-farmer, and anti-black (designated as LF). In a contest against BLF all the individuals who are anti-civil rights will support LF since LF differs from BLF only with regard to civil rights. Thus LF could garner the support of the 30 million others plus the farmers and labor. (See Table 2.)

But other winning proposals exist against BLF. A lemma by Sen which proves that any voting cycle can be reduced to cycles of three alternatives,<sup>9</sup> implies that numerous paths can be taken in order to secure victory, once one has arrived at the "pro-minority" position in the cycle. The opponents can, after all, kick out *any* one (or two) of the pro-minority planks and still win. To illustrate, consider running a pro-labor, pro-black, and anti-farmer platform (LB) against BLF. Now LB gains the support of all but the

farmers. Or imagine that a pro-labor anti-black, and anti-farmer position (L) is proposed against BLF. It too, could win. In the first illustration, MAJ lost to BLF, which lost to LF. LF could lose to L which in turn could be beaten by MAJ. In this example, a four-stage cycle was constructed. The cycle chosen would allow labor to have its interests met in three out of the four stages of the cycle. Blacks, on the other hand, would be 'satisfied' only once during the four cyclic periods. Their roles could just as well have been reversed. That is, cycles have introduced alternative paths to victory. The strategies adopted could have been

Table 2. Illustrative Alternative Logrolling Cycles



<sup>a</sup> These are merely 2 of a larger number of possible cycles.

<sup>b</sup> Either step II or III could be omitted from the cycle.

<sup>9</sup> Amartya K. Sen, "A Possibility Theorem on Minority Decisions," *Econometrica*, 34 (1966), p. 492.

"pro-black and anti-labor" with labor the first minority group "kicked out" of the coalition and blacks the longest retained. Indeed, the larger the number of minorities put together in a coalition, the longer the maximum possible cycle and the greater the divergence possible between the favored and the nonfavored groups.<sup>10</sup> Clearly, the choice of whom to "support" and whom to kick out of any coalition of minorities is arbitrarily related to the objective of winning. But it is an essential element in the strategy choice of any politician motivated by the scent of victory. To predict his choices one must further specify the goals (or constraints) governing his behavior. Thus, such secondary factors as monetary rewards, size of electoral majority, and ideology are left to play a potentially important role in strategy selection for win-oriented politicians.

### Interpreting Electoral Reversals

To the extent that electoral choices reflect voters' paradoxes, one could have electoral reversals without changes in the opinions of the electorate. That is, if the assumptions are met, the changes in electoral outcome could reflect changes in coalition structure or changes of the options facing the voters rather than changes in the opinions of the electorate.<sup>11</sup> In other words, there are many potential majorities in any political constituency, and to explain political outcomes one must deal with the strategic relations between pairs of strategies and their place in an issue cycle.

### On the Value of a Democratic System

But are the coalitions which compete in a democracy likely to be logrolling coalitions? Perhaps, given the vulnerability of coalitions of minorities, other sorts of coalitions are constructed. But if coalitions of minorities do not form the basis for issue differences between parties, along what issues *do* the competitors differentiate themselves from one another? If issues are independent, coalitions could have two purposes.

First, coalitions could be needed to obtain a divergence from the MAJ platform which would win if each issue were designated in isolation.<sup>12</sup> Thus, coalitions can form for purposes of *explicit* logrolling.<sup>13</sup> But if they are to be effective (i.e. constitute a majority) when there are no impediments

to vote trading, the package they support must contain minority positions on a set of issues and vote trades must take place between the minorities. That is, only a set of issues (call it a set of minority issues) which can support victorious coalitions of minorities will be chosen as the basis of such coalitions. Consider other issue sets. If the trades among minorities cannot be engineered to construct a majority, then we might define the set of issues as majority issues.<sup>14</sup> Assuming no costs of negotiation, effective coalition formation can not occur over these issues. A minority position on such an issue is sure to lose. If there is sufficient information to identify sets of issues as "majority" and "minority," then only those sets of issues about which one could construct coalitions of minorities will be involved in logrolling.

On the other hand, coalitions can form quite naturally out of the electoral process. That is, candidates take positions on sets of issues to attract voters, thereby implicitly constructing coalitions. This process of *implicit* logrolling can be analyzed as can that of explicit logrolling. The results are analogous. The parties will try to match on the majority issues and the party differential (in Downs's terms) will tend to consist of minority issues.

To the extent that parties are differentiated merely by minority issues, the outcome of the electoral process is *arbitrary*. More specifically, such a social choice would always be the arbitrary resultant of the order of the agenda—or of a procedure designed to break cycles arbitrarily. The stage in the cycle would determine the issues that would differentiate the parties in any particular period of time. Indeed, if only minority issues differentiated parties, then *with regard to issues that would be settled in the electoral arena, a democratic system would offer no advantages over a nondemocratic one!* That is, any benefits from democracy must occur as externalities of the electoral process. At least one of these externalities can be identified. Note that if only minority issues differentiated parties, *a democratic system leads to the settlement of majority issues by consensus*. These issues tend neither to be fought over by the parties nor raised in the political arenas. Such issues, then, are settled by being excluded from the process of competition, and the settlements are protected by the system of competition. Although these characteristics of democracy have usually not been of central concern to the democratic theorist,<sup>15</sup> it is clearly important to apply

<sup>10</sup> Perhaps it should be noted that "vote-maximizing," in contrast to "victory-seeking" politicians would tend to choose strategies which would lengthen the cycles.

<sup>11</sup> Of course, changes in the issues over time could also occur, as could the traditional explanation of changes in the preference orders of the members of the electorate.

<sup>12</sup> Kadane proves MAJ is the only winner without cycles.

<sup>13</sup> The distinction between explicit and implicit logrolling was first made by Gordon Tullock, "Some Problems of Majority Voting," *Journal of Political Economy*, 67 (December, 1959) 571-579.

<sup>14</sup> The usage parallels that of Downs in *Economic Theory*.

<sup>15</sup> An exception is Bachrach's and Baratz's concern with the "issues which are never raised," cf. Peter Bachrach and Morton Baratz, "The Two Faces of Power," *American Political Science Review*, 56 (December, 1962), 947-953.

these findings to characterize democratic processes.

For example, consider how we can use the notions of majority and minority issue sets to analyze why people vote. Majority issue sets have two characteristics which can be quite illuminating in an examination of why individuals vote. First, there must be a majority of individuals who agree as to which side of *each* issue is preferable if the issues constitute a majority set. Second, these individuals must agree on the importance of the issues: they must feel passionately about their preferences on these issues. Remember, the majority issues were those protected by the system, and not included in the party differential. Assume that a voter feels he is as likely to change an electoral outcome as to protect the issue-consensus. Then we could expect the average voter to go to the polls motivated not by the differences between the parties but by the support his vote gives to the consensus on the majority issues. This follows since majority issues would be made up of only "the most important" issues, as perceived by the majority of voters. Note that this sort of motivation might well be rationalized as a general interest in maintaining the system.

If we compare the relevant statistics regarding this in *The American Voter*,<sup>16</sup> we find preliminary corroborating evidence for the theory espoused here. In particular, 52 per cent of the voters who did not care at all about the outcome of the election, voted anyhow. But only 13 per cent of the voters who had a "low sense of citizen duty" voted. This would make sense if the "sense of citizen duty" question was actually tapping information regarding the citizen's interest in "maintaining the system." Downs discusses voting in order to "save democracy" in some detail, and we can now add a further detail in the argument he makes. If the assumptions of the argument are accurate, the utility of saving the system of democracy (for the majority of voters) would be greater than the utility of the party differential.

Furthermore, to the extent that the party differential is made up of minority issues, and to the extent that *all* issues in the group are minority issues, there will be no value to "saving the system." The further we move from consensus, the less difference will the existence of a democratic system make on the outcome of the policy choosing process.

<sup>16</sup> Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1964), pp. 57, 58, and 59.

### Implications for Further Research

Much of the theoretical work which has been done about the voters' paradox has been oriented toward identifying precisely those conditions which rule out the voters' paradox. It follows directly from the theorem that the empirical domain of assumptions which logically rule out the paradox are of limited applicability in situations involving independent issues. This should prove to be helpful in constructing models of particular political arenas. Thus, for example if one were to construct a model of the legislative process (where unrestricted logrolling of independent issues is prevalent), one would reject those assumption sets which rule out the paradox. On the other hand, politics in certain arenas and over particular issues might well be structured so no cycles occur. There one can adopt sets of assumptions (e.g., single peaked preferences) or observe certain structural features (e.g., gag rules)<sup>17</sup> which eliminate the paradox and allow for the construction of realistic models. With this in mind, one should be able to prescribe the domain for certain methodologies of analysis or models such as spatial models involving single peakedness.

Of course, the relaxation of the assumptions constituting the antecedent in the theorem will be of great interest. Thus, further work along these lines such as that by Schwartz and Ferejohn, where restrictions to vote trading are considered, will increase our ability to handle real-world political problems. For example, often when political situations are highly structured and manipulable there are restrictions to vote trading. In these situations it should be possible and helpful to include the coalitional analysis in any model of manipulation.<sup>18</sup>

In summary, the identification of the voters' paradox with efficacious logrolling between independent issues establishes a number of serious consequences for empirical and normative theory of democracy. Further work in this area could be quite important for the development of a more rigorous, relevant, and realistic theory of democracy.

<sup>17</sup> John C. Blydenberg, "The Closed Rule and the Paradox of Voting," *The Journal of Politics*, 33 (February, 1971), 57-71.

<sup>18</sup> For an example of such a model, see Charles R. Plott and Michael E. Levine, *On Using the Agenda to Influence Group Decisions: Theory, Experiments and an Application*, Social Science Working Paper, California Institute of Technology, 66 (November, 1974).

# Vote Trading and the Voting Paradox: Rejoinder

DAVID H. KOEHLER

Bernholz's comment is useful in emphasizing limits on the generality of my proof of logical equivalence of vote trading and the voting paradox. The specific limit in the case of Bernholz's first example concerns an assumption which I have made and he has not. The situation is summed up by John A. Ferejohn, who states:

The idea of . . . [separability] . . . is that the legislator's preference on any subset of bills can be unambiguously determined and does not depend on bills which have already passed. This assumption is found throughout the literature on voting trading in various guises. In particular, separability is necessarily satisfied if legislators have additive utility functions on the bills.<sup>1</sup>

In my paper, additivity is implicit in the analysis of utilities in Table 2, and it is assumed explicitly in the appendix where preference orderings over outcomes are derived from individual preferences on motions.

Bernholz's interesting example clearly violates additivity as he instead specifies complementarity among issues. Taking the utilities in his Matrix 1,  $V_1$ 's ordering of the social states is as follows:

$$(x_2, y_2)P_1(x_1, y_1)P_1(x_1, y_2)P_1(x_2, y_1).$$

From the first and third ranked states it appears that  $x_2P_1x_1$ . If additivity holds, it follows that  $(x_2, y_1)P_1(x_1, y_1)$ , but examination of his second and fourth ranks indicates that the preference relation is reversed. Hence, additivity is violated. Still, this does not detract from the example, and Bernholz's point is well taken. At the very least, the additivity assumption should have been made explicit in the text.

The second question, raised with respect to the number of alternatives considered, is answered by Sen's lemma which states:

If among a set of alternatives there is no triple such that, given the set of individual preferences, the method of majority decision gives intransitive results between them, then the method will give consistent results for the entire set of alternatives.<sup>2</sup>

Thus, excluding the possibility of ties, a cycle containing more than three alternatives may be reduced to two or more cycles each containing no more than three alternatives.

Finally Bernholz's example of voting on three

mutually exclusive alternatives demonstrates that it is possible to structure a situation such that vote trading cannot occur despite the existence of cyclical preferences. (The last example in my paper indicates that under the same conditions, an *incentive* to exchange votes still exists.) This also occurs if legislative rules, formal or informal, prohibit vote exchanges or communications among members. Likewise, it should be noted that the rules ordinarily preclude voting cycles: if the status quo wins over an amended bill, it (the status quo) is not further tested against the original bill. Thus the possible occurrence of a cycle is procedurally eliminated.<sup>3</sup>

A more interesting case is provided by Ferejohn who states a theorem:

There exists a legislature with no majority winner and in which there is an equilibrium with respect to pairwise trading.<sup>4</sup>

The proof is based on a five-member body deciding two issues. In this case a social state is denoted (1, 0) which indicates an outcome of 1 on the first issue and 0 on the second. The *additive* individual preference orders are given as follows:

Members				
1	2	3	4	5
(1, 0)	(1, 0)	(0, 1)	(1, 1)	(1, 1)
(0, 0)	(0, 0)	(0, 0)	(1, 0)	(0, 1)
(1, 1)	(1, 1)	(1, 1)	(0, 1)	(1, 0)
(0, 1)	(0, 1)	(1, 0)	(0, 0)	(0, 0)

The result of naive voting (voting on each issue alone) is (1, 1), but notice that members 1, 2 and 3 all prefer social state (0, 0) to (1, 1). Nevertheless, by limiting exchanges to pairwise trades an equilibrium prevails: no two members who may both prefer an alternative social state to (1, 1) can by *themselves* change the outcome.

At the same time that Ferejohn's proof specifies a limitation on the generality of the equivalence, it emphasizes the point I was trying to make, i.e., "... that the individual preferences required for vote trading are necessary and sufficient conditions for the voting paradox . . .," and that the logic which underlies the two processes is the same. If the restriction to pairwise trades is lifted to allow all mutually beneficial exchanges (and it was noted in the text that "... three members can be three factions"), as indicated above out-

<sup>1</sup> John A. Ferejohn, *Sour Notes on the Theory of Vote Trading*, Social Science Working Paper, California Institute of Technology, 41 (June, 1974), 8.

<sup>2</sup> Amartya K. Sen, "A Possibility Theorem on Minority Decisions," *Econometrica*, 34 (April, 1966), 492.

<sup>3</sup> William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1973), p. 112.

<sup>4</sup> Ferejohn, pp. 10-11.

come (0, 0) is preferred by three members to the naive outcome (1, 1), but also, (0, 0) is dominated by (1, 0) and (1, 0) in turn by (1, 1). If members become entangled in this unstable situation before a decision is taken, the process is vote trading, while if it occurs at the time of the vote, the result is a cyclical majority.

Oppenheimer's comment notes that others have also established the equivalence between vote trading and the voting paradox. This is true as I have indicated in the paper (footnote 7). What occurred was a rare instance in political science of relatively simultaneous and independent discovery of an unrecognized relation between two important political constructs. The independence of the several pieces of work is indicated by differences in the proofs which have been offered. I would argue that the version presented here is of value as it demonstrates the relation directly in terms of the individual preference patterns which are commonly associated with trading and with the paradox. Deciding which is "best" among these proofs is partially a question of generality, but it also depends upon which provides the most tractable basis for further development, e.g., in the analysis of voting strategy. The latter question is still to be decided, and I can only leave that to the judgment of the reader.

Considering the substance of Oppenheimer's comments, I find little to argue with. The implications of the result for the analysis of both legislative and electoral strategy are profound. However, these applications do not specify identical behavior even though they share the same logic. As a result it is important to distinguish between vote trading and the coalition of minorities.

Downs conceptualized the coalition of minorities as an electoral strategy which might be successful for an opposition party seeking to unseat an incumbent following the "majority principle."<sup>5</sup> This strategy, however, has generally not been considered the same as making voting agreements in a legislature, and in that respect, the coalition of minorities is something of a misnomer. In an election, actors with differing preferences do vote alike, but not as a result of what would ordinarily be considered coalition formation. There is no agreement among voters to coordinate their behavior and, in fact, there need be no communication whatever among the minorities. This is important since it makes little sense to formulate an electoral strategy based on the assumption that large groups of voters are sufficiently disciplined to form and enforce agreements to act as one. The reason the coalition of minorities is a feasible

electoral strategy is precisely because such collusion is not required. It is the candidates, rather than the voters, who behave strategically by manipulating the sets of issue positions which are within the scope of their control. In response, voters are not expected to "swap votes," or for that matter pursue any other type of strategic behavior. Indeed, for the coalition of minorities to be successful, the voters must honestly express their preferences with respect to the competing issue sets or platforms.

In legislative vote trading the situation is quite different, since the relatively small number of members allows for the possibility of securing agreements to vote systematically against individual preferences. In this case, the policy alternatives may remain fixed while individual votes are the subject of manipulation. In contrast to the coalition of minorities, vote trading actually approximates a coalition model; members form an agreement to adopt a single specific voting strategy as they confront known individual payoff functions.

Thus there are good reasons for failing to recognize the equivalence of vote trading and the coalition of minorities; they are applicable to different voting situations, and each prescribes behavior appropriate to one and not the other. The clue which points to equivalence emerges from the analysis of vote trading as it becomes evident that a member's willingness to switch votes depends upon joint consideration of two or more motions. This is made explicit in the salience condition which requires that a member's utility must increase in a single transaction as he exchanges a low salience winner for a high salience winner. In a sense, a platform is formed as a majority set of members agree to support each of its parts regardless of their individual preferences. The joint consideration by voters of a set of two or more issues, whether resulting from agreement among individual legislators or in response to an exogenously created platform, is the point on which vote trading and the coalition of minorities intersect.

Commenting on the political implications of the voters' paradox, Oppenheimer states that "... one could have electoral reversals without changes in the opinions of the electorate." In fact, it is unlikely that this would come as a shock to either spatial modelers or political candidates. If candidates' issue positions influence voting, this statement is true whether the paradox exists or not. The important point is that if the coalition of minorities is an efficacious strategy, there is no equilibrium: a candidate simply can find no platform which he prefers regardless of the platform chosen by his opponent. Beyond simply effecting electoral reversals, the existence of the paradox

<sup>5</sup> Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row Publishers, Inc., 1957), pp. 54-60.



presents repeated opportunities for a candidate to *improve* his chance of electoral success by altering existing positions or by cultivating new minority issues.

The unanswered question concerns the existence of electoral disequilibrium in the real world. Oppenheimer indicates that parties will match positions on "majority issue sets" and differentiate themselves only on "minority issue sets." To the extent this actually occurs, his dismal projections seem unavoidable. On the other hand, matching on majority issue sets is not always possible. Beyond the fact that party tradition and

credibility require some degree of platform consistency over time, the personal characteristics of the candidate—the image—are only subject to marginal adjustments. Thus if candidate evaluation is a dominant factor in the minds of the voters, and if personality matching is impossible, then a majority issue exists and the coalition of minorities can be ruled out as an efficacious strategy. This suggests that there may be good reason for a candidate to emphasize personality, and given the uncertainty surrounding minority issues, good reason for citizens as well to assign it substantial weight in deciding how to vote.

## COMMUNICATIONS

### Subcommittee Chairmanships in the House of Representatives

TO THE EDITOR:

Since the Legislative Reorganization Act of 1946, the role of the subcommittee structure in the decision processes of the House of Representatives has become of prime importance. Richard Fenno, Jr. has stated "When the 1946 Act reduced the number of standing committees from 48 to 19, it stimulated the growth and importance of subcommittees. This outcome has obscured the realities of committee-based influence. Analyses of the committee leadership which exclude the 123 (as of 1964) subcommittees can be but caricatures of the influence patterns in the House."<sup>1</sup> However, in spite of Fenno's indictment, analysis of the subcommittee structure of the House is still in embryo.

It is known that the primary foci of power in the subcommittee structure are the subcommittee chairmen.<sup>2</sup> Thus the assignment of subcommittee chairmanships is a key to the distribution of power in the House, and identification of possible norms in the assignment process critical to an understanding of the distribution of influence within the House.

Two such possible norms are examined here—full committee seniority and subcommittee seniority. Under the former mode, subcommittee chairmen would be those members of the respective subcommittee with the greatest period of continuous service on the parent committee, while under the latter, subcommittee chairmen would be those claiming the greatest period of continuous service on that particular subcommittee. Following the lead of Polsby, Gallaher and Rundquist,<sup>3</sup> the scope and usefulness of these definitions may be increased by introducing the concept of "compensation": A member is said to have received "compensation" if, when due a subcommittee chairmanship and denied, that member either receives or holds another position of in-

fluence, e.g., chairmanship of the full committee, chairmanship of another subcommittee, chairmanship of a different full committee, or chairmanship of a subcommittee on a different full committee. (The latter two types of compensation were extremely rare, occurring only twice in the cases studied.) Thus, in examining the assignment of subcommittee chairmen, in each instance in which the chairmanship of a subcommittee changes hands, we wish to determine (1) if the new subcommittee chairman is that member of the subcommittee with the greatest full committee seniority, or if not, whether all other members of that subcommittee with greater full committee seniority are given "compensation"; and (2) if the new subcommittee chairman is the member of the subcommittee with the greatest subcommittee seniority, or if not, whether all the other members of that subcommittee with greater subcommittee seniority are given "compensation."

These questions were employed in analyzing all changes in subcommittee chairmanship in the 80th-91st and part of the 92nd Congresses. (Except for short-lived ad hoc or special subcommittees—usually extant only one or two Congresses—or cases in which the entire subcommittee was reconstituted. The well-established special and select subcommittees of the Education and Labor Committee were included, however.) The data in Table 1 show the extent to which the above questions were answered in the affirmative.

As seen from the resulting data, of the 303 changes in subcommittee chairmanship, 300 or 99 per cent can be explained by the adoption of either the full or subcommittee seniority norm by each committee. The data also clearly indicate the norms which we may identify with each committee:

#### FULL COMMITTEE SENIORITY

Agriculture  
Armed Services  
Banking and Currency  
Education and Labor  
Foreign Affairs  
Government Operations  
Interior  
Interstate and Foreign Commerce  
Judiciary  
Merchant Marine and Fisheries  
Post Office and Civil Service  
Public Works  
Science and Astronautics  
Veterans Affairs

#### SUBCOMMITTEE SENIORITY

Appropriations  
District of Columbia  
House Administration

<sup>1</sup> Richard Fenno, Jr., "The Internal Distribution of Influence: The House" in Raymond Wolfinger, ed., *Readings on Congress* (Englewood Cliffs, New Jersey: Prentice-Hall, 1971), p. 203.

<sup>2</sup> See George Goodwin, "Subcommittees: The Miniature Legislatures of Congress," *American Political Science Review*, 56 (September 1962), 596-604; Burton L. French, "Subcommittees of Congress," *American Political Science Review*, 9 (February, 1915), 68-92; Charles Jones, "The Role of the Congressional Subcommittee," *Midwest Journal of Political Science* (November, 1962), 327-441.

<sup>3</sup> Nelson W. Polsby, Miriam Gallaher, and Barry S. Rundquist, "The Growth of the Seniority System in the House of Representatives," *American Political Science Review*, 63 (September, 1969), 787-807.

Table 1. Changes in Subcommittee Chairmanship<sup>a</sup>

Committee	Chngs.	S	S*	%	SCS	SCS*	%
Agriculture +	29	15	13	96.5 <sup>b</sup>	17	6	82.1
Appropriations +	38	25	10	92.0	29	9	100 <sup>b</sup>
Armed Services +	12	12	0	100 <sup>b</sup>	4	0	33.3
Banking and Currency +	8	5	3	100 <sup>b</sup>	5	2	87.5
Dist. of Columbia +	31	15	9	77.4	19	12	100 <sup>b</sup>
Education & Labor +	9	6	3	100 <sup>b</sup>	2	1	33.3
Foreign Affairs +	27	11	16	100 <sup>b</sup>	10	4	51.8
Gov't. Operations +	14	9	5	100 <sup>b</sup>	8	0	57.1
House Administration +	15	13	1	93.3	15	0	100 <sup>b</sup>
Interior	26	3	23	100 <sup>b</sup>	5	19	92.6
Commerce	8	8	0	100 <sup>b</sup>	1	0	12.5
Judiciary	20	17	2	95.0 <sup>b</sup>	13	1	70.0
Merchant Marine & Fisheries	17	11	5	94.1 <sup>b</sup>	11	0	64.7
Post Office & Civil Service	13	11	2	100 <sup>b</sup>	12	0	92.3
Public Works	17	9	8	100 <sup>b</sup>	8	6	82.3
Science and Astronautics	3	3	0	100 <sup>b</sup>	0	0	0
Veterans Affairs	16	12	4	100 <sup>b</sup>	13	2	93.7

<sup>a</sup> *Congressional Index* (New York: Commerce Clearing House 1947-71). Data taken from 80th-91st Congresses except for Committees with +, wherein data taken from 80th-92nd Congresses.

<sup>b</sup> Peak correlation in method of determining subcommittee chairmen.

Chngs.: Changes in subcommittee chairmanship

S: Chairmanship Determinable by Full Committee Seniority

S\*: Chairmanship Determinable by Full Committee Seniority with compensation

SCS: Chairmanship Determinable by Subcommittee Seniority

SCS\*: Chairmanship Determinable by Subcommittee Seniority with compensation

Thus we see that the dominant mode of power distribution (99 per cent) in the subcommittee structure is full committee seniority, with the occasional variation of subcommittee seniority; we must therefore modify the common view of the role of the seniority system in governing the internal distribution of power in the House, that:

The only important area in which seniority seems to play a role of overwhelming significance is in the matter of succession to the chairmanship of committees,<sup>4</sup>

by making the following addition:

On all but three of the committees of the House maintaining subcommittee structures, full committee seniority is the overwhelmingly dominant factor in the succession to the chairmanship of subcommittees, and hence, in the distribution of power within the committees themselves. On the remaining three committees, the employment of a subcommittee seniority norm decentralizes power to a limited extent, but still leaves a degree of seniority as the primary criterion for power within the committee, and consequently, within the House.

JACK A. GOLDSTONE

Harvard University

<sup>4</sup> *Ibid.*, p. 787.

### The Authentic Rousseau

TO THE EDITOR:

I would like to respond briefly to Allan Bloom's diatribe against my book, *The Politics of Authenticity*, in your latest issue (September, 1974, pp. 1297-99). In a way it's nice to see that, even in the dismal 1970s, the fervid passions of the late 'sixties continue to seethe. Alas, as those who remember the 'sixties will recall, it's not easy to argue with a pot boiling over. My first impulse was not to bother; but I guess I'm not quite Olympian enough simply to shrug or laugh it off.

Bloom ascribes to me a belief that "any constraint of inclination is tyranny," that "no harm can come of any indulgence whatsoever," that morally "anything goes." I am supposed to assert with particular vehemence that "anything goes" in sexual life: that, says Bloom, is my "erotic faith." I can see how this image might help my book's sales—at least it might have if it had appeared four years ago, when the book came out—but it springs from Bloom's imagination, not mine. I do emphasize Montesquieu's and Rousseau's attacks on sexual repression, and I unfold their arguments that erotic feelings and relationships are crucial to the development of the self. But I am just as emphatic in arguing that, especially for Rousseau, sexuality can be "authentic" only when

it generates deep and enduring human bonds: love, fidelity, commitment. The center of my book is an explication of romantic love as "the primary community," at once an heuristic model and a vital precondition for a wider, political community. "Thus, in making him a lover, I'll make him a good man" (*Emile*); "From now on, only love can guarantee your virtue" (*La Nouvelle Héloïse*): this is the only "erotic faith" in my book. I can see why some readers might find this romanticism naive or sentimental; I can see, too, why other readers might find my whole idea of authenticity ambiguous, elusive, deeply problematic; but for love or money I can't see how Bloom can see it as a general license to swing. I share Bloom's desire to distinguish serious scholarship from propaganda; but if his reading of my book is a fair sample of his scholarship, I feel for, and fear for, the other writers whose books he reads.

As for Bloom's nomination of me as a "hippy Savonarola": although it's a striking image—easily the best thing in the piece, if you ask me—I doubt that I'm fit to be keeper of this particular flame. Indeed, the virulence of Bloom's rage against my book suggests that he himself is carrying an awful lot of the Savonarolan fire within. Maybe that's what makes his review so inflammatory.

Bloom is especially incensed by my portrait of Rousseau as a divided self. "Like a magician in a circus, [Berman] cuts Rousseau in half; what once was an organic whole becomes two distinct personages . . ." I am said to treat his works "as though they were written by Dr. Jekyll and Mr. Hyde taking turns sentence by sentence." Actually, this caricature is not bad at all. In fact, these lurid images—of magic, mangling, and mutilation, of the haunting of a "good" self by a monstrous antiself—are precisely the images that, for over two hundred years, have been most persistently used to describe Rousseau himself. Both his nearest and dearest friends (e.g., Diderot) and his worst enemies tended to portray him this way; moreover, as a random glance into his autobiographical writings will show, he tended to see himself this way. These images define the brilliantly imaginative but tragically tormented soul that is "my" Rousseau. I have explored and explained him, loved him and hated him for years, and I will stand by him to the end. However, in taking my stand, I have no desire to bring the study and interpretation of Rousseau to an end. I realize there are other Rousseaus—not only, as Bloom suggests, from sentence to sentence, but often within a single sentence. Bloom's Rousseau, for instance, is smooth and static as a Grecian urn, a walking Platonic Idea. And why not? There are more than enough Rousseaus to go around,

plenty for all of us to cherish and cultivate. Bloom sees mine as a rank and poisonous weed that must be rooted out, while his prize "organic wholes" look like prefabricated, plastic flowers to me. Still, peaceful coexistence should be possible. In the field we share, the grand old common ground of Political Theory, there is surely enough room to let a hundred flowers bloom.

MARSHALL BERMAN

*The City University of New York*

#### TO THE EDITOR:

Mr. Berman's letter is a fair sample of the style and substance of his book. It requires almost no response, for it addresses itself to none of the precise criticisms I made concerning inaccuracies in his reading of both Montesquieu and Rousseau. Moreover, he admits his belief that writers in general and Rousseau in particular are pretty much what one wants to make of them—a belief I ascribed to him and one which vitiates serious interpretation. He treated his classic authors as imperfect precursors of his views which were the product of "the fervid passions of the late 'sixties," now apparently exhausted.

The only point his letter makes is that Mr. Berman is not a swinger. A close examination of my review will reveal that I made no such allegation. Nor did I say his view of Rousseau is "a rank and poisonous weed that must be rooted out." I suggested only that he wrote a poor book.

Mr. Berman says that I say that according to him "no harm can come of any indulgence whatsoever." My sentence continued ". . . so long as it comes from an authentic self." And I will stick by that formulation, which also implied that "authentic self" is an empty phrase because nowhere does Mr. Berman provide criteria for determining what an authentic self is. He does indeed praise Montesquieu and Rousseau for their "attacks on sexual repression," but he also attacks them ferociously for any restraints they favor—identifying restraint and sublimation with repression. His disclaimers would be more impressive if at any point in his book he told us about limits. Sexuality, he tells us, must "generate deep and enduring human bonds." That is also Rousseau's belief, and he presents the most detailed account of the conditions of "deep and enduring human bonds" ever given. The sexual limits he proposes are part of that account, and these limits Mr. Berman understands to be tyrannical and an expression of Rousseau's divided self. He never for an instant takes seriously the notion—not unique to the "tormented" Rousseau but accepted by most serious men of the past—that there is a tension between free sexual expression and enduring bonds. I did not deny that Mr. Berman wants such

bonds. But I do insist that his book teaches that the removal of all limits, constraints and prohibitions is the way to such bonds. This is an undemonstrated assertion.

ALLAN BLOOM

University of Toronto

### Pessimism and Politics

#### TO THE EDITOR:

The essay by Glenn Tinder, "Transcending Tragedy," appearing in the June, 1974 issue of the *APSR* is extraordinary for its originality, synthesis of a vast amount of thought in a brief compass, and contemporary relevance. The author has made a powerful case for "civility" as a stance to be taken toward contemporary politics and life.

I could not agree more with Tinder's penetrating analysis of the ideal of community, emphasis on the "limits and unreliability of action" (p. 551), concern with the pitfalls of unreserved commitment to any ideology or group and his criticism of "the pride involved in thinking man can control history" (p. 553). His criticisms of Marxism are most apt. He provides a useful warning against "fanaticism in action" and "absolutism in beliefs" (p. 560).

A recurring theme of the article is that the events of this century have been "disappointing" and "tragic." Tinder has used the term "tragic" in its everyday sense to refer to the misfortunes and sufferings, individual and collective, which afflict men, rather than in its more technical meaning in drama, although there is certainly an overlap between the two meanings. In the plays of Greek tragedians such as Aeschylus, Sophocles, and Euripides there are forces confronting each other which each have legitimacy. A tragic hero denies balance and goes beyond a limit, and death and punishment follow. Destiny is fulfilled in spite of the revolt of the tragic figure.

There is a similarity between the fatalistic themes of great tragic dramas and their concern with *hubris* and Tinder's awareness of the extreme difficulty of controlling history and the pitfalls of self-righteousness. However, following Tinder, I will employ the term "tragedy" in its common usage in this commentary.

The author maintains that "we are not forced to become doctrinaire pessimists" (p. 558) and that "civility is not dogmatic pessimism" (p. 559). While well aware of the presence of injustice in the world, he seeks to avoid a pessimistic conclusion by asserting that "what we are morally bound to will and what we can reasonably expect are not always the same" (p. 559). Moreover, he seeks to uphold the values of "the suffering inherent in civility" (p. 557) and "the immeasurable dignity of the human" (p. 560). Although sharing broad

areas of agreement with the author, I find myself bound to raise doubts from a utilitarian ethical perspective and a pessimistic ontology.

Tinder upholds the ethical value displayed in the lives of such individuals as Socrates, Jesus, Lincoln and Solzhenitsyn. No doubt, such values as "recognition of others" (p. 551), being "unreservedly attentive" (p. 554), and "solitary communality" (p. 555) can be seen in studying the lives of these men. Perhaps what they achieved is more admirable than "happiness," which Tinder sees as a "superficial" (p. 557) value.

However, utilitarians have made the point that numbers count. For every individual who reaches the moral heights extolled in the article, there are thousands or millions whose lives are mediocre or worse from a moral viewpoint. Many are broken by suffering rather than achieving dignity through it. It is doubtful that the tragedy of human existence can be redeemed by emphasizing the moral stature of a small minority of individuals. The suffering in the world, today as well as in the past, almost certainly outweighs the happiness in it, and thus from a utilitarian standpoint it becomes morally hard to defend the world's existence.<sup>1</sup>

If we observe that the human situation is gradually improving, or if some person or group discovers a way to change the world so as to make it a good place (i.e., one in which everyone has a decent life or at least a good chance for this), then let us discard pessimism. Until such time, the argument for pessimism is overwhelming. By pessimism I mean the philosophical position which asserts that the nonexistence of the world would be better than its existence and that this will continue to be so. One need not accept all of the more extreme or dogmatic statements of Arthur Schopenhauer to be a pessimist. A simple consideration of the enormous physical pain and suffering which afflicts humans and animals points toward the pessimistic conclusion. However, when one adds psychological suffering and moral evils (e.g. murder, rape, offensive wars, and war atrocities if not all war, dishonesty, ill will), it ought to be clear to a morally sensitive individual that the unfortunate aspects of life, particularly human life, outweigh its positive features.<sup>2</sup>

<sup>1</sup> See St. Augustine's detailed description of "the host of cruel ills" with which this life is filled, "if life it is to be called," *The City of God* (New York: Random House, 1950), Bk. XXII, chap. 22. Rousseau observed that man "is born to suffer" and proclaimed: "Ever more sorrow than joy—this is the lot of all of us," *Emile* (New York: Dutton, 1972), pp. 15, 44.

<sup>2</sup> Even Plato, who believed in the possibility of eternal salvation and cannot be considered a philosophical pessimist, observed that "the good things in human life are far fewer than the evil . . ." *The Republic of Plato*, trans. F. M. Cornford (New York: Oxford University Press, 1970), p. 71.

Tinder has emphasized that which "we are morally bound to will." Surely the great amount of good will in the world should not be overlooked. Yet the existing good will and the hypothetical possibility of closing the gap between what we are morally bound to will and what we can reasonably expect are not sufficient to outweigh sufferings and moral evils which are real and present and show no signs of ceasing.

Those of a "negative utilitarian" persuasion and some other philosophers would join Tinder in his criticism of making positive happiness the primary value.<sup>3</sup> Rousseau, for example, said that man's "happiness in this world is but a negative state; it must be reckoned by the fewness of his ills."<sup>4</sup>

Is there then something beyond the great moral stature of the few or earthly happiness? Tinder refers to "salvation" in quoting Solzhenitsyn (p. 557) and to a number of religiously oriented philosophers, such as Berdyaev and Jaspers. In addition, there is appeal to "the ancient sense that there is a Logos, or meaning, deeply embedded in reality." Moreover, it is said that hope "is not condemned" (p. 558).

Now, one is more in tune with reality, in my view, in accepting that there is no salvation, either on this earth or in some other-worldly existence, either for the individual or for mankind. The yearning for salvation is understandable, but probably based on wishful thinking. A hopeful outlook is often very helpful to individuals as they go about their day-to-day lives. However, in view of the unfortunate realities of hunger, starvation, disease, the infirmities of old age, illiteracy, crime and war, the situation of hundreds of millions of humans in the world today is hopeless, and it would have been better if many had not been born.<sup>5</sup> Nor is there reason to believe that the situ-

ation of the human race is better than two hundred or two thousand years ago.<sup>6</sup> There have been great advances in science and medicine and a decline in certain cruel institutions (e.g. slavery), but the twentieth century has seen totalitarianism, the Nazi concentration camps, and unprecedented numbers killed or injured in war, among other horrors. Tinder asserts that to "hold that history must be disastrous and that general, long range progress *cannot* occur expresses "despairing pride" (p. 558). Possibly so, yet the pessimistic view may also be a realistic assessment of what has happened and will probably continue to happen.

The question arises as to what is the alternative. One hope, though surely a distant one, is that large numbers of both ordinary citizens and government leaders will begin to take seriously the ideal of civility which Tinder has delineated for us so well. Moreover, efforts to prevent war and struggles against hunger, disease and ignorance are admirable. By now I have begun to sound like a reformer, who believes it is possible, after all, to build a better world. But as long as men are on earth it is not desirable to discourage their efforts to realize whatever accomplishments *may* be possible.

In searching further, one finds relevance in the thought of Machiavelli, who observed that "one never tries to avoid one difficulty without running into another" and maintained that "prudence consists in being able to know the nature of the difficulties, and taking the least harmful as good."<sup>7</sup> I will suggest applications of this principle to just a few problems. Government programs to control growth in population are badly needed in many parts of the world, and compulsory sterilization, while not a pleasant alternative, is needed in some areas. Those dying of painful diseases ought to be allowed to take their lives (it is already permitted in hospitals in Switzerland), and we ought to struggle to remove the taboo against this alternative. Mercy killing of severely retarded persons and very ill elderly persons, when they are suffering a great deal, could alleviate much misery. Mere life, unless there is some capacity for satisfaction or productivity or expressed desire to live, has no value, and it may even be a disvalue.

The chances of achieving real progress on a worldwide scale are very minimal. Whether or not it is better for men to realize this is a difficult

<sup>3</sup> Negative utilitarianism explicitly emphasizes giving priority to the relief of suffering over promoting happiness. See H. B. Acton and J. W. N. Watkins, "Negative Utilitarianism," *The Aristotelian Society*, supplementary vol. 37 (1963), 83-114.

<sup>4</sup> Rousseau, *Emile*, p. 44. For another expression of this view see Arthur Schopenhauer, "On the Sufferings of the World," in *The Pessimist's Handbook*, ed. by Hazel E. Barnes (Lincoln: University of Nebraska Press, 1964), p. 131. I believe that positive joy is possible but uncommon. The statements of Rousseau and Schopenhauer have great relevance as broad generalizations. Pain and suffering are not only more prevalent than pleasure and happiness; more importantly, they are more intense.

<sup>5</sup> Schopenhauer was inclined toward extremes, but many of his arguments for pessimism are worth considering. He claimed: "If you try to imagine, as nearly as you can, what an amount of misery, pain and suffering of every kind the sun shines upon in its course, you will admit that it would be much better if, on the earth as little as on the moon, the sun were able to call forth the phenomena of life; and if, here as there, the surface were still in a crystalline state." Schopenhauer, "On the Sufferings of the World," in Barnes, p. 129.

<sup>6</sup> Machiavelli's idea that "as a whole, the world remains very much in the same condition, and the good in it always balances the evil" is a plausible expression of the view that overall progress in the human condition is extremely difficult and maybe impossible. Niccolò Machiavelli, *The Discourses*, introduction to Bk. II.

<sup>7</sup> Machiavelli, *The Prince*, chap. XXI.

question, but probably it is better that they face the hard truth. Many individuals transcend tragedy for periods of time in their own lives and in their human relationships. Tinder and others have made noble efforts, but it is unlikely that the basically tragic nature of human existence will ever be transcended. Even philosophical pessimists, while attempting to understand history, are also struggling within it and subject to its tragic aspects.

MICHAEL C. STRATFORD

*Central Michigan University*

#### TO THE EDITOR:

I am surprised (as well as interested) by the nature of Michael Stratford's criticism. It does not express the notion that at bottom civility is only acquiescence in the evils of established society—a criticism I had anticipated. Nor does it express a realization that civility as I define it does not imply respect for established institutions—an objection I thought might possibly be made by conservatives. His criticism rather is based on a deeply pessimistic utilitarianism, and the central point of it is apparently contained in the statement that, "It is doubtful that the tragedy of human existence can be redeemed by emphasizing the moral stature of a small minority of individuals."

Despite the implication here that the decisive flaw in civility is its presumed confinement to "a small minority of individuals," and despite Stratford's assertion elsewhere that "numbers count,"<sup>1</sup> I do not believe that the primary issue is that of numbers. For one thing, I see no good reason for thinking that civility is beyond the capacity of the majority; in my article I argue against the idea that it is an elitist virtue (p. 560). Aside from this, however, even if civility were universal, it is hard to see what value it would have in a world envisioned according to Stratford's pessimistic utilitarianism. I do not understand Stratford's grounds for conceding that civility might be more admirable than happiness. How could that be, for a utilitarian? This points to what the primary issue, perhaps, really is: not numbers, and not even pessimism (so far as historical prospects are concerned, we are in agreement there), but rather whether Stratford's utilitarianism provides a basis for anything but an extreme and devastating incivility.

The question, as I see it, is whether utilitarianism provides philosophical grounds on which it is possible to affirm the absolute value of every individual. It would be difficult to show that any such value is implied by, or can even be accommodated within, a philosophy that equates happiness and the good. If the absolute value of the individual is

not affirmed, however, then it seems to me that there is no basis for civility—not only for the particular combination of attentiveness, detachment, and political responsibility that I called "civility" in my essay, but for civility of any kind, including that which Stratford evinces in his concern for reducing the sufferings of mankind. If each individual does not have absolute value, why (where my own welfare is not involved) should I care about the welfare of great numbers of individuals? On what basis does Stratford assert that "efforts to prevent war and struggles against hunger, disease, and ignorance are admirable"? If those efforts and struggles do not enhance my happiness, why should I support them?

If it could be made out on utilitarian grounds that each individual is of absolute value, however, then I think Stratford would encounter another serious difficulty in defending civility. In this case his pessimism would become unqualified and morally crushing, for if even one individual in all of history had gone through an unhappy life, the justification of the world would, on utilitarian grounds, be thrown into question. The pessimism that Stratford expresses in his comments is somewhat limited and tentative; it does not bar all hope and thus leaves room for struggling against war, disease, and other evils. But given a pessimism that asserts conclusively that it would be better if the world and mankind did not exist, surely nothing but a nihilistic morality could follow.

Stratford's comments are rich and thoughtful, and I might have drawn the line between us at some other point. For example, is it consistent for him to commend my emphasis on the limits of man's understanding and control, yet to be so confident of our powers of rational judgment as to call for policies (of mercy killing, and so forth) that would allow us to determine conclusively whether or not the lives of others have value?

To keep the present exchange within bounds, however, it is perhaps permissible to oversimplify. It is for this reason that I limit myself to asking how, in view of the principles he invokes in commenting on my article, Stratford can defend not only civility but any kind of concern for the public good except so far as one's own personal happiness is significantly and clearly involved? Of course one might find happiness for oneself in the happiness of others. But in that case, what stands between someone of Stratford's persuasion and the ethics of Dostoevsky's Grand Inquisitor? On the other hand, one may be less concerned than was the Grand Inquisitor with the happiness of all. In that case, what stands between a pessimistic utilitarian and—calling on another example from Dostoevsky—the ethics of "the possessed"?

GLENN TINDER

*University of Massachusetts, Boston*

## EDITORIAL COMMENT

**Off Our Backs, Change.** If anybody doubts there is something amiss with journals of political science, they need look no farther than *Change* magazine, a publication that covers issues in higher education. In discharging this mission, *Change* has undertaken to sponsor reviews of journals. Books, after all, get reviewed, why not journals?

One reason, we suspect, is that it is awfully hard for a single reviewer or team of reviewers to appraise with any competence the substantive contribution of a large and disparate collection of articles. In volume 68, for example, of our humble *Review*, we published 56 articles, 7 review essays, and one presidential address, not to mention a clutch of comments, rejoinders, communications and several hundred book reviews. That's a whole lot of reading, and only for one journal of political science, for one year.

Never mind. "Journals in political science," say our critics "... are badly written, jargonistic, and overly concerned with professional issues. ... [P]olitical science as a profession is an obstacle to the study—let alone the knowledge—of political things."<sup>1</sup>

Decrying the tendency toward high-cost research technology, the critics proceed to offer nothing in the way of evidence to back up this sweeping indictment save—heaven help us—a table, classifying the contents, by subfield, of six journals of the profession. From this table, they deduce such dubious disclosures as "public law and international politics are clearly step-children in the discipline," and "we do not find it accidental that second-class status is accorded the two fields traditionally most involved in questions of policy." This pejorative language is used to describe the finding that by the lights of the critics disproportionately few articles in the indicated subfields appear in the six journals surveyed. No mention is made of the curious fact that both subfields are blessed with several unusually good and prestigious journals of their own. Why don't political scientists care more about policy? Not only are we too "scientific," say the critics, but we are too specialized, unwilling to risk prediction, too narrow, too slow on the draw.

And that about wraps up the political science journals, folks. Without actually looking inside any article, comment, rejoinder, communication, or review, it is possible to tell all these things about the output of several hundred scholars.

We were about to throw in the towel and sug-

<sup>1</sup> Wilson C. McWilliams and Alan M. Cohen, "The Private World of Political Science Journals," *Change*, September 1974, p. 53.

gest a bonfire at the Banta Company when a kind friend sent us a couple of other articles from *Change*, namely the reviews of the economics and sociology journals.<sup>2</sup> Amazing! Apparently, exactly the same things are wrong with our neighbors as afflict us: "This material is narrowly conceived and written in technical detail that makes it forbidding to outsiders. . . ." "For relevance to contemporary issues, you'd best do your own thinking."<sup>3</sup>

"Let not the unwary long be deceived about the direct applicability to life and living of scholarly ruminations."<sup>4</sup> "If once in a while something interesting and accessible to a public wider than the specialists breaks the editorial barrier, it will be luck rather than design. Don't count on it. . . ."<sup>5</sup>

This marvelous uniformity of complaints across disciplines suggests any number of amusing possibilities. Of course one possibility is that all several thousand of us in economics, sociology and political science have somehow gotten off on the wrong foot and what we needed at an early age was more instruction from the likes of *Change* magazine. Another possibility is that there's something about a sampling of professional journals that tends to reflect the actual accomplishments of scholarly work and this work is, on average, just about average, and therefore couldn't possibly aspire to stimulate the jaded palate of a true connoisseur of scholarship.

Another possibility is that there's a *Change* line about these matters, and any journal review from those precincts will reflect a preference for musings of global import and a corresponding contempt for the grubby efforts of scholars. We have no idea what the correct explanation is. We are certain that had the critics at least of the political science journals stooped to do a little actual reading in connection with their task, they might not have filled their nostrils with heady nectar, but they might have learned quite a lot about such things as issue voting, social origins of liberal democracy, recent trends in trust in government, the consequences of party reform and other rules on the political nomination process, and, perhaps especially useful, the idea of civility.<sup>6</sup>

<sup>2</sup> Robert Lekachman, "The Economics Journals," *Change*, September 1972, pp. 59–61. Randall Collins, "Surveying the Sociology Journals," *Ibid.*, Winter 1972–73, pp. 70–74.

<sup>3</sup> Collins, p. 74.

<sup>4</sup> Lekachman, p. 59.

<sup>5</sup> *Ibid.*, p. 61.

<sup>6</sup> See Glenn Tinder, "Transcending Tragedy: The Idea of Civility," *American Political Science Review*, 68 (June 1974), pp. 547–560.



**Thanks, Folks.** For the first time, the memory of person runneth not to the contrary, hard evidence exists that political scientists believe in the *APSR*. "How do we know this? In a sample survey of *APSA* members, over two-thirds (70 per cent) said that the *APSR* was "excellent" or "good."<sup>1</sup> Yet only 21 per cent said they read the greater part of the journal, and a mere five per cent claimed to swallow each issue whole.

As we have often said, selective reading of the *APSR* makes the most sense to us, given the enormous range of concerns that animate the various members of our discipline. But it is heartening, all the same, to see that members of the profession are willing to extend good wishes to those whose interests differ from their own in the form of an endorsement of a journal that conscientiously publishes some material they personally do not read.

#### Articles Accepted For Future Publication

Joel D. Aberbach, University of Michigan and Bert A. Rockman, University of Pittsburgh, "Clashing Beliefs Within the Executive Branch: The Nixon Administration Bureaucracy"

Paul R. Abramson, Michigan State University, "Generational Change and the Decline of Party Identification"

Christopher H. Achen, Yale University, "Mass Political Attitudes and the Survey Response"

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

Francisco Arcelus and Allan H. Meltzer, Carnegie-Mellon University, "The Effect of Aggregate Economic Variables on Congressional Elections"

John A. Armstrong, University of Wisconsin, "Mobilized and Proletarian Diasporas"

John M. Bacheller, Kirkland College, "Lobbyists and the Legislative Process: The Impact of Environmental Constraints"

Richard M. Bank, University of California, Santa Barbara and Steven R. McCarl, University of Denver, "Virtue, Obligation and Politics: Revisited"

Charles D. Cary, University of Iowa, "A Technique of Computer Content Analysis of Transliterated Russian Language Textual Materials: A Research Note"

Jonathan D. Casper, Stanford University, "The Supreme Court and National Policy Making"

Roger Cobb, Brown University, Jennie-Keith Ross, Swarthmore College, and Marc Howard

Ross, Bryn Mawr College, "Agenda Building as a Comparative Political Process"

David Collier, Indiana University, and Richard E. Messick, Office of U.S. Senate, "Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption"

Richard K. Dagger, University of Minnesota, "What is Political Obligation?"

Fred R. Dallmayr, Purdue University, "Beyond Dogma and Despair: Toward a Critical Theory of Politics"

John P. Diggins, University of California, Irvine, "Four Theories in Search of a Reality: James Burnham, Soviet Communism, and the Cold War"

George Edwards, Tulane University, "Presidential Influence in the House: Presidential Prestige as a Source of Presidential Power"

Norman I. Fainstein, Columbia University and Susan S. Fainstein, Rutgers University, "The Future of Community Control"

Robert C. Fried, University of California, Los Angeles, "Party and Policy in West German Cities"

Benjamin Ginsberg, Cornell University, "Elections and Public Policy"

R. Kenneth Godwin and W. Bruce Shepard, Oregon State University, "Political Processes and Public Expenditures: A Re-examination Based on Theories of Representative Government"

Fred I. Greenstein, Princeton University, "The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies"

Susan Blackall Hansen, University of Illinois, Urbana, "Participation, Political Structure, and Concurrence"

Russell Hardin, University of Pennsylvania, "Hollow Victory: The Minimum Winning Coalition"

Fred M. Hayward, University of Wisconsin, Madison, "A Reassessment of Conventional Wisdom About the Informed Public: National Political Information in Ghana"

Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Industrial Conflict in Advanced Industrial Societies"

David K. Hildebrand, University of Pennsylvania, James D. Laing and Howard Rosenthal, Carnegie-Mellon University, "Prediction Analysis in Political Research"

Robert W. Jackman, Michigan State University, "Politicians in Uniform: Military Governments and Social Change in the Third World"

M. Kent Jennings and Gregory B. Markus, University of Michigan, "The Effect of Military Service on Political Attitudes: A Panel Study"

M. Kent Jennings, University of Michigan and

<sup>1</sup> Now we will reveal our favorite reading matter: *P.S. VII*, Fall, 1974, pp. 382-384, an article by Thomas E. Mann titled "Report on a Survey of the Membership of the American Political Science Association."

- Richard G. Niemi, University of Rochester, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations"
- Sam Kernell, University of Minnesota, "Presidential Popularity and Negative Voting: An Alternative Explanation of the Mid-Term Congressional Decline of the President's Party"
- James I. Lingle, University of California, Berkeley and Byron Shafer, Russell Sage Foundation, "Primary Rules, Political Power and Social Change"
- Michael Margolis, University of Pittsburgh, "From Confusion to Confusion—Issues and the American Voter (1956–1972)"
- Richard D. McKelvey, Carnegie-Mellon University, and John H. Aldrich, Michigan State University, "A Method of Scaling, With Applications to the 1968 and 1972 Presidential Elections"
- Richard D. McKelvey and Peter C. Ordeshook, Carnegie-Mellon University, "Symmetric Spatial Games Without Majority Rule Equilibria"
- R. D. McKinlay and A. S. Cohan, University of Lancaster, "The Political, Military, and Economic Performance of Military and Non-military Regime Systems: A Cross-National Aggregate Study"
- Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, University of Michigan, "A Majority Party in Disarray: Policy Polarization in the 1972 Election"
- Helmut Norpoth, University of Cologne, "Explaining Party Cohesion in Congress: The Case of Shared Policy Attitudes"
- Fritz Nova, Villanova University, "Political Innovation of the West German Federal Constitutional Court: The State of Discussion on Judicial Review"
- Karen Orren, University of California, Los Angeles, "Standing to Sue: Interest Group Conflict in the Federal Courts"
- Benjamin I. Page, University of Chicago, "The Theory of Political Ambiguity"
- Thomas L. Pangle, Yale University, "The Political Psychology of Religion in Plato's *Laws*"
- James L. Perry, University of California, Irvine and Charles H. Levine, Syracuse University, "An Interorganizational Analysis of Power, Conflict, and Settlements in Public Sector Collective Bargaining"
- Douglas Rae, Yale University, "The Limits of Consensual Decision"
- David Resnick, Cornell University, "Crude Communism and Revolution"
- Neil R. Richardson, University of Texas, Austin, "Political Compliance and U. S. Trade Dominance"
- Michael J. Robinson, The Catholic University of America, "Public Affairs Television and the Growth of Political Malaise: The Case of *The Selling of the Pentagon*"
- Austin Sarat, Yale Law School and Joel B. Grossman, University of Wisconsin, Madison, "Courts and Conflict Resolution: Problems in the Mobilization of Adjudication"
- Paul R. Schulman, University of Tennessee, "Non-Incremental Policy Making: Notes Toward an Alternative Paradigm"
- Peter J. Steinberger, University of California, Riverside, "Hegel as a Social Scientist"
- J. Weinberger, Michigan State University, "Hobbes's Doctrine of Method"
- Lynn T. White, III, Princeton University, "Local Autonomy in China During the Cultural Revolution: The Theoretical Uses of an Atypical Case"
- Fred H. Willhoite, Jr., Coe College, "Primates and Political Authority: A Biobehavioral Perspective"

# Crises and Sequences in Collective Theory Development\*†

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With the appearance of the seventh volume in the highly visible series "Studies in Political Development," the Social Science Research Council's Committee on Comparative Politics has reached a new stage in its study of the dynamics of political development. The chairman presents it as "both a report on work accomplished and an introduction to a new departure in comparative analysis."<sup>1</sup> Far more than the previous volumes, this book represents the intellectual achievements of the Committee. The earlier works were edited by Committee members—and sometimes they authored individual chapters—but about 80 per cent of the contributors were not affiliated with the Committee. By contrast, all of the authors of *Crises and Sequences* are Committee veterans, and they have had opportunity to interact regularly over a lengthy time span, to call upon experts from a

variety of disciplines, and to plan their research on a long-term basis with a reasonable assurance of funding.

In a last Committee report, Lucian W. Pye, the present chairman and an author of *Crises and Sequences*, observed that the "most important criterion in evaluation of any research activity presumably is the quality of the published products; . . ."<sup>2</sup> Since *Crises and Sequences* is in part a "report on work accomplished," a review and evaluation of this volume is, by the Committee's own standard, an appraisal of its research activities insofar as they are covered in this work. In undertaking an evaluation, we must first consider the Committee's goals and the resources available to pursue them.

## The Committee: Goals and Resources

In 1954, when the Committee on Comparative Politics was formed, "comparative government" was a weak, parochial field of study. Under the thoughtful and imaginative leadership of its first chairman, Gabriel A. Almond, the Committee set about to revitalize the field in two ways: (1) to broaden the scope of investigation to include non-Western political systems, and (2) to move beyond the historical and descriptive treatment of formal institutions to analyses of "groups and interests that provide the dynamics of politics in different settings."<sup>3</sup>

The Committee was eminently successful in achieving these objectives. By 1960, the "revolution" had broken out and was sweeping the comparative field. Instrumental in bringing about significant change were the Committee members of the 1950s. Almond has suggested<sup>4</sup> that the time was ripe for innovation, but the Committee's accomplishments cannot be overstressed.

A turning point in the intellectual history of the Committee on Comparative Politics came in 1960, when Almond and James Coleman published *The Politics of the Developing Areas*. Here one could see—especially in Almond's praiseworthy introduction—the effort to bring non-Western societies within purview and to move away from the "formal and institutional bias" in

\**Crises and Sequences in Political Development*. By Leonard Binder, James S. Coleman, Joseph LaPalombara, Lucian W. Pye, Sidney Verba, and Myron Weiner. (Princeton, N.J.: Princeton University Press, 1971. Pp. 326. \$8.00.)

The earlier volumes in this series, which have been reviewed previously in the *APSR* and will be referred to in this review essay, include:

*Communications and Political Development*. Edited by Lucian W. Pye. (Princeton, N.J.: Princeton University Press, 1963. Pp. 381. \$6.50.)

*Bureaucracy and Political Development*. Edited by Joseph LaPalombara. (Princeton, N.J.: Princeton University Press, 1963. Pp. 487. \$8.50.)

*Political Modernization in Japan and Turkey*. Edited by Robert E. Ward and Dankwart A. Rustow. (Princeton, N.J.: Princeton University Press, 1964. Pp. 502. \$8.75.)

*Education and Political Development*. Edited by James S. Coleman. (Princeton, N.J.: Princeton University Press, 1965. Pp. 620. \$10.00.)

*Political Culture and Political Development*. Edited by Lucian W. Pye and Sidney Verba. (Princeton, N.J.: Princeton University Press, 1965. Pp. 574. \$10.00.)

*Political Parties and Political Development*. Edited by Joseph LaPalombara and Myron Weiner. (Princeton, N.J.: Princeton University Press, 1966. Pp. 487. \$8.50.)

† The authors wish to acknowledge helpful criticisms and comments provided by Professors Harry Eckstein, Princeton University; W. Phillips Shively, University of Minnesota; Henry Teune, University of Pennsylvania; and three anonymous readers selected by the Book Review Editor. The authors, of course, assume sole responsibility for the contents of this essay.

<sup>1</sup> Leonard Binder et al., *Crises and Sequences in Political Development*, p. ix.

<sup>2</sup> Lucian W. Pye and Kay K. Ryland, *Activities of the Committee on Comparative Politics, 1954-1970* (New York: Social Science Research Council, 1971), p. 8.

<sup>3</sup> *Ibid.*, pp. 1-2.

<sup>4</sup> Gabriel A. Almond, *Political Development: Essays in Heuristic Theory* (Boston: Little, Brown and Co., 1970), p. 12.

comparative studies. By the time the volume had appeared, however, something more was needed. It had become apparent to the Committee that "without a more solid theoretical basis comparative studies of groups and leadership would only produce a proliferation of *ad hoc* research that would not be cumulative."<sup>5</sup> What was required was a theory of political development based upon a conceptualization of the whole political process changing through time.<sup>6</sup> Contributing to the development of such a theory thus emerged as a major objective of the Committee.

Almond's introduction to the first volume in the series shows that the Committee's attention was being directed toward the building of theory: "... the Committee hopes to make a contribution to political theory . . . The Committee's program seeks to develop concepts, insights, and theories which will improve our capacity to understand these experiments [new or rapidly changing nations] and to evaluate their prospects."<sup>7</sup>

Not only must *Crises and Sequences*—"a report on work accomplished"—be considered in terms of the Committee's objectives and intentions, but it must also be viewed in light of the resources available for conducting its programs. In terms of personnel, the Committee has been favorably situated. Between 1954 and 1971 (when *Crises and Sequences* was published), nineteen scholars had served on the Committee; of the ten Committee members in 1971, seven had served for a decade or more. Only two chairmen, Almond and Pye, had guided its activities. Thus the Committee has happily facilitated continuity through long tenure, and has sought fresh ideas and different skills through cooptation of new members at appropriate times.

Used to austere budgets, most political scientists would view the Committee's work as having been quite well financed. From January 1954 until March 1971, the group received about \$774,475 for its work.<sup>8</sup> But this averages only about \$45,000 per year. With this budget, the Committee sponsored 23 conferences, cosponsored six others, and conducted five summer workshops. Participating in these activities were 245 scholars from 67 different institutions; 45 of the scholars came from

other countries. Most of the \$200,000 spent directly on research by individuals went for a competitive grant program, supporting the field studies of 34 scholars in 21 countries.

While some might suggest that more funds should have been directly allocated for specific theory-construction objectives, there can be no doubt that the Committee was well-administered and that it generated a great deal of intellectual activity—a tribute to the two chairmen. The Committee's balance sheet is evidence of the piddling amounts available for attacking the difficult research problems in comparative politics, where expensive fieldwork overseas is essential.<sup>9</sup>

### Theory Development: The Approach in the Earlier Volumes

Since an evaluation of *Crises and Sequences* represents an appraisal, at least in part, of the Committee's research activities in terms of its goals in the 1960s, the volume cannot be examined in isolation. The six previous books, which are part of the research effort, must also be taken into account. Our concern in this essay is with the "quality of the published products"<sup>10</sup> as they relate to the goal of theory development; hence, in the earlier volumes we shall consider not the detailed empirical subject matter but only the conceptual and theoretical issues that arose and bear upon the content of *Crises and Sequences*. Most important, we shall dip into the six books to see how the Committee approached the task of theory development, which will help to account for the kind of theory that emerges in the final volume.

In the period before it began the work on *Crises and Sequences*, the Committee apparently did not place much emphasis on the development of a conceptual structure and a body of coherent and interdependent propositions. Although the Committee held conferences on theoretical and methodological issues, the earlier volumes in the series were planned without benefit of rigorous design and frameworks to which the contributors would be expected to adhere. The Committee members seem to have viewed such conceptual activities as being more appropriate at a later stage, after a body of empirical information had been collected

<sup>5</sup> Pye and Ryland, *Activities of the Committee*, p. 2.

<sup>6</sup> This is implicit in the statement in Pye and Ryland, p. 2.

<sup>7</sup> Gabriel A. Almond, "Foreword," in Pye, *Communications and Political Development*, p. vii. The Forewords to the next two volumes contain similar statements. In *Political Development*, Almond points out that the Committee "took on the task of encouraging the development of theory which would enable political science to deal more adequately with the causation of political phenomena and with the variety of political forms" (p. 15).

<sup>8</sup> Pye and Ryland, *Activities of the Committee*, pp. 6, 70-71.

<sup>9</sup> We must, of course, recognize that some members of the Committee had access to funds for research that were not an official part of the Committee's activities. Some of the research supported by these funds has contributed to the theory-building work of the Committee.

<sup>10</sup> See Pye and Ryland, *Activities of the Committee*, p. 8. Another criterion is the impressive number of publications that emerged from the Committee's research activities. According to the authors, the Committee "helped in the production of 296 written reports, including books and articles in journals, unpublished research memoranda, and other documents."

and absorbed.<sup>11</sup> This orientation appears to have been dominant during the Committee's formative stage:

The committee did not question the possible usefulness of theoretical insights but considered ways in which their usefulness could be tested. It was suggested that a "forced draft" approach to theory construction often produces quite harmful results . . . The committee plans at a later time to give a more thorough consideration to the question of the applicability of general theoretical systems to the study of comparative politics.<sup>12</sup>

The first six volumes show that a case study of some aspect of political development in a specific country was the favored vehicle for accumulating and presenting the empirical information. Although the studies open up rich veins of factual knowledge, they are not usually employed as a device for testing hypotheses that emerge from a body of coherent theory. They are not even used to supply tentative answers to a set of common questions. Instead they are used to present diverse types of information as a basis for developing empirical generalizations. Rejecting the strategy of designing a rigorous framework to guide the preparation of the individual studies, the Committee chose to issue only general instructions and to give the authors relatively free rein so that they could use their imaginations and develop their materials according to their own preferences. Take, for example, the *fifth* volume (*Political Culture*), which apparently involved the most structured guidance up to that time—a "suggestive but not prescriptive memorandum." One of the editors points out the general approach:

. . . it seemed best to emphasize more the existing richness of area studies than the potential advantages of systematic schemata for defining and classifying political cultures. . . . Some uniformity was requested of the authors as all were asked to include in their chapters some historical treatment and some evalua-

tion of the significance of various socializing agents. . . . We are still convinced that the subtlety and richness of analysis guaranteed by providing latitude to the authors outbalances any returns that might have come from more restrictive but uniform instructions.<sup>13</sup>

He indicates that the country studies "were all initiated, and most were concluded" before Verba's instructive theoretical essay was available to the authors.<sup>14</sup>

Even the key concept "Political Development" was not given a precise definition which could be consistently employed. According to the editors of the sixth volume (*Political Parties*), "The term 'political development' remains elusive, and we have not attempted any systematic definition. Several preceding volumes in this series suggest preliminary conceptualizations, and it is our hope that the . . . concluding volume will provide a definition that confronts most of the conceptual pitfalls. . . ." <sup>15</sup> Imprecision in the central concept of the research means that the reader, as he proceeds through the first six volumes, encounters more than a dozen definitions of political development.<sup>16</sup>

<sup>13</sup> Pye, "Introduction," in Pye and Verba, *Political Culture and Political Development*, pp. 13-14.

<sup>14</sup> *Ibid.*, p. 14. The reader encounters in some of the other volumes similar statements concerning the objective sought and the research strategies employed. "[A] first measure in seeking intellectual order is to survey and appraise existing knowledge, and this has been a prime purpose behind our experiment" (Pye, *Communications and Political Development*, p. x). "As we are able to gather more information concerning the political forms, processes, and policies with which the newer nations experiment, it is hoped that our theoretical understanding of the dynamics of political change will be enriched and that, as a result, additional contributions can be made to the evolution of a genuine comparative political science" (Almond, "Foreword," in LaPalombara, *Bureaucracy and Political Development*, p. vii). "[An] endeavor of this kind can do little more than suggest the richness of thought and information that the varied papers of the authors represent" (LaPalombara, "Introduction," *Bureaucracy and Political Development*, p. 6). "Although no rigid uniformity has been sought in outlook or organization, the treatment is broadly parallel" (Ward and Rustow, *Political Modernization in Japan and Turkey*, p. 12). The editors of the introductory chapters in the volumes on *Education* and *Political Parties* introduce a more systematic, theme-oriented format, although not all of the contributors address the essential themes that were laid out.

<sup>15</sup> *Political Parties and Political Development*, p. 399. In his introduction to the fifth volume, Pye said: ". . . we have not tried to adhere to a rigid definition of political development. We have instead sought to incorporate in our analysis most of the dimensions of political change and modernization which students of the new states tend to refer to when speaking of political development" (*Political Culture and Political Development*, pp. 12-13).

<sup>16</sup> To document the various uses of the term in the several volumes would require a lengthy footnote. Pye lists some of the ways in which the concept has been defined in his introduction to the work on *Political*

<sup>11</sup> A short conference on each of the first four volumes was held between early September 1961 and late June 1962. No specific conference was held for the volume on *Political Culture*; instead, the authors were given a "memorandum, suggestive but not prescriptive, which outlined subjects that might be generally relevant for the analysis of a political culture" (Pye and Verba, *Political Culture and Political Development*, p. vii). The planning conference for the *Crises and Sequences* volume was held in the summer of 1963. According to Pye: "Our focus at that time was on conceptualizing political systems, identifying their universal functions, and describing the processes of political modernization and development" ("Foreword," *Crises and Sequences*, p. vii). In 1968, the Committee held another conference on the *Crises and Sequences* book, during which the preliminary work of the authors was discussed with other scholars.

<sup>12</sup> Gabriel A. Almond, "Research in Comparative Politics: Plans of a New Council Committee," *Items*, 8 (March 1954), p. 2.

In well designed research, the concepts are sharply defined and assigned labels so that the nature of the phenomenon being examined is clear from the outset. When the main concept lacks precision, individual authors are in effect invited to define it for themselves with the result that they will not be studying the same thing. To the extent that "political development" in this series is defined in different ways, it will be difficult—if not impossible—to relate the various propositions one to another. Many of the attempted generalizations in the first six volumes cannot be cumulative and will have to be redone when the concept is refined and a consistent usage facilitated.

Reworking some of the generalized statements will not be arduous because a few authors were careful to attach specific meaning to "political development" and they were able to make unambiguous statements about relationships among variables. Many contributors, however, did not define the concepts they were using, and while they sometimes asserted the existence of relationships, the reader often experiences difficulty in interpreting the meaning of the statements and in locating the evidence needed for the assertions.

Other types of unevenness in the six volumes also detract from their usefulness in theory construction. Some authors, whatever the questions they were addressing, carefully documented their reports; others, however, wrote impressionistically and speculatively. Some of the contributors specialized in fact-gathering—with or without classification schemes—and a few concerned themselves with historical chronology. Many of the studies suffer either from a paucity of quantitative data or from measurement problems. Moreover, since the data are typically cross-sectional, they cannot be used for the study of political development as a dynamic process. Statistics on school enrollments and curricula may provide information on exposure to the educational system but do not themselves constitute evidence on the importance of the school as a socializing agent in the absence of data concerning how effective the socialization experience actually was. A number of the studies list factors that "may influence" a particular phenomenon, sometimes noting that the list is not "exhaustive." But neither a listing of potential variables nor references to "explanation" are substitutes for theory construction.

We voice these criticisms not in order to blame but to show that since the questions and key concepts were not clearly defined in advance, one

could hardly expect that a single volume, let alone the whole series, would be sharply focused and tightly integrated. It would be fortuitous, indeed, if the studies were cumulative. The scholars who passed judgment on the first six volumes in this *Review* criticized them in this regard.<sup>17</sup> Given the Committee's approach to theory building during the 1960s, however, these were not significant weaknesses. The studies in these early volumes were generally intended to be the raw material for concept formation and theory development. At this stage richness, variety, and broad coverage were preferred to rigor, consistency, and additivity. In other words, the Committee got what it wanted: the input for its theory mill. *Crises and Sequences* is the first major output from the mill. We shall now turn to an examination of this output. If it passes inspection, the criticisms of the earlier volumes are irrelevant; the Committee stands justified in the approach that it fol-

<sup>17</sup> The following are examples of critical comments of the first six volumes by reviewing scholars in this journal. *Communications and Political Development*: "[The essays] are in no way cumulative, and not always complementary" (Richard B. Fagen, *American Political Science Review*, 57 [September, 1963], 676-677). *Bureaucracy and Political Development*: "However, most of the other contributors [except Eisenstadt and Riggs] never stopped to ask what this volume was to be all about and thus confronted the editor with a difficult task indeed" (Alfred Diamont, *American Political Science Review*, 58 [December, 1964], 1032-1033). *Political Modernization in Japan and Turkey*: "Few of the chapters on the two nations make explicit comparisons. . . ." (Douglas H. Mendel, Jr., *American Political Science Review*, 58 [December, 1964], 1038-1039). *Education and Political Development*: "It would have helped had more precise definitions of different kinds of political elites been set forth, since they are used by the several authors differently—and even by the same author . . . The contributions are very uneven and do not present similar research results from different countries . . . The Committee . . . should consider being more selective and more restrictive as to focus next time around" (Edward W. Weidner, *American Political Science Review*, 60 [March, 1966], 121-123). *Political Culture and Political Development*: "There are rather fundamental differences in the conceptualizations . . ." (Aristide R. Zolberg, *American Political Science Review*, 60 [March, 1966], 119-121. Since the reviewer objects to the "premature achievement of theoretical monopolies," he regards the representation of different approaches and themes as a "major quality" of the book). *Political Parties and Political Development*: "[The] articles are special studies and not really additive theoretically . . . [The] concepts utilized are often unclear and the operationalization of key variables left to the imagination of the reader. There is no systematic empirical analysis attempted" (Samuel J. Eldersveld, *American Political Science Review*, 61 [December, 1967], 1166-1167). Most of these reviewers were more critical of these books than we have been. In fact, we regard much of the criticism as a bit premature, since the Committee, at this juncture in its activities, was explicitly in the preliminary stage of theory development which involved surveying data and seeking stimulating ideas and insights.

*Culture* (pp. 11-12). He presents a good discussion of the concept and its diverse meanings in his *Aspects of Political Development* (Boston: Little, Brown and Co., 1966), pp. 33-45.

lowed. But if we find the output deficient, we shall then be interested in looking at the basic design of the mill and how it has been operated. Serious weakness in design and operation call into question not the content of the early works but the whole approach to theory development adopted by the Committee.

### Crises and Sequences: An Exposition

For a multiauthored work, *Crises and Sequences* has remarkable unity, demonstrating the advantages of close interaction and long tenure on the Committee. With the exception of the first and last chapters, the central theme of political development and the crises that emerge is laid out and elaborated in terms of several key concepts, used with striking consistency.

The crucial task of constructing a definition of political development was undertaken by James S. Coleman in Chapter 2:

Our conception of the political development process is a continuous interaction among the processes of structural *differentiation*, the imperatives of *equality*, and the integrative, responsive, and adaptive *capacity* of a political system. The interaction of these three dimensions constitutes what we have termed the "development syndrome." Political development, in these terms, is seen as the acquisition by a political system of a consciously sought, and a qualitatively new and enhanced political capacity as manifested in the successful institutionalization of (1) new patterns of *integration* and *penetration* regulating and containing the tensions and conflicts produced by increased differentiation, and (2) new patterns of *participation* and resource *distribution* adequately responsive to the demands generated by the imperatives of equality. The acquisition of such a performance capacity is, in turn, a decisive factor in the resolution of the problems of *identity* and *legitimacy*.<sup>18</sup>

The first two technical terms in the definition are very clear. Differentiation refers to

the process of progressive separation and specialization of roles, institutional spheres, and associations in societies undergoing modernization. The assumption has been that the more highly developed a political system becomes, the greater will be its structural complexity and the larger the number of explicit and functionally specific administrative and political structures it will have.<sup>19</sup>

<sup>18</sup> *Crises and Sequences*, pp. 74–75. The notion of the "development syndrome" was first presented by Coleman in the *Education* volume, pp. 15–16.

<sup>19</sup> *Crises and Sequences*, p. 75. Coleman's definition of political development still leaves considerable ambiguity. If the political development process is an *interaction* among a class of processes, a class of imperatives, and a type of capacity, then the more highly developed a political system becomes, the greater will be this interaction. According to Coleman's formulation, the levels of the variables do not enter into the definition until later, and then in a manner that leads to a quite different definition. In fact, high political development as

Equality is treated as having three components: national citizenship, a universalistic legal order, and achievement norms.<sup>20</sup> As it is defined, capacity refers not only to the ability of a polity to "manage the tensions created by increased differentiation" and to "respond to or contain the participatory and distributive demands generated by the imperatives of equality," but also a "new and enhanced capacity to plan, implement, and manipulate new change as part of the process of achieving new goals." The main attributes of this concept "creative capacity" are *scope* and *effectiveness*, which are aspects of a developing polity's performance, and *rationality*, which is "historically and logically associated with the secularization of government, that is, with the *differentiation* of government processes and procedures from religious organization, influence, and control."<sup>21</sup>

The central theme of the volume emerges clearly in the Coleman chapter. As a polity develops or modernizes (i.e., increases in differentiation, equality, and capacity), certain crises are generated: identity, legitimacy, participation, penetration, and distribution. Starting at some position on a multidimensional scale, a polity has a given degree of differentiation, equality, and capacity; if the polity is stable, it will have a type and level of identity, legitimacy, participation, penetration, and distribution that are compatible with that position. But as the polity modernizes, the degree to which these five elements are present becomes inappropriate or their types incompatible with one another, and a crisis is precipitated in one or more of the "problem areas." The various potential crises are interrelated; one can give rise to another. This raises the question—not systematically addressed until the final chapter—of whether there may be sequences of crises.<sup>22</sup>

The analytical scheme, then, looks something like this: the elements of the development syndrome interact over time, and the incongruencies that emerge tend to generate particular types of crises. But if a polity has moved through the process, it is "developed" in the sense that it has new, enhanced capabilities.

Several questions arise as one ponders the

high interaction is never used in the book. Instead, greater political development is conceived of as greater differentiation, greater equality, and greater capacity. Why the authors should force the careful reader to explore the implications of the interactions and to sort out the intricate shifts between process-oriented and structure-oriented definitions perplexes us.

<sup>20</sup> *Crises and Sequences*, p. 77.

<sup>21</sup> *Ibid.*, pp. 78–80 (emphasis added).

<sup>22</sup> Significant subthemes are, of course, introduced by individual authors, but this summary catches the essence of the concern. Without this concern, there would be no reason for a book with this subject matter and title.

scheme. If the development process involves continuous interaction among the three elements, is there not some measure of tautology built in because capacity is partially defined in terms of differentiation? What guidelines do we have for assessing the interaction—an increase in how much differentiation is equivalent to how much equality or capacity? This is a matter of some importance, since, according to the authors, the elements of the syndrome do not covary. An increase in “survival” capacity, for example, may require societal diffusion (de-differentiation) and/or a decrease in equality. In what specific ways are the five “crises” related to the components of the syndrome? From the viewpoint of theory, it would be helpful if there were hypotheses that stated *precisely* what types and degrees of change in what elements can be expected from particular types of incongruences.

Rather than elaborating specific hypotheses, Coleman discusses each of the three dimensions of the syndrome as they apply in the developing areas. Differentiation is largely a problem of balance and mix; equality is the problem of mass expectations and demands; capacity is the problem of integration and performance. There is a considerable body of literature on the problems of differentiation and equality in the Third World, to which the author makes good reference. Capacity, however, is a trickier concept which, although widely used by students of development, has been little analyzed.

Moving smoothly from Coleman's contribution, Lucian Pye treats the identity crisis and the legitimacy crisis in Chapters 3 and 4. After distinguishing between mass political culture and elite political culture, he argues that a

crisis begins to occur when the particular character of either the mass or the elite culture, or both, causes the inherent strains between, say, capacity and equality to become greatly magnified and perceived as a major threat to rulers or subjects or both. The result may then be that people question the basic unity of the society, causing an identity crisis, or they may question the proprieties of government, causing a legitimacy crisis.<sup>23</sup>

Pye lists four types of identity crisis: (1) those relating to nationalist feelings about territory; (2) situations in which social structural cleavages prevent effective national unity; (3) conflicts between identification with subnational groups (e.g., ethnic loyalties) and commitment to national identity; and (4) those deriving from the “psychological consequences of rapid social change and ambivalent feelings toward outsiders.”

The author then presents a classification of elite cultures that are related to problems of national

unity. In an *expanding elite culture*, the members of the political class are willing to share their standards and norms with the masses and to provide a meaningful place in the polity for all citizens who are prepared to accept the essential rules of the political game. This pattern for solving the national identity crisis provides a basis for coping with participation crises at a later time.

An *exclusive elite culture* attempts to establish a common basis for nationhood, but significant elements of the population (such as ethnic minorities) are excluded and relegated to uncertain status.

A *closed political culture* establishes two identities, one being related to the dominant elite culture and the other being linked with the mass culture.

In the *parochial elite culture*, the elite, while professing commitment to modernization, clings tenaciously to traditional values, denying leadership posts to those who possess the skills to carry out development programs.

In countries that have no dominant culture, there may be created a *synthetic elite culture* that “rests heavily upon the institutions introduced through foreign rule, but that must somehow be accepted as expressing a legitimate form of nationalism.”<sup>24</sup>

Finally, there is a category that embraces countries *without a model elite culture*. In these cases no dominant culture existed historically, and the colonial power ruled directly without fostering the growth of a local elite and an indigenous culture.

Pye concludes his discussion of identity crises with a statement about the importance of leadership in “resolving identity problems and creating the basis for national unity.” Given the perspective of the volume under review, this strikes us as being a type of capacity, but the author does not use the term in this context.

Pye's treatment of leadership takes him to an analysis of the legitimacy crisis, which he defines as a “breakdown in the constitutional structure and performance of government that arises out of differences over the proper nature of authority for the system.”<sup>25</sup> Four classes of legitimacy crisis are identified: (1) conflicting or inadequate bases of authority, as when a ruling group cannot develop an acceptable rationale for enhancing its authority, or is forced to compete for power with other groups, such as a religious hierarchy or a tribal oligarchy; (2) excessive and uninstitutionalized competition for authority, typified by societies in which orderly governmental structures have not yet taken form; (3) rejection by the masses of the rulers' claim to legitimacy on the basis of their

<sup>23</sup> *Crises and Sequences*, p. 104.

<sup>24</sup> *Ibid.*, p. 131.

<sup>25</sup> *Ibid.*, p. 136.



interpretations of history or their promises of "future accomplishment"; and (4) dysfunctional socialization processes.

The key to the solution of any legitimacy crisis is *dynamic leadership*, which in turn requires an appropriate mix of institutional legitimacy and personal legitimacy. In seeking to establish their authority, leaders are often confronted with "basic dilemmas of choice." These include the need to remain firm in their adherence to traditional forms balanced against the need to accommodate to changing conditions; the question of preserving or rejecting the past; and the problem of responding to the immediate desires of the people or of persuading them to make sacrifices for future goals.

In summarizing his analysis of the identity and legitimacy crises, Pye indicates that he has in effect been discussing political alienation. He identifies two forms of alienation which arise from incongruities between the general and political socialization processes. The first type occurs when early childhood socialization creates an image of social relations as being orderly and just, but later experience in politics reveals a world that is "unregulated and dishonest." The second form of alienation—prevalent in contemporary developing societies—arises when early socialization fosters a suspicious and distrustful outlook upon the world, and agents of political socialization later present conflicting outlooks—some making exaggerated appeals to idealism while others portray political life as vicious and corrupt.

After formulating a broad definition of *participation*,<sup>26</sup> Myron Weiner, in Chapter 5, marks out several conditions under which a rapid rise in participation occurs: (1) periods of *social mobilization*; (2) change in the patterns of *social stratification*, as when the modernization process creates new social groups or modifies the relations among existing groups; (3) activity on the part of the *elite*—in, for example, nationalist movements or causes devoted to the spread of egalitarian ideas; intra-elite conflict may also lead to expanded participation as various factions compete for wider support; (4) changes in *government output*, illustrated by increases in government activity and the granting of suffrage rights, which bring new groups into contact with the rulers and broaden the arenas of political conflicts. According to the author, social mobilization and changes generated by the elite are related to equality, social stratification is associated with differentiation, and the government output factor is linked with capacity.

Weiner then looks at the growth of participation—and the timing of the growth—in relation to other changes in the political system. (1) In those

countries where the *institutional framework for participation* grew out of the demand for participation, citizens are likely to value representative institutions. But where, as in many developing states, such institutions were created before participatory demands, popular commitment to the new forms may be shallow. (2) As citizens feel the impact of the *growth of central authority*, they try to increase their influence in national politics, and their new links with the center can undercut the local leadership. But if widespread participation occurs before the transfer of power from the local to the national level, the establishment of a representative system may enable local notables to extend their power to the national scene. (3) If political institutions are able to sink roots before *bureaucratic structures* grow overpowering, the people stand a better chance of making the bureaucracies responsive to their wishes. (4) Most Western countries developed a *sense of national identity* before the demand for participation arose. However, in many contemporary modernizing societies, where multiethnic groups are already participating to a high degree, the fostering of central loyalties is a much more difficult task.

A participation crisis is defined as "a conflict that occurs when the governing elite views the demands or behavior of individuals and groups seeking to participate in the political system as illegitimate."<sup>27</sup> Thus a crisis can occur when the ruling authorities refuse to share power with social groups whose demands or values they consider to be illegitimate, or when the elite does not approve of the mechanisms through which the demands are being made, or the methods that are employed. Sometimes the demands themselves may be regarded as being out of the question, as when a dissident group calls for secession, special corporate representation, or the transfer of power to a local level. Finally, a participation crisis can break out when the group seeking influence will not settle for the sharing of power with existing elites but is determined to replace these elites and then to deny them "the right to hold power."

Weiner next discusses the various ways in which governing elites respond to participation crises, the new institutions that may emerge from the resolution of a crisis, and the dilemmas facing new participants who seek to enhance their influence within the political system. The crises of identity and legitimacy, he points out, may be settled for relatively long periods of time; but alterations in socioeconomic patterns and generational change may lead to participation crises in recurring fashion.

In Chapters 6 and 7, Joseph LaPalombara treats the crises of penetration and distribution.

<sup>26</sup> See below, pp. 988–989.

<sup>27</sup> *Crises and Sequences*, p. 187.

Both of these are related to capacity, but distribution is also associated with equality. He defines penetration as "conformance to public policy enunciated by central governmental authority. The degree of penetration in any polity may be viewed as the probability that governmental policies regarding the polity as a whole, or any of its subdivisions, will be carried out . . ." <sup>28</sup> The penetration phenomenon has two important dimensions: (1) the central government's capability of achieving it regardless of the attitudes of the citizenry; and (2) the ability and predisposition of citizens to "receive information regarding policy accurately and to *wish* to conform to such policies voluntarily."

The author's definition of "penetration crisis" is not as clearcut as his specification of "penetration." <sup>29</sup> Apparently a crisis is distinguished by "pressures on the governing elites to make institutional adaptations or innovations of a particular variety." Avoidance or adequate resolution of such a crisis requires that a government be concerned not only with popular attitudes but also with structures, employing a mixture of traditional forms and new institutions.

LaPalombara detects four types of penetration crisis, some of which may overlap: (1) *empty territory*, as when a regime attempts to push forward into new lands or to impose tighter central controls over localities within the present boundaries; (2) *regional differences*, typified by heavy contrasts in cultural and economic life between the center and outlying districts; (3) *communal autonomy*, as when the rulers decide, in the interest of national development, to change the status of a minority ethnic group; and (4) *peasant communities*, represented by a regime's efforts to extent its influence into the countryside.

The author, in Chapter 7, narrows the definition of distribution problems "to include those that imply that the political elite take a hand to increase the material goods available to a society or to redistribute such goods as may be available at any given time." <sup>30</sup> Either of these two policy situations can lead to a crisis at any stage of development, and the crises can recur.

The chapter explores the factors that influence the capability of political elites to deal satisfactorily with distribution problems—ideology, physical and human resources, and the international environment. In discussing the ideological factor, LaPalombara deals with nationalism (including the "competition for a constant pie" orientation); socialism; and "welfare capitalism"—the contemporary view that responsibility for solving distribution problems rests with the public sector.

On the issue of natural resource development, the author questions the belief, prevalent among economists, that an emphasis upon industrialization at the expense of agricultural development will invariably result in enlarging the economic pie. Urbanization in the developing countries, he warns, is characterized by pools of unemployed peasants and intellectuals whose demands upon the government might precipitate distribution crises.

Turning to human resources, he discusses the manpower problem in terms of the quality of leadership required to formulate and execute economic plans, and the psychological barriers to economic growth. The obvious answer to the problem of training managerial talent and of inculcating appropriate attitudes toward modernization among the population is "education." But here the author raises some intriguing policy questions which will challenge the leaders of developing countries. He suggests that "both the configuration and hierarchy of demands will differ from country to country . . ." and that "each situation must be treated in many respects as if it were . . . unique." <sup>31</sup>

When LaPalombara turns to the constraints of the international environment upon the elite's capacity to handle distribution problems, he decries the tendency of nation builders to want to imitate the West. Such a posture leads them to neglect agriculture, to uproot indigenous institutions that may be useful, and often to make unwise investment decisions in their haste to make their country appear "modern."

Compared with the crises of identity, legitimacy, and participation, distribution crises are less permanently resolved. Even the "mature" industrialized states face recurring crises of this type. In the Third World countries, however, the recurrence of the distribution problem may come while the rulers are still trying to resolve the identity question, the legitimacy crisis, or some other type of emergency. Having to face so many demands simultaneously places a burden on the political system which it may not be able to carry.

This, then, completes the description of the conceptual framework and the summary of the several components of the main theme, leaving for discussion the first and last chapters. Chapter 1, by Leonard Binder, is intended to be a general introduction to the entire book. It treats the development crisis through historical and philosophical discussions of stability and change and of the individual's role in society. Aristotle, Ibn Khaldun, Hegel, Marx, Rousseau, Sartre, and other eminent philosophers make their presence felt through Binder's erudite pen. The task of pro-

<sup>28</sup> *Ibid.*, p. 208.

<sup>29</sup> *Ibid.*, pp. 205–206, 227.

<sup>30</sup> *Ibid.*, pp. 235–236.

<sup>31</sup> *Ibid.*, p. 265.

viding an expository review of this chapter we shall have to leave to those who are more competent than we. The essay appears to us to be ambiguous in many places, failing to define technical terms, to provide transitions needed to follow the argument, and to establish foreshadowing connections with the rest of the book. Consider the problems raised in the following paragraph:

Fairer pricing, or the greater efficiency of factor markets throughout the whole system, might be thought of in terms of the "integration" of factor markets. The view does not require that factor prices be the same throughout the entire geographical area coinciding with the economic system; it merely insists that price differentials be justified in terms of costs such as transportation, storage, financing, and the like.<sup>32</sup>

Does "or" in the first sentence equate "fairer pricing" with "greater efficiency" (alternative terms for the same concept), or does it mean that two different things ("fairer pricing" and "greater efficiency") may be thought of as elements in a single concept "integration" of factor markets? Does "integration" of factor markets mean the integration of the market for labor with the market for land and/or capital, or does it refer to integration with one another of geographically local markets for land, labor, and capital? From a reading of the previous page, we learn that this "theoretical use of the term [efficiency] still has practical consequences for the price structure and the availability of the factors of production throughout the economy, while its more analytical aspect is evident in the use of the alternative term 'integration'."<sup>33</sup> It might now be implied that, by looking at the greater efficiency of factor markets in terms of the "integration" of those markets, we gain a greater analytical perspective. Does thinking of "fairer prices" in terms of the "integration" of factor markets give us the same advantage? But what is this "more analytical aspect"? The author does not give us much help on these questions, for he digresses into some thoughts about how intriguing it is that the concept "integration" has terminological relevance for a number of the social sciences. Even if we were to concede that the statement makes sense, we cannot see what purpose it serves. Since there are no footnotes in this portion of the essay (nor in some of the other crucial sections) to indicate what literature the author is referring to, we are left floating adrift.

The concluding chapter by Sidney Verba is an entirely different matter.<sup>34</sup> In many respects it is a

commentary on the other chapters in the volume and on the work of the Committee on Comparative Politics to date. Verba's chapter, which is an admirable piece of "Committee self-criticism," could have been published separately as an analytical overview. We shall be referring to it frequently in the analysis that follows.

### Crises and Sequences: A Critical Analysis

According to the foreword, *Crises and Sequences* is "noteworthy as an effort at collective theory building in the social sciences."<sup>35</sup> The approach taken is what we would call *intuitive empirical generalization*. In this approach, there is little emphasis during the early stages on rigorous concept formation, systematic analysis, or the development of interrelated propositions. Information and ideas are needed first, and impressions, speculation, and some systematically collected data are gathered in. Insightful scholars then reflect upon these materials, attempting to develop general concepts and conclusions which will "make sense" out of the rich diversity of human experience. The process by which one moves from the raw material to the theory is never made explicit. Rules of inference are not spelled out. Intuitive processes are apparently considered to be the most important.<sup>36</sup>

This approach may be contrasted with two others—*systematic empirical generalization* and an *analytic-deductive approach*. In systematic empirical generalization, more attention is paid initially to concept formation and particularly to opera-

<sup>32</sup> *Ibid.*, p. ix.

<sup>33</sup> Besides utilizing their own research for the impressive knowledge about the developing areas which they display, the authors draw upon the Committee's earlier works in formulating the empirical generalizations that emerge in *Crises and Sequences*. The book on *Political Culture*, which has implications for identity, legitimacy, and participation problems, is an important antecedent of the *Crises and Sequences* volume; in the wrapup commentary, one of the editors treats the developmental crises and presents a short discussion of the question of sequencing. *Political Parties and Political Development* obviously deals with political participation; but it is also relevant to the treatment of legitimacy and penetration. The volume on *Bureaucracy* is clearly related to problems of penetration, but it is not used in this context as explicitly as one might have expected. *Education and Political Development* is also concerned with penetration, but its heavy emphasis on socialization makes a good portion of the content relevant to the analysis of identity and legitimacy problems. As we have already indicated, an early discussion of the development syndrome appears in this work. *Political Modernization in Japan and Turkey* bears upon all of the crisis areas, and was employed particularly in the discussion of the identity problem. The first volume in the series, *Communications and Political Development*, contains material that influences ideas on penetration, integration, participation, and—to the extent that communications processes affect "rising expectations"—distribution.

<sup>34</sup> *Ibid.*, p. 25.

<sup>35</sup> *Ibid.*, p. 24.

<sup>36</sup> The essay was originally written as "an internal memorandum to alert the other authors" to certain conceptual problems. (See Pye, "Foreword," in *Crises and Sequences*, p. ix.)

tional definitions, but general propositions are developed from an examination of empirical data with rules of inference being made explicit at the outset. Theory-building efforts that rely heavily upon inferential statistics are good examples.<sup>37</sup> Since quantifiable data are highly desirable if the theory builders are to express the relationships among variables systematically and precisely, attention must be given in the early stages to problems of measurement and to the development of measuring instruments.

An analytic-deductive approach to theory building stands in marked contrast to the other two. Its initial emphasis is on deduction rather than induction. At some point early in the theory-development process, careful attention must be paid to the specification of certain concepts as theoretical primitives in terms of which all other concepts will be defined. It is also important to specify the propositions that will be treated as axioms, and to identify the logical system (rules of deduction) which is being employed. In the subject matter of political science, William Riker's *Theory of Political Coalitions* and Mancur Olson's *The Logic of Collective Action* are examples of this approach.

There should be little argument that a body of precise, rigorous, and general propositions with real deductive power is a *sine qua non* of a science of politics. We assume that this is what Verba has in mind when he speaks of the goal of "a coherent and interdependent set of propositions."<sup>38</sup> Major disagreement exists, however, about how to achieve it.

We seriously doubt whether the intuitive empirical generalization approach taken by the Committee, as represented by these seven volumes and other published works, is either efficient or effective in realizing this scientific goal. After seventeen years of prodigious effort, dozens of commissioned and financially supported articles, six major volumes, and 284 pages of exposition in *Crises and Sequences*, we find Verba raising four basic questions:

- I. What is a sequential model?
- II. Sequences in relation to what?
- III. Sequences of what?
- IV. What is the relationship among the items in the sequence?<sup>39</sup>

It seems to us to be an inversion of the proper order that these four questions are raised at the end of the book. In our view, they should have been raised in 1963, when the volume was con-

ceived so that the authors of the central chapters could have addressed themselves to the issues.

This objection suggests an even more basic question: If a theory of political development is the goal, why did the Committee choose the approach of intuitive empirical generalization, which places so little emphasis on stating a coherent and interdependent set of propositions with rigor and precision? Verba provides one answer:

In most social research that attempts to deal with problems as complicated as "nation building," we usually face a dilemma. We can use fairly precise models that do not fit the empirical material or fairly loose formulations that fit reality better, but only because they are loose.

... [A] sequential model may represent a loose framework for narration in which case it is more useful than no framework at all—but not much. Or it can be a more precise model of social change—in which case it would be terribly useful if we could achieve it, but quite *hard to achieve*. The rest of these remarks will be based on the assumption that we aspire to the latter model, but *are often forced*, in fact, to operate closer to the former.<sup>40</sup>

This paragraph raises several questions. Is developing a "loose framework" (through the approach taken by the Committee) in fact easier than developing a "precise model"? Why, if we aspire to the latter, are we *forced* to work with the former? Is it because working with a loose framework is a way of developing precise models?<sup>41</sup>

Our answers to these questions, we suspect, will differ considerably from those of the Committee. In order to determine whether one approach is inherently easier than another, let us compare with *Crises and Sequences* an important monograph that derives a precisely formulated, coherent, and interdependent set of propositions in an analytic-deductive approach—namely, Kenneth J. Arrow's *Social Choice and Individual Values*.<sup>42</sup> Arrow's book was published in 1951, three years before the Committee on Comparative Politics was organized and twelve years before *Crises and Sequences* was conceived.

Let us see first how the two works handle the problem of defining the key concepts. In his chapter in the Committee volume, Weiner defines political participation as "any voluntary action, successful or unsuccessful, organized or unorganized, episodic or continuous, employing legiti-

<sup>39</sup> *Ibid.*, p. 286 (emphasis added). *Model* is the key term in Verba's chapter; it is used ten times on this page alone. But we must confess that we are still not sure what the author precisely means by the term. It is clear, however, that a model is a *very good thing*.

<sup>41</sup> See *ibid.*, p. 283. We assume that by a "precise" model of social change Verba has in mind "a coherent and interdependent set of propositions."

<sup>42</sup> Kenneth J. Arrow, *Social Choice and Individual Values* (New York: John Wiley and Sons, 1951).

<sup>37</sup> Other examples would include systematic comparative research and even case studies in which the strong inferential principle is employed.

<sup>38</sup> *Crises and Sequences*, p. 283.

<sup>39</sup> *Ibid.*, p. 285.

mate or illegitimate methods intended to influence the choice of public policies, the administration of public affairs, or the choice of political leaders at any level of government, local or national."<sup>43</sup>

In arriving at this definition, Weiner reviews ten ways in which participation has been defined or treated in a large body of literature. He wanted to make sure that his definition would cover a "wide range of activities viewed as participation in different societies" and (at least implicitly) that his work would relate to that of other scholars who have analyzed the same phenomena. The formulation of this definition, we submit, was not easy. Besides the literature review, Weiner devoted considerable intellectual effort to distilling the various elements for his definition.

In Arrow's monograph, the concept "social welfare function" is as central as "participation" is in Weiner's chapter.

By a *social welfare function* will be meant a process or rule which, for each set of individual orderings  $R_1, \dots, R_n$  for alternative social states (one ordering for each individual), states a corresponding social ordering of alternative social states,  $R$ .<sup>44</sup>

This should be an acceptable example of what Verba would consider a precise definition. But was it more difficult to formulate than the looser one developed by Weiner? Arrow based his definition on the work of one other scholar, and although he was familiar with alternative formulations, he made no attempt to incorporate them into his definition, nor did he examine what is meant by social welfare function in a wide range of societies. In this sense, Arrow's task was easier than Weiner's.

In the earlier sections of his work, Arrow was careful to attach precise definitions to such terms as *social states* and *individual orderings* for alternative social states. Later he provides clear-cut definitions of certain subtypes of social welfare functions, such as *imposed* and *dictatorial*.<sup>45</sup> Weiner, too, further explicates key terms in his definition, like *action*, *voluntary*, and *choice*. Although for the most part his terms are not as rigidly defined as Arrow's, we cannot see how the task he set for himself was easier—different, yes, but not easier.

If the crucial job of defining concepts is not any easier in a loose formulation than in a more precisely developed approach, let us see whether the comparison holds up with respect to the *hypotheses* that are developed. For this purpose we can

look at two propositions submitted by Pye and one presented by Arrow. Pye's formulations are as follows:

If there is no legitimacy crisis and the people are prepared to trust their leaders, then there can be a broad acceptance of the right of the government to do more than merely process and aggregate into public policies those interests articulated by the public at large . . .

On the other hand, when there is a serious legitimacy crisis, particularly if it is related to a rise in the demands for equality, then the popular reaction may be that the elite lacks the authority to go beyond meeting the immediately articulated interests of the public.<sup>46</sup>

These are examples of propositions that Verba would consider to be widely applicable and loose in the sense that they are not tightly linked with other propositions in a coherent set. The first proposition, however, is precise. It states that if two conditions are met—(1) there is no legitimacy crisis, and (2) people are prepared to trust their leaders—then it is *possible* ("can be") that a particular consequence will come about: "a broad acceptance of the right of government to do more than merely process and aggregate into public policies those interests articulated by the public at large." Hence, if a single situation can be found where conditions (1) and (2) are met and where the consequence is also found, then the possibility will have been demonstrated and we could accept the statement as being empirically true. It is a statement that can be confirmed by a single case but can only be falsified by an investigation of every case. (Note that neither the necessity nor the high probability of the consequence is stated in the proposition.)

The second proposition states a single condition linked with a consequence by the verbs "may be." The statement has what Verba considers the virtue of being widely applicable, but it has this quality because of the ambiguity of the verbs "may be" in the formulation. Can any proposition employing this combination of verbs ever be falsified? Does "may be" denote anything different from its negation "may not be"?

Mr. Jones asserts, "John may be home at 1:00 A.M. on December 1, 1975." Mr. Brown goes to John's home at the stated time only to discover that John is not there. Has the proposition been demonstrated to be false? Would it have meant anything different if Mr. Jones had said, "John may not be at home . . ."? In everyday parlance, "may be" sometimes connotes a probability statement, i.e., more likely than not (probability > 0.5). Since in scientific discourse there are such conventional ways of making probability statements, we

<sup>43</sup> *Crises and Sequences*, p. 164. We assume that most readers will agree that this definition is "loose" in the sense in which Verba uses the adjective.

<sup>44</sup> Arrow, p. 23.

<sup>45</sup> *Ibid.*, pp. 28, 30.

<sup>46</sup> *Crises and Sequences*, p. 155.

assume that Pye does not wish to convey any specific probability range.<sup>47</sup>

While criticizing the second proposition on the basis of its ambiguity, we do not wish to imply that either of the two statements was easy to formulate. Obviously, Pye has invested an enormous amount of time and thought in studying problems of authority and legitimacy. The effort, however, was devoted to learning about problems of legitimacy in many different political systems and to achieving wide applicability in the formulations; there was much less concern for precision, rigor, and the interrelationship of a set of propositions.

The most important proposition in Arrow's work is the general possibility theorem.

If we exclude the possibility of interpersonal comparisons of utility, then the only methods of passing from individual tastes to social preferences which will be satisfactory and which will be defined for a wide range of sets of individual orderings are either imposed or dictatorial.<sup>48</sup>

Unlike any of the propositions that might be extracted from *Crises and Sequences*, the statement by Arrow has been formally proved through the use of axioms and definitions (previously provided) and mathematical logic. The formulation is precise, and it is tightly connected to a number of other propositions by means of the deductive system employed. In other words, it is one proposition in a coherent set of propositions.

It would be meaningless to argue whether formulating the general possibility theorem was easier or more difficult than developing Pye's hypotheses. But we would contend that it is much easier to build a scientific theory of social choice and individual values from Arrow's work than to build a scientific theory of political culture and legitimacy from Pye's. The main reason is, of course, that the approach taken by the Committee does not provide for the coherent and interdependent set of propositions which is Verba's desideratum in a scientific theory, whereas the route that Arrow takes does. Furthermore, constructive criticism, disproof, and additive work are much easier in the Arrow approach. For

<sup>47</sup> We should call attention to the fact that a number of these "may be"-type propositions can be found in the other volumes. An example of another form of proposition that is difficult to falsify is the following: "... the higher the degree of differentiation of group and other relationships is in a political system, the greater is the probability for the development of political parties, though this probability may be reduced by the presence of other impeding conditions." (William N. Chambers, "Parties and Nation-Building in America," in LaPalombara and Weiner, *Political Parties and Political Development*, pp. 83-84.)

<sup>48</sup> Arrow, p. 59. All technical terms (e.g., "imposed," "dictatorial," and "satisfactory") are precisely defined.

example, Blau pointed out an error in the proof of the general possibility theorem; this was recognized and accepted by Arrow and other scholars, and the necessary modifications were made. We submit that it would be an immensely difficult task to detect flaws in the reasoning and analysis that led to Pye's conclusions (or the authors' conclusions in the other chapters).

We would not make so much of this issue were it not that Verba implies that the intuitive empirical generalization approach will over time facilitate the hammering out of a coherent and interdependent set of propositions that will explain political development. Indeed, one cannot justify the Committee's allocation of resources, given its goal of theory development, without this assumption. Rigorous deductive methods, we contend, are necessary for the theory building which the Committee has set for its goal. But, as reflected in the seven volumes under discussion, the Committee did little in the period from 1963 to 1970 to facilitate the development of theory that has coherence and interdependence, and hence deductive power.<sup>49</sup> Is it easier to specify theoretical primitives in 1975 that it was a decade ago? Is it easier now to designate a set of axioms? Is it easier now than it was earlier to identify the "rules of deduction" that would be the most appropriate? We think that the answer to all of these questions is "No." Thus, it really makes no difference which of the two approaches is the easier. If the development of the loose formulation does little or nothing to further the development of tight, coherent, and interdependent sets of propositions and that is the goal, then why waste resources on such formulations?

While what we have said indicates our disagreement with the Committee on the most efficient way to develop a science, we nevertheless commend Verba for exploring the structure of sequential models.

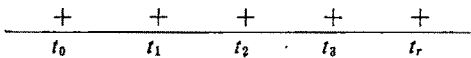
... The resultant condition that one is trying to explain (the existence of democracy, the development of a "modern" state, the growth of a bureaucracy) is explained in terms of a number of initial conditions. Furthermore, the initial conditions are not necessarily treated as if they occur simultaneously. The crucial concern is with the effect on the resultant condition of the order in which the initial conditions occur—this may include simultaneous occurrence, but that

<sup>49</sup> The references cited by the authors of *Crises and Sequences* reveal a secondary but interesting point. Literature that is relevant to the subject matter but employs an analytic, deductive approach is ignored. We find it difficult to imagine, for example, a chapter on political participation published in the 1970s that does not even refer to Mancur Olson's *The Logic of Collective Action* or does not address itself to some of the analysis set forth in Anthony Downs's *An Economic Theory of Democracy*.

is only one possible pattern. It is this stringing out of conditions over time that makes the model a sequential one.<sup>50</sup>

Verba's formulation, however, strikes us as somewhat elliptical. Why not simply use the classic statement: "The state of the system at some future time  $t_r$  (resultant state or condition) is a function of its state at some earlier time  $t_0$  (initial state or condition), plus all of the boundary events in the time interval  $t_0-t_r$ ?" If the initial conditions do not all occur initially, it is a bit confusing to view them as initial conditions; they are intermediate system states or boundary events and should be treated as such. In Figure 1, for example, Verba would be inclined to treat system states  $t_0$ ,  $t_1$ ,  $t_2$ , and  $t_3$  as initial conditions occurring in a sequence. (If this phrase means anything, it must be that the time is collapsed.) The alternative is to explain system state at  $t_1$  in terms of system state at  $t_0$ , plus boundary events in the time interval, and so on, until system  $t_r$  is accounted for. Since there can be different system states at each time point—depending upon initial condition, boundary events, and the probabilities assigned—different paths can be identified.

Figure 1



This conceptualization seems to be what Verba is advocating on page 288: "Sequential models link a series of conditions or events stretched out over time. In this sense, a sequential model makes the dependent variable of the first proposition the independent variable of the next. One has a series of statements about the relation over time of  $A$  to  $B$ , and  $B$  to  $C$ , and so forth." This statement suggests a first-order difference equation, and we are somewhat puzzled why this "logical form" was not suggested rather than that of the time-series version of causal modeling which Verba advocates.<sup>51</sup> The first-order difference equation seems to have all of the analytical characteristics that Verba wants, as well as some very desirable features that will interest him. For example, he is not limited to first-order relationships; he can easily shift to a continuous form (differential equations), if this would be more appropriate, without destroying the basic structure of the analysis; and he can address formally the questions of path stability and robustness and of equilibrium. We are not necessarily advocating the use of differential equations as a language (including rules of deduction) for both stating and analyzing sequential theories; we are suggesting

that they are superior to some modification of causal modeling in dealing with the problem as Verba defines it.<sup>52</sup>

In either approach it is necessary, of course, to devise a classification system which will handle different types of initial conditions. Since most efforts to build theories of political development begin with a category called the "traditional" political system, this means that the different types of traditional systems have to be specified. The Committee on Comparative Politics seems to have given little attention to this task, the essential nature of which should have been obvious by 1963. Some of the literature on political development suggests ways of attacking the problem; Apter's work is a notable example. In *The Political Kingdom of Uganda*, he presents a fourfold classification based on two types of authority structure and two types of value structure, yielding four types of "traditional" political systems, or "initial conditions." Apter is interested in the case in which the boundary event (Western colonialism) is the same for different types of initial conditions. If a society is to modernize, then a mobilization regime is likely if the initial condition was pyramidal authority and consummatory values; a modernizing autocracy if the initial condition was hierarchical authority and instrumental values; a consociational regime if the initial condition was pyramidal authority and instrumental values.<sup>53</sup> These, of course, can be treated as alternative paths to modernity. But, with further elaboration, such as

<sup>50</sup> Rather than attempting to adapt the Blalock causal analysis technique to longitudinal data, we would suggest going directly to Sewall Wright's method of path coefficients. While employing a logical structure comparable to causal analysis, Wright's technique was developed especially for the analysis of evolutionary phenomena. His original presentation was published in the 1920s. (See Sewall Wright, "The Relative Importance of Heredity and Environment in Determining the Piebald Patterns of Guinea Pigs," *Proceedings of the National Academy of Science*, 6 [June 15, 1920], 320-332; "Correlation and Causation," *Journal of Agricultural Research* 20 [January 3, 1921], 557-585.) For further elaboration, see Wright's "The Method of Path Coefficients," *Annals of Mathematical Statistics*, 5 [1934], 161-215, and "The Interpretations of Multivariate Systems," in *Statistics and Mathematics in Biology*, ed. Oscar Kempthorne, Theodore A. Bancroft, John W. Gowen and Jay L. Lush (Ames, Iowa: Iowa State College Press, 1954), pp. 11-34. An interesting discussion is contained in John W. Tukey, "Causation, Regression, and Path Analysis," in Kempthorne, pp. 35-66. John V. Gillespie has already explored the use of both the Blalock and the Wright techniques for the analysis of temporal sequences. He detects certain advantages in the Wright approach for the comparative analysis of time-series data. (See John V. Gillespie, *The Application of Causal Models to Comparative Political Analysis* [M.A. thesis, University of Minnesota, 1966], pp. 131-182.)

<sup>51</sup> See David E. Apter, *The Political Kingdom in Uganda: A Study in Bureaucratic Nationalism* (Princeton, N.J.: Princeton University Press, 1961), pp. 85-91.

<sup>50</sup> *Crises and Sequences*, p. 287.

<sup>51</sup> *Ibid.*



Apter provided in some of his later works, it is possible to introduce refinements, e.g., subtypes of the basic regime categories and intermediate system states. The important point is, however, that the different initial conditions and intermediate regime types would seem to give rise to different sorts of identity, legitimacy, participation, penetration, and distribution problems and in different sequences. It is a pity that important questions like these, which Verba introduces in the concluding chapter of the seventh volume, did not attract the attention of the Committee more than a decade ago when *Crises and Sequences* was in the planning stage.

Any sequential (or evolutionary) theory must deal either with the basic dimension(s) to which the sequences will be related, or to some end state or resultant condition. The development syndrome provides the basic dimensions, or—if “modern” is considered to mean high equality, high differentiation, and high capacity—the resultant condition. Verba and some of his colleagues suggest that of the three elements in the syndrome, capacity is the most fundamental. It is certainly crucial to many of the approaches to the analysis of political development, and especially to one that emphasizes crises. The capacity to cope with crises and to resolve them must eventually be part of an adequate explanation of development.

In spite of the centrality of this concept, however, it is not defined with clarity and precision, as we pointed out on page 983 and footnote 19. In fact, the definition used in *Crises and Sequences* shows little refinement beyond that given in Coleman's earlier writing. We recognize that the problem facing the authors is difficult, but after seeing how they have struggled with the conceptualization of capacity, we venture to make what may appear to be a startling point: Capacity is a characteristic of a *formal theory* of empirical phenomena, not a characteristic of the phenomena themselves.

Some illustrations will help to clarify our position. Although the terminology is a bit strange, we can speak of the perfect market as having the capacity to allocate resources Pareto-optimally if external (dis)economies, increasing returns, public goods, and indivisibilities are absent from the decision environment. That the perfectly competitive market has this capacity can be proven in a formal sense, but it is the capability of the market in the theory, not of any empirical market economy.

We could also speak of the horsepower rating of a gasoline internal combustion engine as its capacity (not to be confused with displacement capacity). Rated horsepower is computed from a formula that takes into account the diameter of

the cylinder, length of stroke, compression ratio, etc. If an individual hooked an engine to a dynamometer and discovered that its maximum performance was below the rated horsepower, would he conclude that its *capacity* rating (horsepower) should be reduced? Probably not. One would say that the engine was not performing up to capacity. But such a statement requires a conceptualization of capacity that is independent of measures of performance.

The usefulness of concepts like “capacity,” however, is not dependent upon the researcher's ability to measure performance in the same terms, as is the case with the engine on the dynamometer. We are not aware of any study by an economist to explore empirically through direct measurement the question of whether a real market economy was allocating resources Pareto-optimally. (Indeed, we are uncertain whether the practical measurement problems of such a study could be solved.) Useful analyses of suboptimal resource allocation in the presence of monopolies and monopolistic price-setting *are* undertaken, but only after “optimal” has been precisely defined.

The work of the Committee on Comparative Politics has not facilitated the development of the type of analytical concept represented by capacity because such a concept requires a theory with deductive power. The Committee, as we have seen, has not yet moved to an approach that would make this objective realistic. Verba's extended discussion about the difficulty of measuring capacity is, we think, putting the spotlight on the wrong problem.<sup>64</sup> Given this context, one does not measure capacity; one seeks to measure performance, and then to compare performance with capacity.

In its original form, the conceptualization of the five crises also presents some difficulties. Putting aside for the moment the question of what sorts of phenomena these really are and how they are related to the components of the “development syndrome,” we can detect the rudiments of a classification structure which has five different categories. The problem is that the categories are not defined with sufficient sharpness, and there is considerable overlap. The authors frankly admit that the categories are not mutually exclusive, although the possible overlaps do not seem to disturb them. But the fact is that analysts using this scheme would have the very difficult task of deciding whether a particular crisis fits into the category of “identity” or “legitimacy,” or is better suited in the “legitimacy” classification rather than that of “participation,” etc. The editors of *Political Parties and Political Development*, for example, conclude: “The earliest participation

<sup>64</sup> *Crises and Sequences*, pp. 292–294.



crisis may thus also involve a crisis in legitimacy."<sup>56</sup> Pye makes the statement: "The Russian, Chinese, and French revolutions all changed the basis of legitimacy in their respective countries but none were legitimacy crises as defined here because all were more centrally crises of participation and distribution. Some interpretations of the American revolution would make it essentially a legitimacy crisis, while other interpretations would make it too a participation and distribution crisis."<sup>56</sup> The authors never really tell us how to decide what kinds of crises one is actually observing.

In the latter part of his chapter, Verba clarifies the situation by seeking to refine the concepts in an important and useful way. Expressing a preference for "problem area" rather than "crisis," he then defines a crisis as a "situation where a 'problem' arises in one of the problem areas (that is, members of the society are discontented with one of the five aspects of the decisional process), and some new institutionalized means of handling problems of that sort is required to satisfy the discontent."<sup>57</sup> This conceptualization could lead to the identification of discontinuities which in turn makes a "stage" theory more attractive. Had this refinement been considered earlier, it too might have led to tighter specification in those chapters that deal with substantive matters.

Another difficulty—largely overlooked by the authors—arises from their concern about bridging the gap between the level of microanalysis (the individual) and the level of macroanalysis (the collectivity). In his introduction to *Political Culture and Political Development*, Pye expresses the hope that the study of individuals and of total systems can be brought together.

There has been a need to discover a method for working back from the complex subtleties of individual psychology to the level of the social aggregate which is the traditional plateau of political science. It is this problem of aggregation—which involves the adding up of the discoveries of individual psychology in such a manner as to make community-wide behavior understandable in the light of individual actions—that is now the great challenge to political science and for which the concept of political culture holds such great promise.<sup>58</sup>

The shift in analysis from the individual level to the group level becomes especially marked in discussions of the "identity crisis." Some of the authors, influenced by the work of Erik Erikson, view the question of national identity as "the political culture version of the basic problem of self-

identity." Individuals, for example, undergo a (political) identity crisis. (Am I a Virginian or an American?) And through some mysterious process this is translated into an identity crisis at the national level. But surely we need stronger scientific underpinning before we can speak of a *polity* as undergoing an identity crisis. The need here is for some rules that enable us to move from the psychological predispositions of individuals to a generalization about a pattern of psychological predispositions that characterize a reified entity like the nation-state. Pye sees the concept "political culture" as a way of relating micro phenomena to macro phenomena, but much remains to be done before this important objective can be realized scientifically.

Another difficulty involved in the level-of-analysis problem is suggested when the authors, in handling their nation-state materials, discover variability among geographic regions within the nation-state. One section of the country, for example, may be modernized, while another may still be under the strong influence of tradition. In some research problems, these differences among geographic areas may be greater than the differences between particular nation-states. The level of analysis at which research is to be undertaken will, of course, depend upon the nature of the phenomena being studied. In the study of some aspects of political development, a rigid focus upon the nation-state may obscure important variation at the local or regional level, which may reflect political change more sensitively.

Throughout this critique, we have not discussed the effort of the Committee to develop a capability for prescription. As was true in the earlier volumes, many assertions made by the authors of *Crises and Sequences* take the form of prescriptions. But the members of the Committee seem to recognize no major difference between a theory that will explain empirical phenomena (sometimes called "positive theory") and a theory that will guide the design of institutions and policies (sometimes referred to as "normative" or "design theory"). We argue that these are really distinct and that the failure to recognize the difference makes for deficient theory of both kinds.<sup>59</sup>

### A Final Word

Our critical remarks about the efforts of the Committee on Comparative Politics to make progress toward the construction of a theory of political development should not obscure the significant contributions the Committee has made to the discipline. Quite apart from the useful in-

<sup>56</sup> LaPalombara and Weiner, *Political Parties and Political Development*, p. 18. See also p. 224.

<sup>57</sup> *Crises and Sequences*, p. 136.

<sup>58</sup> *Ibid.*, p. 302.

<sup>59</sup> Pye, *Political Culture*, p. 9.

<sup>59</sup> Some of the important differences are discussed in Herbert Simon, *The Sciences of the Artificial* (Cambridge, Mass.: M.I.T. Press, 1969).

formation provided in the case studies, the several volumes contain many explicit or implicit propositions which will be challenging to researchers. We have already mentioned how the Committee has encouraged scholars to move away from traditional perspectives. Equally important, it has expanded the context of investigation into non-Western areas of the world, making Western political scientists realize that many of their generalizations are extremely limited in scope, inapplicable to the developing areas without considerable revision.

But this list of accomplishments does not alter our basic point: given the objective of theory building—a coherent and interdependent set of propositions in terms of which political development can be explained—we feel that the Committee's work has fallen short of the target. Verba points out that the chapters in the concluding volume present a "framework for the study of political development, not yet a theory; . . ."<sup>60</sup> After such a long-term effort, we have only a framework—and one that still does not satisfy Verba as he perceptively highlights important weaknesses. But even he appears to recognize only the symptoms of the crises in theory development; he does not identify the basic reasons for the weakness—the way the Committee defined the sequence of intellectual activities in the collective theory-building effort.<sup>61</sup>

While being critical of the work of the Committee since 1960, we single it out only because we were invited to review its works. In fact, many of the attempts at theory building by political scientists would be subject to the same type of criticism. Many political scientists confronting the task of the Committee in 1960 would have approached it in the same way. The emphasis in the profession is on building intuitively attractive frameworks for organizing data, not on identifying significant theoretical problems (or puzzles) and presenting the propositions in terms of which these problems can be resolved, even though the propositions may turn out to be empirically false. When propositions are developed, those that can be easily confirmed but are difficult to falsify are preferred to those that are easy to falsify but difficult to confirm. Many scholars exhibit little interest in elaborating the reasoning that lies behind the development of a proposition; persuasive rhetoric seems to be preferred.

These tendencies seem to us to reflect the re-

ward structure of the discipline. Scholars soon learn the "common law" of research propositions: to be trivial but true is a virtue if the alternative is to be meaningful but false. It takes no detailed history of science to point out the crucial instances when science took a major step forward as the result of a proposition being disproved and the theory on which it was based collapsing. The phlogiston theory and the ether theory, for example, could only be cleared away and something better developed if unambiguous, falsifiable propositions were advanced, with the reasoning behind them being well articulated. We have our phlogiston and ether theories in political science but they are partially hidden—implicitly embedded in grand but loose frameworks. They cannot be cleared away until they are made explicit and tested. But this activity is not rewarded—in terms of money or glory—when compared to the work of presenting a loose framework for organizing data. Had the members of the Committee set out in 1963 to develop a precise and rigorous set of coherent propositions which they (or others) tested and found to be false, the Committee's work would probably have been dismissed, if not derided. Thus, while we are critical of the Committee's approach and the published products, we attribute responsibility much more widely—to the practices and norms of the profession as a whole.<sup>62</sup>

Even though we have crossed lances at times with the authors of *Crises and Sequences*, we do not wish to be dogmatic about our position, and certainly we have no intention of directing an argument *ad hominem*. On the question of what is the most efficacious way to build theory, we hold open the possibility that we might be wrong. But we are sincere in our belief that progress in the development of a science of political life has been hindered by the absence of criticism and controversy over important, well-defined issues among those who are dedicated to the scientific enterprise. The history of science is replete with examples of advancement that came about as a result of intellectual disputation among adversaries. We share with the members of the Committee the goal of developing a scientific theory of political development. We hope that our critique will give rise to serious discussion which will contribute to the achievement of that goal. For our part, we approach the disputation in the spirit suggested by the Bard of Avon:

Do as adversaries do in law,  
Strive mightily, but eat and drink as friends.<sup>63</sup>

<sup>60</sup> *Crises and Sequences*, p. 283.

<sup>61</sup> We recognize that the position we have taken in this essay represents a minority view in all of the social sciences except economics. For a criticism—very much like our own—of the state of research efforts in psychology, see Paul E. Meehl, "Theory Testing in Psychology and Physics: A Methodological Paradox," *Philosophy of Science*, 34 (June, 1967), 103–115.

<sup>62</sup> We wish to acknowledge a special debt to Professor Harry Eckstein for contributing to our thoughts on the reward structure of the discipline.

<sup>63</sup> *The Taming of the Shrew*, Act I, Scene ii.

## BOOK REVIEWS

**Objectivity in Social Science.** By Frank Cunningham. (Toronto: University of Toronto Press, 1973. Pp. 154. \$8.50.)

This slim volume provides a convenient catalog of arguments and counterarguments invoked by those who deny and those who affirm the possibility of "objectivity" in social science. As such it should be helpful to those professional social science practitioners who have neither the time nor disposition to pursue all the arguments and counterarguments concerning this issue bruited by real or pretended philosophers of science. The book provides reasonably adequate accounts of the basic arguments construed as supporting some one or another form of "anti-objectivism" culled from the works of notables such as Thomas Kuhn, Benjamin Lee Whorf, Karl Mannheim, Norman Hanson, Peter Winch, Michael Polanyi, P. K. Feyerabend and Juergen Habermas. Professor Cunningham's counterarguments to "anti-objectivism" are themselves reasonably adequate, and generally follow the lines of argument articulated by specialists such as Israel Scheffler, Ernest Nagel, Richard Rudner, and Adolf Gruenbaum. The reader, as a consequence, has the opportunity of supplementing, in a substantial manner, his repertoire of cognitive responses to some of the more interesting questions that seem seriously to disturb the contemporary social science community.

Cunningham's book is a commendable attempt to support the claim that there are no defensible arguments for maintaining that the descriptions and explanations entertained by social scientists are somehow "nonobjective" functions of the social scientist's language, thoughts or desires regarding them. Cunningham insists that whatever we know of social science practice, social science products, and their underlying philosophy of science rationale, supports the claim that social science, does in fact, reveal something of the actual nature of its subject matter—and further, that there is, in principle, a public—that is to say, intersubjective—strategy for resolving the truth claims of rival empirical theories.

The book is effectively organized, and the principal arguments against scientific, and particularly social science, objectivity are collected in chapters devoted to arguments from (1) the nature and history of science; (2) linguistic relativism; (3) perceptual relativism; and (4) the special subject matter of social science.

Arguments and counterarguments are presented in an intelligent and intelligible manner.

The major shortcoming of the book is that it is obliged by the very nature of the complex, often vague and ambiguous, character of the subject matter, to attempt too much. One can only accomplish just so much within one hundred twenty-three pages of text. As a consequence, there are any number of special, entailed, subsidiary, and tangential arguments that might have been pursued. The entire issue, for example, of the "objectivity" of the decision rules governing the acceptance or rejection of statistical hypotheses (particularly in social science research) is assessed and dismissed in about a page. On the other hand, we are sometimes teased with a suggestion that even moral issues might be "objectively" resolved (cf. p. 11)—but the suggestion is nowhere developed. But despite all such qualifiers, Cunningham's book, given its size, is admirable.

There is one curiosity about this book that requires comment: it is a book about the objectivity of science written by a convinced Marxist. To the perverse this might suggest something like a convinced Roman Catholic writing about the "objectivity" of the Church's strictures against birth control. To the less perverse it does suggest some speculation. Would Cunningham hold that the "theory" of Karl Marx is, in fact, supported by best evidence—that evidence that rests on discriminable and objective "fact," on rigorous classification and typology, on competent explanation, and reasonably precise and time-conditioned prediction? Or one might choose to speculate on whether Cunningham takes Lenin to be an "objective" social scientist and a competent philosopher of science. If such questions are not engaging enough, how about: Is Mao a Marxist social scientist? How about Fidel Castro? Or, perhaps to give the entire game away: Is Marxism the only objectively true social science? One day, Professor Cunningham will have to write a book about all that. For the time being it is enough that he wrote a good book about serious matters. That he chose to confound us by confessing something about his personal faith detracts not in the least from its merits. If he is serious about his methodological convictions—and I am sure that he is—he is the kind of "Marxist" the academic and scientific community could only welcome.

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**Jean Bodin: Verhandlungen der Internationalen Bodin Tagung in München.** Edited by Horst Denzer. (Munich: Verlag C. H. Beck, 1973. Pp. xiv, 547. DM 78.)

Bodin is of course famous, and rightly so, as the chief developer of the theory of sovereignty in modern times; and it is among the merits of *Jean Bodin: Verhandlungen der Internationalen Bodin Tagung in München*, a multi-lingual collection of some two dozen essays on Bodin, that the reasons for this fame are impressively unfolded. One says "developer" and not "inventor" with good reason: the notion that *princeps legibus solutus est* was a key point in Roman law, as F. H. Hinsley has made plain in his fine *Sovereignty*; and Walter Ullmann has shown how much of the theory of sovereignty was (at least) incipient in some of the arguments against Boniface VIII's claims for papal universalism in his notorious *Unam Sanctum* in the early 14th century. But Bodin is usually taken to have been first to present a fully worked out and coherent sovereignty doctrine: he defined the sovereign power as that which can "give laws unto all and every one of its subjects and receive none from them," a power which is supreme, and not merely one among other law-giving public authorities (the Church, various medieval *collegia*, etc.). Bodin's sovereign was to be above the positive law (as its creator) but subject to divine and natural law; it was also to respect the fundamental constitutional laws of the realm and subjects' property rights. Bodin's sovereign was, in short, though the highest and indeed final authority, a *law-giving* authority, not an arbitrary or capricious one; the distinction between absolutism and arbitrariness was as important to Bodin as it was to be to Bossuet's *Politique tirée des Propres Paroles de l'Écriture Sainte* (a book which revolves around this very distinction). Bodin was first to conceive a nonarbitrary absolute political power in this sense; he not only developed the concept of internal sovereignty within a single state, however, but went on to deny the existence of a *Respublica Christiana* overarching the various states and to develop a new theory of international relations based on state autonomy rather than on "universal" institutions such as the papacy and the empire. Thus Bodin's theory of sovereignty simultaneously undermined the claims to political authority of all groups and institutions *below* the level of the national-state and the claims of all "universal" institutions *above* the state level.

It is true enough that Bodin did not have the "last word" on sovereignty and that later

writers, particularly Hobbes and Rousseau, took the doctrine of sovereignty even farther than did he. As Robert Derathé shows, in one of the best of the essays in *Jean Bodin* ("Bodin dans l'Histoire des Théories de la Souveraineté"), Rousseau dropped Bodin's notion that sovereigns must operate within the limits of "fundamental" laws when he urged that "it is against the nature of the body politic that the sovereign impose upon itself a law which it cannot infringe"—though here, of course, Rousseau is thinking of the sovereignty of a people whose general will is "always right," a concept quite foreign to Bodin. It is also true that a certain inflexibility in Bodin's theory of sovereignty led him to some odd political analyses: his insistence on the indivisibility of sovereignty made him insist that the 16th century Holy Roman Empire was an "aristocracy" because the seven electors of the empire chose and might depose the emperor, whereas the reality of the case was far more complicated, involving a mixture of declining feudal monarchy, rising regional states, "free cities" such as Hamburg and Lübeck, *inter alia*. Not even Bodin's distinction between "form of state" and "form of government" (well treated by Horst Denzer in another of the essays, "Bodins Staatsformenlehre") made his treatment of German politics persuasive. (Voltaire, for all his flippancy, was nearer the mark when he confined himself to saying that the Holy Roman Empire was neither holy nor Roman nor an empire.) And in the case of a federal polity such as Switzerland the inflexibility produced still worse effects: sovereignty-doctrines, which rely precisely on the notions of absoluteness and indivisibility, are hard-pressed to account for territorial divisions of power and for multiple political allegiances (hence Bodin's treatment of federal systems as "alliances" of "sovereign" powers).

Despite all this, Bodin's theory of sovereignty is still taken to be the first full and adequate statement and defense of the doctrine; and it is at least arguable that the difficulty which Bodin faced in trying to explain sixteenth-century German or Swiss politics in terms of indivisible sovereignty was an almost universal difficulty in his time—though his refusal to countenance any notion of a "mixed state," explained well in the present book by R. J. Schoeck's "Bodin's Opposition to the Mixed State and to Thomas More," did not help matters. One can further point out, as does Julian H. Franklin in one of the most informative essays in *Jean Bodin* ("Bodin and the End of Medieval Constitutionalism"), that the theory of sovereignty was not altogether accurate even

as an account of French politics, since it succeeded in "proving" the existence of an absolute sovereignty in France "only by ignoring or distorting considerable evidence pointing to a tradition of consent" (p. 166)—a point, incidentally, made as early as 1677 by Leibniz in Chapter 11 of his *Caesarinus Fürstenerius*, which urged that in France "aid is not obtained from the clergy, which is the third part of the kingdom, by mandates given from the plenitude of power (as they say), but by demands, negotiations, and discussions."

It would be quite wrong, however, to suggest that *Jean Bodin* treats Bodin's theory of sovereignty at the expense of the rest of his thought; on the contrary, a generous number of pieces is devoted to less familiar Bodinian topics. Here one could mention a most persuasive piece, "Bodin and the Idea of Order," by W. H. Greenleaf, which draws illuminating parallels between Bodin's *Six Books of the Commonwealth* and his less well known *Method for the Easy Comprehension of History*; three substantial essays on Bodin as an historian and as a philosopher of history; and an intriguing contribution by Kenneth D. McRae entitled "Bodin and the Development of Empirical Political Science," which makes the plausible point that Bodin was not just describing sovereignty (to return to that apparently inescapable subject) but also promoting it. There is even a piece by Christopher R. Baxter, "Jean Bodin's Daemon and his Conversion to Judaism," which will present to those familiar with Bodin simply as a theorist of sovereignty a completely "new" side of his character and writings.

If, though, one had to single out one of these essays as most helpful and instructive, one's choice might well fall on Raymond Polin's "L'Idée de République selon Bodin"—a piece which, while laying sufficient stress on the distinction between absolutism and arbitrariness, and on the notion that a Bodinian sovereign is hedged round by natural law, constitutional law and private property-rights, dwells at the end, as most of the other pieces do not, on what sovereignty is *worth*. And in this assessment Polin urges that while it is true that Bodin spoke often in the language of "necessity," and talked of a political power which was to be "absolute, infinite, perpetual," he was concerned that a sovereign "have as much obligation as power" and that his power be "in truth, a function of his obligation" to create and uphold laws which imitate divine laws (p. 356); that a state be possessed not just of absolute power but of "a rightful government, well-founded and assured public authority, harmo-

niously realized justice" (ibid.). Of the two dozen essays in *Jean Bodin* it is arguable that Polin's finds the most adequate equilibrium among all of the elements that Bodin valued, and puts the question of sovereignty's value above the question of its historical origins—though it does not go so far as to ask, with Laski, whether the notion of sovereignty has not lost whatever utility it might once have had; nor to ask, with de Jouvenel, whether the concept ever had enough validity to offset the danger involved in elevating a theory of "absolute, infinite, perpetual" power to a supreme principle of political right.

It remains to add that the editor, Horst Denzer, has appended to these essays a remarkably comprehensive bibliography and an excellent index, and that a 72-page "discussion" between the authors of the twenty-four essays has been appended at the end. Since this discussion proceeds in three languages (German, French, English), and sometimes in a mixture of several of them—Derathé, for example, sometimes answers his German colleagues' arguments in German, but makes his own observations in French—it will obviously be of use mainly to those able to switch languages rapidly. But it ought not to be thought that this book is closed to those who are not linguists; there are quite enough good pieces in English (nine out of twenty-four) to warrant the attention of readers who know English only. This collection, in short, merits careful consideration, and deserves the praise of those interested in Bodin's political thought.

PATRICK RILEY

*University of Wisconsin*

**Strategic Interaction.** By Erving Goffman. (Philadelphia: University of Pennsylvania Press, 1969. Pp. 145. \$2.95.)

Erving Goffman has already demonstrated his ability to write interestingly on rational gamesmanship (see especially *The Presentation of Self in Everyday Life*), and this book will not disappoint his readers. *Strategic Interaction* amounts to two long papers, the first one, "Expression Games," and the second bearing the title of the book. While certainly linked, the two seem most probably to have been written separately and simply bound together in this volume. No matter, for Professor Goffman has stimulating points to make.

"Expression Games" is a fascinatingly introspective analysis of the kinds of ploys we may all use in "informing" others the way we want them informed. How someone else will behave is a function of what he believes about his opportunities, and about our intentions and char-

acter. Goffman shows how we try to alter such a person's beliefs to our benefit, how he will in turn cross-examine the evidence we thrust at him, and how we develop a powerful intuition about ways to get believed despite this cross-examination. Getting believed is no mean trick even when one is telling the truth, but Goffman depicts us all as being more expert than novice at it. The author, to prove his point, perhaps relies too much on examples drawn from "gee whiz" books about World War II subterfuge and espionage, some of which might be taken with a grain of salt. Yet his basic point is so clearly correct for most human interactions that he hardly needs the supporting or illustrative "big war" examples. The propaganda expertise we live by may thus rest at the level of everyday intuition. Goffman's forte is analyzing this in explicit and thoughtful detail. His analysis parallels some of Robert Jervis's writings (especially *The Logic of Images in International Relations*) (Princeton, New Jersey: Princeton University Press, 1970) and Thomas Schelling's *Strategy of Conflict* (Cambridge, Massachusetts: Harvard University Press, 1960).

The author's style is all the more interesting and remarkable because he is an anthropologist and sociologist by training, rather than an economist or political scientist. Sociologists have usually tended to stress the inadvertent over the premeditated. In Goffman's uncovering of and emphasis on conscious behavior and rational goal-serving mechanisms, there is thus an enthusiasm and excitement which an economist (for whom all of this is old hat with a suspicion of tautology) more typically might not display.

The second paper on "Strategic Interaction" amounts to an interesting step-by-step explication of strategic analysis in social situations as intuited in ordinary English. Can one demonstrate by any stream of consciousness that we rationally pursue explicit goals in such situations? Goffman's essay spins out a persuasive illustration that we indeed do. One cannot claim that Goffman's structures and definitions here are thoroughly new or revolutionary, although some of his asides on the inputs of sociological analysis for the sorting out of game-theoretical problem types are important. The essay is valuable because it is so thoroughly readable, and profoundly independent, the product of someone working through the conceptual forest again, to find very parallel trails.

Goffman throughout relies heavily on his intuition, illustrated by examples taken from a broad acquaintance with writings in history and politics, but never worrying about whether his *N* is large enough to generate some sort of sta-

tistical reliability. His problem, as with any other believer in strategy, is almost a form of Heisenberg uncertainty principle: since it is obviously so sensible for actors to pretend to be less clever and devious than they really are, we must assume that they are more clever and devious than they seem. Where the evidence does not leak out, assume it, or get it by introspection, for the proof of strategy is that one does not indeed see all the strategy.

We may no longer have any shortage of lively books on strategic reasoning. Yet anyone who felt ill-equipped, for purposes of teaching students, or prodding his own research imagination, would do well to read this book.

GEORGE H. QUESTER

Cornell University

**Social Justice and the City.** By David Harvey. (Baltimore, Md. The Johns Hopkins University Press, 1973. Pp. 336. \$10.50.)

"Cities . . . are founded upon the exploitation of the many by the few" (p. 314). Thus David Harvey concludes in this polemical book. He observes that a "genuinely humanizing urbanism has yet to be brought into being" (*Ibid.*). The first half of the book considers liberal theories of urbanism; the second half formulates a revolutionary theory.

First we are told of the difficulties urban specialists have in relating decisions affecting the spatial form of the city (where will housing or means of transportation be located?) with decisions affecting the social processes going on within the city (employment, welfare services, etc.) so as to accomplish some "coherent social objective" (p. 50). Both kinds of decision actually redistribute income, defined as "all receipts which increase an individual's command over the use of a society's scarce [man-made as well as natural] resources" (p. 53). So both may be linked by a conception of social justice defined as an equitable distribution of income. Presently decisions allocating urban space and governing urban social processes tend to cumulate income advantages for the rich. For example, improvements in public transportation enable the affluent to move swiftly from downtown to the suburbs. Since the employment opportunities in the suburbs are growing, the impact on income distribution of the decision to build public transportation is regressive; the black domestic cannot even find a bus in her neighborhood to take her out to the suburbs. Worse still, efforts to counteract income inequities seem futile. For example, the effort to attract investment funds to inner-city rental housing will cause a shortage of investment funds for suburban home construction; since

the suburbs can afford to do so, they will increase interest rates and attract these funds back to the suburbs.

The fault of all liberal theories of urbanism is that they deal with symptoms; they do not deal with underlying causes. "Programmes which seek to alter distribution without altering the capitalist market structure within which income and wealth are generated and distributed, are doomed to failure" (p. 110). Thus liberal theories of urban phenomena amount to an apologetic for the existing inequitable income distribution (p. 147). For instance, present day urban land use theory separates the value to the occupier—the use value—from the exchange value of land and property. A socialist theory is competent to explain why the use value of land or property, which should be the primary consideration from the point of view of social justice, is not. The market economy presupposes maintenance of an artificially created scarcity of land and property. As long as there is scarcity, exchange value will predominate over use value. Maximization of exchange value by some groups automatically reaps disproportionate benefits for the few (p. 175). Social justice is unattainable for as long as the mechanism responsible for these inequities—the free market system—is preserved. ["The rentier will get that pound of flesh no matter what" (p. 191).]

The city itself, in its totality, is merely a convenience for the market system. The consumers conveniently collected together in the city have needs which can be easily manipulated; they consume the surplus product of capitalist society and by doing so preserve the artificial scarcity requisite for high profits for a few. The city serves as a repository for the surplus value of a capitalist society (p. 231). Herein lies the flaw in the free market system: "if the rate at which surplus value is being appropriated at the centre [of the city] . . . exceeds the rate at which social product is being created, then financial and economic collapse is inevitable" (p. 264). Nor will efforts to restrain the monopolistic practices succeed; such efforts will simply hasten the inevitable collapse. "A more open and individualistic capitalism . . . would almost certainly destroy all civilized society" (p. 273).

Only the socialist view which recognizes that social injustice in the city is the inevitable consequence of inherent flaws in the free market system can inspire a theory which is not apologetic for the *status quo*—a revolutionary theory. And with such a theory the revolutionary practice needed for a "genuinely humanizing urbanism" (p. 314) may be achieved. "The

Chinese and Cuban efforts to promote growth with social justice are probably the most significant so far undertaken" (p. 115).

In Harvey's book, the free market system assumes a life of its own independent of politics; the political process merely reflects the dominant economic forces (p. 73). Existing inequities are held to be due to the operation of an economic mechanism rather than to defective political arrangements. Harvey ignores the connection between racial tensions and urban problems. Also, he predicts, without any supporting evidence, that labor will be driven to subminimum subsistence levels. (p. 224).

The critical step in Harvey's argument is his definition of income as command over all resources, manmade as well as natural. Once income is defined so roomily, it is easy to view all decisions in the city as primarily economic. Since there does not seem to be any conspiracy orchestrating the growing income inequity, it makes sense to attribute the cause to an underlying economic mechanism—the free market system. This argument might be credible if and only if Harvey demonstrated that the "income" inequities he inveighs against are not the result of a web of conflicting if not coordinated political judgments. Also, his argument would have been strengthened had he defined his view of social justice, instead of asserting somewhat mysteriously that a valid conception of social justice can be verified only in practice (p. 12).

A happier scheme of organization might have been chosen—some scheme other than forcing the reader to struggle along the same paths Harvey apparently followed in evolving his "revolutionary theory." A coherent statement of his thoughts in their final version supported with an array of reasons and evidence would have made the book more forceful and more intelligible.

The book contains several compelling insights, yet the nagging doubt remains that it reveals more about Harvey's theorizing than about socialist theorizing about urbanism.

PHILIP B. LYONS

*House Budget Committee*

**Tools for Conviviality.** By Ivan Illich. (New York: Harper and Row, 1973. Pp. 110. \$5.95.)

Illich, in this his latest book, identifies a nearly idyllic future state. This state, the convivial society, is defined as ". . . a society in which modern technologies serve politically interrelated individuals rather than managers" (p. xxiv). The convivial society entails ". . . autonomous and creative intercourse among per-

sons, and the intercourse of persons with this environment; . . . individual freedom realized in personal independence" (p. 11).

The author is closely identified with the Centro de Información y Documentación (CIDOC) located in Cuernavaca, Mexico. The publications of those associated with CIDOC, including Illich's *Deschooling Society*, are critical of the structures and institutions of advanced capitalist societies. The thesis of *Tools of Conviviality* can be stated simply: Technology, originally a servant of man, is now more and more man's master. Illich finds this technologically conditioned lifestyle basically anti-convivial and hostile to the full development of human potentials. The United States is the prime example of an anti-convivial society.

To make his point about the increasing abuse of technology, Illich turns to the field of medicine. In an oversimplified analysis, he finds that doctors early in this century focused much of their energy on improvement of the quality of life through the use of a limited number of medicines and by working for water purification, personal hygiene, and control of rodent-transmitted diseases. Since 1955, he holds, the doctor-patient relationship has become increasingly impersonal; doctors spend increasing amounts of time treating doctor-induced diseases and in generating demand for additional medical services. Medicine increasingly has become man's master rather than man's servant.

This evolution from servant to master can also be seen in "education, the mails, social work, transportation, and even civil engineering . . ." (p. 26). Regrettably, Illich does not attempt an analysis of developments in these fields.

Having made the point that technology (tools) can become more master than servant of men, Illich proposes to "lay down criteria by which the manipulation of people for the sake of their tools can be immediately recognized, and thus to exclude those artifacts and institutions which inevitably extinguish a convivial lifestyle" (p. 14). While his discussion of the criteria is confused and rambling, he does include networks of multi-lane highways, strip mines, and compulsory school systems as tools which "inevitably increase regimentation, dependence, exploitation, and rob not only the rich but also the poor of conviviality" (p. 26).

Convinced of the validity of his analysis, Illich believes that the possibility of a convivial society can be transformed into a political program if it can be shown that "the structure of our tools menaces the survival of mankind . . . and that this danger is imminent" (p. 45). Rather than show that the apocalypse is near,

Illich presents a well-thought-out indictment of the evils of industrial development too detailed to summarize here.

The apocalypse, according to Illich, can be avoided only by a bureaucratic dictatorship or by engaging "in a political process by the use of legal and political procedures" (p. 100). The latter alternative becomes viable only in the context of what Illich calls a "multiple catastrophe," which he defines as a "crisis in the industrial mode of production" (p. 107). In the face of this catastrophe, a nascent and growing coalition advocating a convivial lifestyle would emerge as a majority, thus obtaining control of legal and political structures.

Illich's politics are as utopian as his vision of a convivial society. "No present government could restructure society along convivial lines" (p. 16). Although Mao's China receives favorable mention, the author contends that "different balances . . . in the distribution of goods and services can prevail in societies striving for conviviality" (p. 24). His notion of an impending multiple catastrophe in industrial societies is not convincing. *Tools of Conviviality* does, however, provide a brief and insightful critique of the problems of overindustrialization. Would that Illich had written more on that subject!

GAYLE AVANT

Baylor University

**Guillotine in the Wings: A New Look at the French Revolution and Its Relevance to America Today.** By Alex Karmel. (New York: McGraw-Hill, 1972. Pp. 237. \$6.95.)

It is difficult to tell whether this book is harder on the French Revolution or on "America today." In any case, it is a literary tribute to the banality of relevance. Karmel has written his account for those who "tend to think of the French Revolution as a bolt of lightning across the sky of history" (p. 3) and for those who believe it was "all blood and gore and heads popping into baskets" (p. 4). Having made clear that bolts of lightning and heads popping were the least of it, and having set the record straight on certain other crucial issues (e.g., "everyone gets older every year . . ." and "one nice thing about time, it moves only forward") (p. 15) of more concern to historians than social scientists no doubt, he gets down to the real business at hand: a "new look" at the "original granddaddy of all revolutions" (p. ix).

Presumably because of his commitment to relevance, Karmel has dispensed with the usual paraphernalia of scholarship—footnotes, references, bibliography—leaving the reader to trust in his rather unique credentials (several novels and a "fictional autobiography of Restif



de la Bretonne." Relevance supplies what scholarship has denied: "Joe McCarthy," for example, turns out to be "the portrait of a twentieth century Robespierre" (p. 69); Marat, on the other hand, "would have understood and applauded the prison revolt at Attica" (p. 115). Louis himself, for all "his good intentions . . . made a mess of everything" (p. 66)—an assertion which, I must admit, hardly seems to require documentation. Not that Karmel's account is exclusively *ad hominem*. Relevant ideas also receive attention. We learn, for example, that the idea of Rousseau's General Will was "one of those abstract ideas tucked away in philosophical works that can be dynamite with a long fuse" (p. 105). Indeed (shades of J. L. Talmon?), "the whole mentality of the modern totalitarian state was created overnight" (p. 161). Of course it took some time for this to be recognized because "there was no six o'clock news in Paris in 1793." This latter datum, though it will come as a revelation only to the "bolt of lightning" crowd, is a useful contextualist reminder to fans of Lovejoy who are forever reading universal themes into particular times and events. At least I think it is.

Toward the end of his remarkable rampage through history, Karmel confesses that he has "been led to use the kind of language that belongs more properly to fiction," presumably to explain such "language" as this: "even the most kinky forms of sex antedate Neil Armstrong's setting foot on the chaste bosom of Diana" (p. 227). (Relevance accounts for the subject, my point is the style). But Karmel does not do himself justice. For *Guillotine in the Wings* is not to be judged by the standards of historical fiction, much less the quaint requisites of serious scholarship. Its unusual virtues belong to an entirely different genre whose parameters are defined by Metro-Goldwyn-Mayer historical spectacles and the modern pulp novel. *The American Political Science Review* ought perhaps in the future to assign such works to Rex Reed for review. Or Spiro Agnew.

BENJAMIN R. BARBER

Rutgers University

**The Great Jackass Fallacy.** By Harry Levinson. (Boston, Mass.: Harvard University Press, 1973. Pp. 178. \$7.00.)

The Thomas Henry Carroll-Ford Foundation Distinguished Visiting Professorship of Business Administration at Harvard University was held by Harry Levinson from 1968–1972. During this period, Professor Levinson produced three books, in addition to the one being reviewed. Books that have artful titles, and that represent an integration of previously published

papers, are often less than able contributions to knowledge. This book is a happy exception to that rule.

Professor Levinson has made a significant effort in this book to present sound conceptual designs which add to the ever-growing literature in organization theory and behavior. Much of the book is based upon past articles published in the *Harvard Business Review* and in other journals. One new chapter (chapter 11) has been added.

From his long experience in organizational psychology, Levinson suggests that the dominant philosophy of motivation in American management is that of reward and punishment. In effect, this is an image of a carrot at one end and a stick at the other. The central figure of that image is a jackass to be manipulated and controlled. One may assume that in many instances university administrators and leaders of public and private bureaucracies hold this view. Organizational administrators assume that they are dealing with jackasses who are stubborn, stupid, and willful. Those who exercise leadership in organizations, and who operate within reward-punishment assumptions, imply that they have control over others, and that these individuals are in a jackass position in their relationships to them.

Levinson presents his own theoretical framework within which alternatives to the jackass fallacy can produce effective organizational effort. His main theoretical assumption is that of psychological man, which is based upon the product of four interacting forces (1) psychological motivation, (2) social role encompassed in the concept of work, (3) a modal organization structure within which people work, and (4) a broader ethos within which people function.

This book makes several worthwhile theoretical contributions to current organizational analysis. First, several parts of it deal with the implications of the emotional toxicity of work and of the responses of organization members to middle age and to loss. Given the fascination with change, planned or otherwise, which tends to dominate the present state of organizational knowledge, this book clearly argues that all change is a form of loss. Loss engenders important psychological and physiological consequences. Persons who suffer loss tend to recoup their losses and compensate for them. Second, two important recent theoretical approaches to organizational theory, Management By Objectives (MBO) and Organizational Development (OD), are systematically analyzed by Levinson. Many analysts have already raised the issue about whose objectives are determinants of

managerial philosophies as posited by MBO. We are aware that responsible practitioners of MBO are recasting its original assumptions. Certainly the objectives of an organization must be formulated by those at varied levels of the hierarchy, and not only by those at the top. In Levinson's view, MBO misses the whole human point. He suggests that OD often fails to engage in genuine organizational diagnosis. Its techniques are too often *ad hoc* and are based upon expedient assumptions. A diagnosis would require a comprehensive examination of the client's system. Interestingly, Levinson asserts that there is no systematic body of knowledge about organizational development. Recent volumes on OD have spent too much time trying to answer the question of whether or not OD is merely a fad. Third, Levinson deals perceptively with the problem of revitalizing a bureaucracy, with special focus upon the Department of State. Building upon the work of Argyris and others, he makes several pertinent points: (1) Bureaucracies cannot be changed from the outside, and (2) if bureaucracies cannot be changed by the top, by the inside or by the outside, the only conditions under which they can be changed is when there is pressure from all three. A mechanism of simultaneous task forces will bring the organization face to face with its realities. These realities would include the degree of support required from the consultant, the level of competence and skill to effect change, and the capacity to master problems.

This volume can be compared to the work of Chester Barnard in terms of its wisdom and ultimate contribution to organizational theory. It is to be hoped that those who did not read its parts when they were originally published will avail themselves of the materials contained in this book.

EDWARD R. PADGETT

*University of Cincinnati*

**II Liberalismo in un Mondo in Trasformazione.** By Nicola Matteucci. (Bologna: Società Editrice Il Mulino, 1972. Pp. 266. Lire 3,000.)

What is liberalism? Will any two scholars ever agree on an answer? Probably, if we are flexible enough in our characterization of this important movement. We American political scientists generally know what we mean by liberalism (with a small "l"), assuming that we have a modicum of familiarity with the history of political thought (admittedly a generous assumption in some quarters). We will have read Frederick Watkins, Guido de Ruggiero, and John Hallowell, and from them and others we

will know that, at its best, liberalism is that broad political outlook, commencing roughly with Grotius and Locke, and going through various transformations in response to different challenges, which welcomes the secularization of political life, individualism, consent, peaceful change, limited government, and moderation in all things.

Professor Matteucci, who holds the chair in the History of Political Doctrines at the University of Bologna, is well qualified to reassess the condition of liberalism in the light of its contemporary situation. He has written on de Tocqueville, McIlwain, and constitutionalism and possesses an excellent grasp of the literature on liberalism, both European and American. He has drawn inspiration from one of the greatest liberal minds of this century, Benedetto Croce.

Matteucci's book is felicitously written; its argument is urbane and undoctinaire. Nonetheless, for all its merits, the volume fails to give a sustained treatment of a few major themes. Instead, it flits briefly over a number of topics, both historical and contemporary, leaving the impression that the book lacks sufficient unity and depth to be accounted a major contribution.

One of the interesting subjects to which the author does devote more space than he does to others is the "challenge" which industrial or technological society holds for contemporary liberalism. Matteucci is eloquent in his advocacy of a "multi-dimensional society" over against the "one-dimensional society" of the messianic technocrats. He accurately assesses the ideas of St.-Simon, Comte, and their contemporary apostles, however, beneficent in intention, to be profoundly illiberal in their premises and in the results of those premises in the society in which we live.

Matteucci is right: everything considered, liberalism is the most decent, humane, adequate, and tolerant of the various political doctrines to appear in the modern period. It has endured for at least three hundred years and, as he suggests on pp. 73-74, has resisted, with considerable success, three great "challenges": those of political absolutism (in the eighteenth century), the "social question" (in the nineteenth century), and totalitarianism (in the first half of our own century). But will present-day liberalism be adequate to meet the challenge of "postindustrial" society, and the threat of the technocratic ethos to mobilize all desires, structures, and symbols in the name of the latest idols: productive efficiency and technical rationality?

These considerations are practical in nature,

and they raise the question of whether liberalism is a *doctrine* responding to the exigencies of the political struggle or a *philosophy* capable of transcending the immediate priorities of that struggle in order to consider the perennial features of the human condition. If liberalism is primarily a doctrine responding to immediate crises, then its conclusions, however effective practically, must theoretically have only partial validity, because they are inevitably *responses* to those immediate challenges and are conditioned by them. To understand why conservatives and radicals, for example, in vastly different ways find the liberal doctrine of man and society deficient would require a noetically critical perspective, which is to say a perspective different from that of even a most intelligent liberal publicist such as Professor Matteucci.

DANTE GERMINO

*University of Virginia*

**Imperialism and Revolution: An Essay for Radicals.** By Bernard S. Morris. (Bloomington: Indiana University Press, 1973. Pp. 81. \$5.00, cloth; \$1.95, paper.)

This is another product of the Vietnam war, an echo of the passionate debates and protests which rocked the campuses only a few years ago but now seem to belong to another age. Professor Morris writes as though he were speaking to a group of indignant students, accepting as much of their case as possible and using some of their language while gently explaining that the Leninist theory of imperialism as expansion dictated by monopoly capitalism really won't wash in the case of Vietnam. The refutation is mild; Lenin's political interpretation, which in an exact science would hardly merit the designation of "theory," is left seeming less logically deficient than it really is. The neo-Marxist theory of imperialism, he avers, is "the best we have" (p. 26). The author then proceeds to take apart George Liska's 1967, and now outdated, rationalization of the American role as world keeper of law and order. Morris, like the protesters, was profoundly moved by the wrongness of the American actions in Vietnam, impatient with the militarization of United States foreign policy, and doubtful of improvement without fundamental change in American society. He gloomily felt that the public had been so brainwashed that "change indeed seems almost impossible" (p. 57), and that "the governing class has become so powerful as to cast doubts on the viability of the democratic process . . ." (p. 35) Revolution is hence the secondary theme of the book. Unhappily, he does not find in neo-Marxism or

anywhere else a real promise of radical change; and he concludes with a wan hope of a new consciousness, à la Charles Reich.

This is all well written and generally well pondered, and the points made are mostly unexceptionable. But it does not tell us much. It is dubious reification to treat "imperialism" as a coherent, analyzable phenomenon. Many reprehensible actions of stronger states in relation to weaker ones may be called "imperialistic," but it is not clear that they necessarily have much in common. Professor Morris concedes (p. 50) that Roman imperialism, which had nothing to do with capitalism, was a very different breed from what he sees as American imperialism; but he does not discuss whether the colony grabbing of the late nineteenth century was really of a kind, in motives and methods, with the American role in Southeast Asia or the "neo-colonialism" through which Third World economies are dominated by foreign, largely American corporations without specific action by the American government. He notes (p. 39) David Landes's definition of imperialism as "a multifarious response to a common opportunity that consists simply in a disparity of power." But this is so broad as to be uninteresting; and it allows any action of the stronger power to be called imperialistic.

The whole discussion, in any case, has become passé with extraordinary rapidity as the U.S., chastened by the Vietnam nonsuccess, pulls inward. If this nation spent the past generation, during which it possessed always overwhelming economic power and usually overwhelming military potential, in empire-building, there is little to show for it, politically or economically. Almost powerless Arab countries have demonstrated how they can not only manhandle the supposedly dominant giant corporations but send a chill through the economy of the whole Western world; and the mighty imperialists hesitate even to mutter about countermeasures. No one can feel certain that premodern ways of thinking may not lure policy makers one day to new interventions, but the problems to which Professor Morris addresses himself no longer seem to be the great issues of the day.

ROBERT G. WESSON

*University of California, Santa Barbara*

**Modern Organizational Theory: Contextual, Environmental, and Socio-Cultural Variables.** Edited by Anant R. Negandhi. (Kent, Ohio: The Kent State University Press, 1973. Pp. 404. \$10.00.)

The twenty-two papers which comprise the present volume grew out of a series of four

annual conferences held by the Comparative Administration Research Institute at Kent State University between 1968 and 1971. At these meetings scholars in the field of organizational theory presented a wide variety of viewpoints in papers and discussions examining the impact of size, technology, environmental, and socio-cultural variables on organizational structure, behavior patterns, and effectiveness. Selected and edited in light of the discussion they generated, these papers provide a valuable survey, through both theoretical and empirical studies, of the currently developing research strategies and theories of organization.

The volume is divided into four parts. Each part comprises a number of original papers as well as a number of critical appraisals.

Part I is entitled "Contextual Variables: Size, Technology, Ownership, and Dependence." Here we find papers by Arlyn J. Melcher, Derek Pugh and Diana Pheysey, Charles Perrow, and Joan Woodward.

The first two papers are focussed generally on the impact of size, hierarchical structure, spatial-physical-temporal factors, leadership style, information flow, and control mechanisms upon organizational behavior and effectiveness. The latter two are concerned primarily with a definition of the concept of technology and the specific impact of technological factors on organizational functioning.

Part II is entitled "Environmental Variables and Interorganizational Focus." Included here are papers by Louis C. Gawthrop, Basil S. Georgopoulos, Jay W. Lorsch, Roland L. Warren, et al., Mark Lefton, together with a paper jointly authored by Paul E. White, Sol Levine, and George J. Vlasak.

The first two papers, by Gawthrop and Georgopoulos, explore the implications of major social and technical changes, both current and prospective, on governmental bureaucracy and hospitals, respectively. The next paper, by Lorsch, explores the effect of differing market and technological conditions on organizational structures and effectiveness. The fourth paper, by Warren and colleagues, explores the complex pattern of relationships between public school administration offices, health and welfare planning councils, community action agencies, as well as other so-called "community decision organizations." Lefton's essay explores the consequences for organizational behavior arising out of the specific interests which given organizations have in their client-organizations. The final essay, by White, Levine, and Vlasak, elaborates upon the utility of "exchange theory" for understanding interaction among social organizations.

Part III appears under the heading: "Socio-Cultural Variables." It includes papers by Michel Crozier, Piotr Hesselting, Richard N. Farmer, and, not least, a paper by Anant R. Negandhi, the editor of the present volume.

The paper by Crozier depicts the manner in which the power relationship in organizational settings is influenced by the sociocultural characteristics of the people involved in such relationships. The second paper, by Hesselting, reports on a multi-national study designed to cast new light upon the impact of four independent variables, namely, ecology, culture, technology, and the power structure, on organizational behavior and effectiveness. The essay by Farmer presents a classification schema by means of which various sociocultural, economic, political, legal, and educational factors influence organizational practice and effectiveness. And while this paper is cast on a macro analytical level, the last paper in this section, by Negandhi, attempts an alternative micro level conceptual scheme with which to examine the impact of environmental, sociocultural, and organizational variables on management practices and effectiveness.

The fourth and final part is presented under the rubric: "Introducing Change in Complex Organizations." Here we find papers by Gene W. Dalton, and Berton H. Kaplan, respectively. The former paper by Dalton marshalls the available evidence showing that significant alterations of individual behavior patterns are possible. The essay by Kaplan explores the problem of how the traditional bureaucratic model will have to be modified in order to adapt to the changes taking place in modern society.

Included also in this section is a short epilogue entitled "Beyond the Present Trends," by the editor, Negandhi, in which he shares his critical concerns for the future direction of organizational research. Negandhi deplores the prevailing emphasis upon structure at the expense of function, the apparent lack of concern about the linkages between important variables, and the general lack of interest in the processes by means of which organizations adapt and cope with environmental forces.

With all of this one can concur most heartily. For it appears that organizational theorists, as here represented, regrettably share with their sociological compeers a decided preference for methodological viability at the expense of theoretical adequacy, and for correlational rather than causal study designs. The reader will search in vain for any evidence of movement toward a processual approach which recognizes that any given item of behavior may very well

have both multiple causes and multiple consequences.

But despite the numerous shortcomings and limitations of the present collection of organizational studies, there is some warrant for both gratitude and optimism. For it does appear that the crucial image of an organization situated in an environment of other organizations to which adaptations must be made has at long last been permanently established in both organizational and administrative theory. This notion, having been forcefully and dramatically brought to stage-center, will hopefully inform and illumine the formulation of future research studies in ways that more effectively meet the criticisms of prevailing research designs and approaches. And to the extent that this constitutes a realistic expectation, it seems reasonable to suppose that political science in some or all of its specialized foci of inquiry, can look forward to an increasingly more effective rapprochement with a sociology of organization and administration.

HENRY ZENTNER

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**The Concept of Representation.** By Hanna Fenichel Pitkin. (Berkeley: University of California Press, 1972. Pp. 323. \$8.50, cloth; \$3.65, paper.)

Hanna Pitkin's volume on the concept of representation is the crowning achievement of many years of hard effort—a fine example of Hegel's *Anstrengung des Begriffs*. It encompasses the work of many others, and does so ably and fairly. It promises to become a classic in this central and important concept of political theory and related fields. Its writing is condensed and the presentation of difficult issues clear. A masterly treatment of which I can say that I would be proud to have done it. Not that I agree with all Professor Pitkin says; far from it.

Pitkin starts with a careful presentation and analysis of Hobbes's handling of representation. She designates it as the authorization approach, which starts from the individual but leaves Hobbes' individualism unanalyzed. I regret this, because it leaves the problems which she rightly points out as shortcomings of Hobbes's approach without solution. Hence, the reader is not told clearly that Hobbes is wrong. What was needed here was a deeper inquiry into the phenomenon of authority—political authority as rational elaboration. [For a more detailed statement of what is involved, see my *Man and His Government* which appeared in 1963 and might have helped Professor Pitkin in strengthening and clarifying her position.] Unfortu-

nately, like so many political scientists, she uses the term "authority" uncritically, and as it is customary in the legal tradition [see my *Tradition and Authority*]. I find myself handicapped here by my own work which laid a firmer foundation for representation than did the authors with whom she is primarily concerned. She justifiably defends herself against the accusation of being preoccupied with words, but tends to fall into the related error of being preoccupied with *one* word without regard to the family to which it belongs.

Her neat tracing of representation to its classical origins might usefully have been implemented by some attention to the question of why the Greek and Roman thinkers when confronted by the fact of representation did not explore its nature. The oft-remarked failure of the Greeks in the field of federalism is directly related to their lack of insight into the potentialities of representation. (Rome's failure is less often commented upon, but even more glaring and disastrous in accepting empire as the only solution of the problems which territorial expansion created.)

Professor Pitkin is laudably explicit in her critical comments upon the popular misuse of terms like representative government. Such terms have been important weapons in the Anglo-American armory of propagandistic manipulation of analytic terms. (Another striking example of this propensity is the talk about "rigid" versus flexible constitutions in adulation of parliamentary representation.) She rightly insists that in one perspective, every government is representative; she might have added that in another, none are. Yet, realistic political analysis has to face the question of more or less. Is Nixon (or any other public official) always representative to the same degree? If not, what explains the change? What, in other words, constitutes the representativeness of representatives? Is there some empirical ground for calling a proportional representation scheme more representative than another? Pitkin offers sane and insightful comments on these matters so hotly disputed and so fanatically proclaimed over the years. Here again I regret her failure to penetrate deeper than her single focus; for since the argument is conventionally cast in terms of "justice," what makes one representation more just than another or indeed *more* anything? There is a lack here of sensitivity to the *quantitative* dimension of representation. I am no fanatic of the quantitative approach, to be sure; but I do maintain that it is a significant perspective yielding new insights. This is as true for representation as for other basic issues of politics. An interesting practical illus-

tration is found in federal orders. Is it vital that the component units are represented according to their size, or may they have equality regardless of their size? Involved here is the familiar issue of one vote for each person. The U.S. is not the only federal system which for expediency's sake has been run on the basis of disregard for such numerical equality, and will continue to do so, despite the Supreme Court's preference for the "one man, one vote" principle.

In addition to its other great merits the book is well printed and provided with a good index and a bibliography. The latter is by no means complete; I missed some authors who have devoted much labor to the study of representation: Dolf Sternberger, formerly professor at Heidelberg; and Gerhard Leibholz, former federal judge at Karlsruhe and Professor at Goettingen. East European authors are almost entirely missing. It is a minor blemish in a work so thorough in its learning and so painstaking in its scholarship. I wish that Professor Pitkin might find time to explore the concrete experience of federal systems, past and evolving. More especially the European Common Market needs her analysis, and so does Federal Germany. And what about our own Puerto Rico? Is Puerto Rico properly represented as an associated free state? What about her resident commissioner? Popular election of this vital representative is supposed to solve the problems of Puerto Rico's representation. Does it? Finally what about the UN? Are the US and Nicaragua equal? And if not, how can they be properly represented?

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**Sex-Pol: Essays, 1929-1934.** By Wilhelm Reich.  
Edited by Lee Baxandall. (New York: Random House, 1966, 1971, 1972. Pp. 378. \$10.00.)

Political scientists, if they have heard of Reich at all, mistakenly associate him with sexual libertinism, or the notorious orgone therapy, for the promotion of which through the mails Reich was sentenced to a term in the Lewisburg federal penitentiary (where he died in 1957 at the age of sixty). If one regularly sat in a so-called orgone box, Reich believed, one could recapture the vital life energy that had been lost, and by that means regain health and productivity. The orgone itself, in effect, not to be confused with what Freud meant by *libido*, was orgasmic potency in quantifiable

form; unlike most of the early psychoanalysts, Reich recognized that ejaculation and orgasmic potency are not identical. By now this finding is broadly accepted, but where Reich went wrong was to assume that orgasmic energy was, like electricity, something that constantly was being released into the atmosphere and therefore could be recaptured. The orgone box allegedly trapped this energy before it could be further depleted, and returned it to the body.

It is of interest that this Reichian excursion into input-output analysis, if I may use that overworked expression, with reference to the psychobiophysiology of sex nowhere is mentioned by *Sex-Pol* editor Lee Baxandall or by Professor Bertell Ollman (who wrote an introduction). Nor do we learn anything about Reich's significant contributions to psychoanalytic theory or his repudiation toward the end of his life of much of what he had earlier believed about the relationship between sex and politics. Indeed the few references to Reich's troubled memberships in the German Communist Party (which ended with his expulsion in 1933), and in the International Psychoanalytic Association (from which he was expelled in 1934), are incorrect or misleading. Reich was in trouble with some of the leading figures in the IPA, although not apparently with Freud himself, long before he published *The Mass Psychology of Fascism* in 1933, and it is irresponsible if not dishonest of Professor Ollman to imply that his expulsion owed something, "as difficult as it is to believe today," to the IPA's hoping "to make their peace with fascism." No doubt some of the German members of the Association wished to avoid difficulties, but such wishes were doomed from the beginning even apart from the fact that Jews were prominent in psychoanalytic circles. Between psychoanalysis, which can not be understood except in the context of the Judaic-Christian tradition, and the paganism and nihilism of Naziism, or looked at another way, between the eros of Freud and the thanatos of Hitler, there could be no common ground, and with the exception of Jung, who cooperated with Goering until 1940, no prominent psychoanalyst went over to the Nazis.

Baxandall, of course, is much less interested in presenting Reich's credentials as either a Marxist or a psychoanalyst than in establishing him as the major theorist of sexual politics. "The Imposition of Sexual Morality" stresses the connection between capitalism and sexual repression and because this assumed relationship is crucial in the thinking of such apostles of revolution and/or counterculture as Roszak, Slater, Charles Reich, Goodman, and Marcuse,

among others, it merits careful reading. "Politicizing the Sexual Problem of Youth," which first appeared in 1932, seems rather dated today despite the efforts of Ollman to make it appear both topical and relevant. (How many present-day Marxists really believe that "If you want to be rid of your sexual troubles, fight for socialism. Only through socialism can you achieve sexual *joie de vivre*"?) Reich's essay "Dialectical Materialism and Psychoanalysis" is rather heavy going, and the remaining essays are more or less typical of much Marxist writing of the 'thirties, e.g., on the need to arouse the sleeping masses and inspire class consciousness in the labor movement. Perhaps I missed something along the way, but while reading these pieces I was more inclined to join the masses asleep than to arouse them.

The impact of Reich's central thesis about the relationship between capitalism and sexual repression is very different. Here Reich took issue with Freud's position in *Civilization and Its Discontents* (1930) that sexual repression was the inevitable price paid for cultural development, by arguing in accordance with Marxist principles that sexual morality like everything else was determined by economic forces and the material condition of society. Reich argued, in essence that sexual repression was the means by which the capitalist class gained control of the *unconscious* of the masses, and through this control insured that the will to rebel would be too inhibited and crippled ever to assert itself in a decisive manner. Indeed, the masses would never even know that they were being manipulated through the agency of sexual morality, believing, instead, that morality was grounded in religion, health, the preservation of the family, the sanctity of womanhood, etc.

Whatever one may think of Reich's assumption that sexual health is possible only under socialism, it is by no means certain that he was wrong about the relationship between sexual repression and submission to authority. There is some empirical evidence (an unpublished study by Lasswell and Rogow) that individuals who suffer from a variety of psychosexual conflicts are more apt to identify with established institutions and disassociate themselves from those who challenge such institutions, than individuals who suffer much less or not at all from psychosexual conflicts. In other words, it is possible that sexual repression *does* work in support of the authority institutions of society, whatever the ideology in terms of which they justify themselves. Certainly those ridden by anxiety and guilt do not make good revolutionaries, whether for social justice under capital-

ism or freedom and democracy under communism.

Were Reich alive today he would insist probably that there is no such person as a good revolutionary, and no such thing as a socialist society. Well before the end of his life Reich had ceased to think of himself as either a Marxist or a psychoanalyst in the usual sense, and instead, according to Charles Rycroft, had become a virulent anti-Communist and fervent admirer of Eisenhower. He apparently believed that his views about sexual repression had been perverted into "a free-for-all fucking epidemic" (Rycroft, *New York Review*, December 4, 1969), and he was lonely, bitter, and more than a little paranoid, to judge from an interview with Reich conducted by Kurt R. Eissler in October, 1952. As Reich remained self-exiled and remote in his Maine retreat, his recollections of the past became distorted, and his thought processes bizarre. Those who create in terrible loneliness, working in isolation from the mainstream or any tributary, are no less likely to hallucinate and lose touch with reality than those who live alone in bare rooms. In the United States, the fate of the dissident typically is not prison or a labor camp, as in the Soviet Union, but a kind of solitary confinement that can break him just as effectively as a term in jail. It is tempting but futile to speculate what might have become of Reich and psychoanalysis had he been allowed to remain within the Freudian circle, or what would have been the effect on Marxism had Reich been regarded by the Third International as a leading theoretician. Perhaps the end would have been the same. In any case, the tragedy of Reich's final years speaks for itself.

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#### **An Introduction to Positive Political Theory.**

By William H. Riker and Peter C. Ordeshook. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. 387. \$11.95.)

Seen in the broadest possible terms, politics is a social method for generating collective decisions from divergent and often discordant individual preferences. In this respect, it resembles economics. Every basic economics text emphasizes that three major allocative decisions—what to produce, how much to produce, and for whom—can be analyzed as the result of households, firms, and governments choosing to exchange goods and services in a variety of organizational settings that combine to form the web of interactive relationships abstractly called economic markets. Drawing much of their inspiration and specific technique from

scholars working on the ragged seam dividing politics from economics, Professors Riker and Ordeshook view the study of politics as arid and intellectually uninteresting unless it incorporates a theory of rational choice as a set of fundamental axioms from which more specialized theoretical propositions may be deduced. *An Introduction to Positive Political Theory* springs from the premise that human beings consciously strive to produce collective outcomes at least broadly consistent with their personal preferences. Without this crucial premise, Riker and Ordeshook claim, political science can never be more than a loose amalgamation of weak empirical correlations which present political activity as the mechanical reflex of class position, childhood training, or social location in various hierarchies or groups. Whatever theory emerges from this inductive method they hold, is nothing more than a compendious restatement of the latest empirical findings.

As might be expected from the books and articles they have previously published, Riker and Ordeshook are preoccupied with collecting and integrating bits and snippets of theories dealing with the summation and transformation of individual preference orderings into authoritative collective decisions having the force of law. (There is nothing novel about this theoretical focus; it has been a staple of political theory since Plato.) In my judgment, these chapters form the most solid and substantial portion of the book. The chapter dealing with Arrow's paradox is the clearest exposition of the General Impossibility Theorem I have ever seen in print. Although their conclusions are somewhat guarded, Riker and Ordeshook suggest that attempts to guarantee the transitivity of social choices by organizing vote-selling markets (logrolling) are likely to misfire. Logrolling is methodologically indefensible unless one introduces cardinal utility numbers which can be interpersonally compared and summed. They are skeptical of the von Neumann-Morgenstern technique for transforming an ordered set of preference ranking into an interval scale measuring *intensities* of preference, a point of view which I share. It is important to note that the rejection of cardinal utility numbers renders the use of mixed strategies in game-theoretical formulations questionable as well.

In a brief review, one must forego critique for summary. Riker and Ordeshook present an especially rich variety of models and theories developed by scholars dealing with cooperative and noncooperative games, the calculation of power indices in voting bodies where electors constitutionally cast unequal numbers of votes,

electoral competition in two- and multi-party systems, and processes of coalition formation in legislative bodies and international conflict. Their treatment of each topic is deft and subtle, and, where the data permit, Riker and Ordeshook are able to marshal an impressive volume of evidence to test the implications of their theoretical structures.

In treating policy outputs, the authors trenchantly analyze a number of contributions in the rapidly developing field concerned with public goods (or public "bads"), satisfactions or dissatisfactions produced in such a way that no one can be excluded from consuming them. Their handling of the "free rider" problem resulting from the indivisibilities inherent in public goods is standard but lucid. In the absence of coercion or pleasurable selective incentives, all will try to seize the satisfactions derived from consuming the good at the same time they attempt to shift the costs of providing it onto others. One result may be that the good will be supplied at a suboptimal level—how many people would voluntarily tax themselves to finance a fire department or a public park? Paradoxically, with governmental coercion, the polity may produce *more* of a public good than most people prefer. One homely example is a city so oversupplied with public roads that it becomes crowded, noisy, and smelly. Those trapped in the immediate neighborhood are made to suffer uncompensated adverse effects.

A definitive survey is always destined to be staked out as a definitive target, a sure sign of its importance. Political scientists who have been trained to view empirical testability as the prime requisite for intellectually respectable theory are likely to scorn the book as an elegant but misbegotten exercise in the manipulation of factually uninformative symbols. For most of the book, it is true, the correspondence between theoretical and observational languages is disappointingly tenuous. More serious, in my view, is the frequent failure of Riker and Ordeshook to specify evidence that would falsify their theoretical implications. In some cases, this omission invites the conclusion that the theories they present are actually pseudo-theories, vacuous tautologies consistent with whatever facts are discovered. For example, Olson's standard theory predicts that large latent groups will receive few collective benefits from governmental action. If they in fact become the beneficiaries of governmental action, the deviation from Olson's theory can be explained away by endowing legislative entrepreneurs with heroic propensities to assume large risks (pp. 75–76). If, however, the bene-



fits do not materialize, it is safe to conclude that legislative politicians are risk-averse. But, since the only evidence of a politician's appetite for risk are his actions in pressing for legislation, the theory manages to spin out of every counter-argument on its own internal circularity. What the formulation lacks is independent evidence of a legislator's taste for risk, a variable that is entirely exogenous to the theory.

Right off the mark, Riker and Ordeshook aver that their book is intended as an introduction "for students at the junior-senior and graduate levels" (p. xi). I am sorry, but I do not believe that statement for even a moment. My hunch is that the book will be rough sledding for most professors. It bristles with formidable talk of Lagrange multipliers and indifference contours, matters that are generally beyond the grasp of at least 95 per cent of all the incoming graduate students I have known. I hope my guess is wrong, because *An Introduction to Positive Political Theory* is far too rich and imaginative to merit only coterie readership.

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**Fascism in France: The Case of Maurice Barrès.** By Robert Soucy. (Berkeley: University of California Press, 1972. Pp. vii, 350. \$15.75.)

Maurice Barrès (1862–1923) is generally regarded as a conservative, a traditionalist, and one of the leading exponents of French nationalism during the two decades preceding World War I. During the war he was a fervent propagandist for the French cause; but after the war his reputation suffered an eclipse from which it has never recovered. Since his literary and political careers were antecedent to the epoch of Hitler and Mussolini, Barrès has not usually been described as a fascist. And yet the title of Soucy's book implies that he should be classified as a fascist of sorts. In the author's view, "A generation before Mussolini and Hitler came to power, Barrès, along with others, anticipated a cluster of fascist values, sentiments, and doctrines" (p. 313). As one follows Soucy's exposition, it becomes clear that the author finds affinities not just between the thought of Barrès and that of such French fascists as Pierre Drieu La Rochelle and Robert Brasillach, but between Barrèsism and Hitlerism as well. The latter connection is particularly emphasized in the chapter on "Vitalism, Massendemocratie, and Racism." But when he then explores at length the similarities between Barrèsism and Pétainism—and Gaullism for that

matter—it becomes clear that he has his own conceptions about the pervasiveness of fascist ideology.

Soucy, in the course of this work, seems to imply that Ernst Nolte in *Three Faces of Fascism* (New York: Holt, Rinehart and Winston, 1966) was mistaken in his choice of Maurras rather than Barrès as one of the principle fascist prototypes. Indeed, Nolte gives little attention to Barrès in his monumental study and suggests that if Barrès kept his political distance from Maurras it was because of his inability to accept the narrow-mindedness of Maurras and his fear that Maurras was preaching a doctrine of enslavement.

Soucy opens up a controversy, then, not only about who was the real *maître* of French fascism, but also about the role of Barrès in the genesis of European fascist ideology. The author, to be sure, notes certain dissimilarities between the thought of Barrès and that of the Nazis, of Pétain, and of de Gaulle, but the main thrust of his argument seems clear. Soucy, in a series of articles and now in this work, has undertaken to demonstrate that fascism and conservatism in France, while not completely identical, were more similar than different. It is in this light that his references to his subject's nationalism, conservatism, traditionalism, and national-socialism are to be understood. The fact that Barrès was something of an intellectual vagabond, that he moved from *Boulangisme* to a kind of national-socialism, then to traditionalist conservatism, and finally to Catholic mysticism, poses few problems for Soucy in his analysis of the similarities between Barrèsism and fascism. Indeed it seems to be his major purpose to take up a position in opposition to such writers as René Rémond and Paul Sérent, who see a fundamental difference between French fascism and conservatism.

Some of the French fascists, and particularly the literary fascists, in the nineteen thirties occasionally referred to Barrès as one of their mentors. They also attempted to annex Fourier, Proudhon, and even Péguy. But, in all such efforts, they were eagerly trying to demonstrate that, far from being only the servants of foreign powers, they were in step with an authentically French tradition. After quoting extensively from writings of Drieu and Brasillach (and Maurice Bardèche) that seem to coincide with aspects of the thought of Barrès, Soucy does point to the fact that Barrès "reconciled himself more and more to bourgeois society, becoming one of its staunchest defenders" (p. 300). He is right in acknowledging how far Barrès was from the French fascists on the question of the bourgeoisie, but this in itself

would seem to be of capital importance in distinguishing conservatives from fascists.

To say that the thought of Barrès had a curiously diffused impact on Frenchmen of a variety of persuasions would have sufficed. But to argue that he was a major intellectual forerunner of fascism raises further questions about the nature of generic fascism that this book will not put to rest. Soucy seems intent on proving a thesis, but whether he is entirely successful or not will depend on the reader's preconceptions concerning fascism. His book is well researched and he has an admirable, straightforward style despite an annoying repetition of the adjective "tough-minded." Still, Henry Ashby Turner's hypothesis that the term "fascism," used generically, serves more to confuse than to enlighten and that it therefore might be abandoned altogether, irresistibly comes to mind.

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**Main Trends of Research in the Social and Human Sciences.** Edited by UNESCO. (Paris and The Hague: Mouton-UNESCO, 1970. Pp. 819. \$30.00.)

By arranging for the compilation, editing, and publishing of this monumental volume, UNESCO has provided scholars with an extraordinarily informative and useful overview and critical survey of a broad field of knowledge, together with impressive evidence of limited but significant progress and growth in much of it.

Large as it is, the present volume is limited in the main to only six of the social sciences, each of which is treated in a substantial chapter: *sociology* (105 pages) by P. F. Lazarsfeld; *political science* (59 pages) by W. J. M. Mackenzie; *psychology* (58 pages) by Jean Piaget; *economics* (168 pages), begun by the late Oscar Lange, continued after Lange's death for a time by W. Brus and two other Polish scholars, and eventually completed by the anonymous staff of the UNESCO Secretariat; *demography* (68 pages) by Jean Bourgeois-Pichat; and *linguistics* (44 pages) by Roman Jakobson. As can be seen, economics and sociology together make up about two-thirds of the total.

A particular strength of the volume is the attention devoted to problems common to the social sciences. This begins with an extremely worthwhile 60-page "Introduction" by Jean Piaget on "The Place of the Sciences of Man in the System of Sciences"; and it continues throughout the second part of the book, which comprises four substantial chapters dealing

with interdisciplinary problems. In Chapter 7, Jean Piaget continues his major intellectual contribution to the entire work by discussing "General Problems of Interdisciplinary Research and Common Mechanisms."

In the next chapter, one of the most valuable of the entire book, Raymond Boudon deals with great competence and insight with "Mathematical Models and Methods." Pierre de Bie follows in Chapter 9 with a treatment of problem-focused research; and Stein Rokkan contributes a valuable chapter on "Cross-Cultural, Cross-Societal and Cross-National Research," in which he manages to condense a great deal of thought, research and experience.

The third and final part of the volume consists of a single massive chapter of 120 pages by Eric Trist on "The Organization and Financing of Research." This chapter is perhaps particularly important because the entire volume, in the words of the Director-General of UNESCO, René Maheu, is intended not only for the educated reader, for research workers themselves, and for their professional associations, but also for those institutions, both national and international, whose task it is to *organize and finance scientific research* (p. x; italics supplied). In this sense, the UNESCO volume is also a bid to exercise some power and control over social scientists and their work, in favor of such urgent social needs and promising intellectual trends as are perceived by the authors.

The entire volume is intended to deal only with law-seeking or nomothetic sciences of man, which seek verifiable and cumulative knowledge. A second volume, presumably of similar format, is to follow and to deal with the normative, historic, philosophic, aesthetic, and generally humanistic approaches to the study of man, in which the values and personality of the investigator tend to play a much larger part. It is to be hoped that those parts and practitioners of political science who are working in this predominantly humanistic tradition of our field—it is an ancient and honorable one—will be represented by a chapter in that future volume, too.

Among the many issues raised by a volume of this size and scope, three stand out: (1) the general intellectual merit of this work; (2) the particular quality of its treatment of political sciences; and (3) the possible implications of its line of reasoning on social science policy and financing.

As to the first point, the intellectual quality of the book is remarkably high. This is by no means the usual fat compendium held together by little but the efforts of the bindery. On the

contrary, it is a serious, first-rate work full of highly relevant ideas. The intellectual influence of Jean Piaget appears in many places throughout the book, in addition to the three chapters he has written for it. Together with the contributions by Raymond Boudon and Paul F. Lazarsfeld, these chapters would make the book even at its high price a bargain.

The long chapter on economics seems to offer the noneconomist a very useful and open-minded overview of economic doctrines, problems, and methods of research and analysis; and many of the points it raises—such as the still persisting lack of an adequate theory of economic development—should prove challenging and stimulating also to professional economists. A particular contribution of the chapter on economics is its undogmatic discussion of different schools of economic thought, including both non-Marxist and Marxist views and treating them in a calm, intelligent, and problem-oriented manner. It exemplifies the kind of significant intellectual contribution an organization such as UNESCO can make, and it reminds us that some of the greatest problems facing mankind are not those that divide East from West and North from South, but those that all of us have in common.

In the panorama of social sciences, as presented in this volume, political science has been allocated a very modest place. Not only the limited number of pages but also the lists of consultants and collaborators bear this out. Among these in sociology, we find many active researchers and theorists, such as Robert Angell, François Bourricaud, Ralph Dahrendorf, K. Davis, S. N. Eisenstadt, Johan Galtung, T. H. Marshall, Talcott Parsons, Neil Smelser, Otto Stammer, and Wolfgang Zapf; in psychology, there are Marie Jahoda, Otto Klineberg, Charles Osgood, Jean Stoetzel; but in political science there are only two names with similar records of major substantive contributions to their field, Raymond Aron and Stein Rokkan, both of them also claimed by the profession of sociology. This is compensated only in part by the inclusion of a number of meritorious functionaries of political science organizations, such as the Secretary-General of the International Political Science Association, André Philippart; of his predecessor, Serge Hurtig; of such leading functionaries of French political science organizations or institutions as J. Meyriat and the late J. Touchard; and of some respected scholars from smaller or non-Western countries, such as J. Steiner from Switzerland and R. N. Trivedi from India. My list is incomplete, and I apologize in advance for the possible omission of the name of some

fine scholar, but the contrast to the broader representation given in this volume to other fields remains manifest. It is much less a reflection of our field than of the limitation of UNESCO's consulting process in this instance.

An additional peculiarity is the almost complete absence among the UNESCO consultants of political scientists from the United States. Since more than one-half of the world's political scientists professionally engaged in research or teaching are in the United States, their absence from, or extreme scarcity among, the collaborators listed seems somewhat surprising.

Under these conditions it is doubly impressive that a British scholar, Professor W. J. M. Mackenzie of the University of Manchester, has written as excellent a chapter on political science as we have in this book. His treatment is broad, tolerant, shrewdly critical, and eminently fair. Throughout, there is a stress on the unity of our discipline, not as a matter of pious hopes but as one of instance after instance of demonstrated intellectual cross-connections and interdependence among the different analytic perspectives and approaches. An enterprising publisher could do a great deal worse than reprint this chapter as a brief paperback pamphlet.

Within this overall positive appraisal, some critical points deserve to be raised. The author stresses "three major changes which are irreversible": (1) political scientists have learned the language of variables; (2) they are learning "that models—if they are used—should be used scrupulously," but that "the magic resides in rigor, not in symbolism"; and (3) there is a new appreciation of the importance of quantitative data which "can be stored and analyzed electronically," even though "serious problems" remain about their reliability and completeness. Nonetheless, Professor Mackenzie also seems to accept the late 19th-century distinction between "nomothetic" or law-seeking sciences, such as the "natural" sciences, on the one hand, and "idiographic," or quality and uniqueness-oriented disciplines, such as the "cultural sciences" or humanities, on the other. The distinction was proposed by the German scientists Heinrich Rickert and Wilhelm Windelband in the late 19th century, and effectively criticized by the British historian R. G. Collingwood in his book *The Idea of History* in 1945. One wishes that Professor Mackenzie had found space to tell his readers about this criticism, and about the opposing viewpoint of the "unity of science" movement of the 1930s and 1940s.

Finally, Eric Trist's recommendation in the last chapter of the volume for science policy and the channeling of financial support should be read with great but critical care. After pre-

senting a variety of data from various countries, Trist concludes that "academic individualism" is outmoded. The future, one gathers, should belong to larger teams and organizations, avoiding "duplication" of research and directing their efforts according to plan and in response to the wishes of their financial supporters and their clientele. As the problems to be studied become larger and more complex, he argues, they will transcend the capacities of a single human mind, and social scientists will have to form large teams, and their work will have to be supported for decades. All this, in turn, will make social scientists more dependent on support by governments or clients and make "scientific individualism" still more obsolete.

These arguments, in my view, mix insights and oversights. What is the frequency and distribution of errors made by planners of research—by individuals, by committees, by bureaucracies, and by the opinion of large groups? If these error rates are not negligible, what will be the probable results of pyramiding one decision upon another, as is often inevitable in research? Might it be sensible to budget some proportion of resources for "nonobvious" research projects—perhaps those endorsed by at least one-third, but by less than one-half, the members of a well-qualified reviewing board? Under the system of "academic individualism," nonobvious or currently unfashionable lines of inquiry still have some chance of being pursued. How would they fare under a system of centrally planned priorities, or of increased client's "co-determination"?

There are as yet few conclusive answers to these questions. We cannot accept Mr. Trist's prescription, I think, as it stands, but he does give us, here and now, much food for thought.

So, clearly, does the entire book. As a whole, it is an important contribution. No respectable library should be without it; and any social scientist or general student of human nature, if temporarily in funds, might consider it as a valuable investment.

KARL W. DEUTSCH

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**Regicide and Revolution: Speeches at the Trial of Louis XVI.** Edited with an Introduction by Michael Walzer. Cambridge Studies in the History and Theory of Politics. (New York: Cambridge University Press, 1974. Pp. 219. \$12.50.)

Why should we care today about the judicial murder of a king almost two hundred years ago? Michael Walzer gives us several reasons in the long and thoughtful essay which opens this book.

Professor Walzer argues that the renunciation of ancient authority requires a clear symbolic action of its own, and that the founding of a new regime is unlikely to be successful unless the old has been put formally and publicly to rest, not just swept quietly into a corner. "Public regicide," he says, "is an absolutely decisive way of breaking with the myths of the old regime, and it is for this very reason, the founding act of the new" (p. 5). Such decisiveness, he asserts persuasively, is essential if a people is to recognize and accept that they have renounced their servitude and taken on new responsibilities.

According to Walzer, France and England, by breaking boldly with the past through the formal killing of the king, prepared themselves better for their political future than did peoples who dealt with kings in a less clearcut fashion. He raises some interesting questions about political psychology, the uses and powers of myths in politics, and the ways in which they can be finally retired. He also, with his customary wit and elegance, makes us take seriously some rather archaic problems in political procedure: How indeed does a people go about killing a king legally and formally, with appropriate ritual and some sort of legitimacy to grace the act? Finding the language and the laws, the proper accusations and the proper punishment, was a most delicate job for the French as it had been for Cromwell's men the century before.

Walzer, well versed in the behavior of the Puritans, draws comparisons between the trial of Louis and the trial of Charles Stuart, and uses both events as a basis for his generalizations. He gives a clear idea of the extraordinary audacity and political imagination it took for Cromwell and the People of England to say "We will cut off his head with the crown on it" and then carry off the deed. The distinction between the public and private capacities of the king, familiar for so long, was never more important than at that moment. To kill a king in private, to smother a man in a tower or stab him in the road—all this was commonplace. But to kill the kingship and not just the king was quite a different matter.

When one has finished Walzer's essay one has the sense of having learned much about the ways in which a people come to terms with their most powerful public symbols. The speeches themselves, ably translated by Marian Rothstein, are less rewarding. There is much rhetoric about the blood of tyrants, much screwing up of courage so that the deed may be done, a certain air of whistling in the dark, but very little theory. This is what we might ex-

pect of men in such a situation, I suppose. But given the long and carefully protracted tradition of discourse in France for several centuries about the origins and limits (if any) on the kingship, one might have hoped the French would do a better job of ornamenting with philosophy, or at least with finer language, what they wanted to do anyway. They carefully skirted, for the most part, the major issues. What right had they, this particular group of men, to be pronouncing sentence anyway? For whom did they speak, and how did they relate to that mystical entity, the French People? For what, on what charges, was the king being tried? For treason? Against whom, and in what form? For pillaging and cheating and making war to defend himself? Or only, after all, for being king?

This of course was what it came to in the end, as Walzer recognizes. The deputies themselves could hardly be expected to say it openly. To justify killing Louis they had to strip him of his divinity. Destroying the kingship was such a radical act, it seems, that to be able to perform it the deputies had to move violently from one extreme to the other. The king had been thought almost a god; now he must be made to seem a monster, less than human. He must be decked out in the most horrid colours, charged with the most awful crimes, his hands shown to be stained with blood, in order to be a fit object for execution.

Saint-Just and Robespierre, less squeamish and more direct, if no more logical, than their colleagues, were readier to talk about what was really going on. Robespierre put it this way: "Regretfully I speak this fatal truth—Louis must die because the nation must live" (p. 138). While Louis lived, the fragile new republic could not hope for tranquillity or for safety; these things required first the punishment of the tyrant. Saint-Just put it even more succinctly: "The Revolution begins when the tyrant ends" (p. 176).

It is hard to avoid drawing parallels between France in 1792 and America today. The useful comparative lessons for the most part have to do with political psychology and the ways people learn to understand and change their sacred symbols. This is one reason why Walzer's essay is so thought-provoking. More specific parallels are few, but there is a delightful irony about the homespun speech by Tom Paine which closes this selection. Paine argues that Louis though guilty not be killed, but exiled to America. "Let then those United States be the safeguard and asylum of Louis Capet. There, hereafter, far removed from the miseries and crimes of royalty, he may learn, from the

constant aspect of public prosperity, that the true system of government consists not in kings, but in fair, equal and honourable representation" (p. 212). *Sic semper tyrannis*.

NANNERL O. KEOHANE

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**The Power Motive.** By David G. Winter. (New York: Free Press, 1973. Pp. 373. \$12.00.)

Psychologists and political scientists have common concerns but dissimilar approaches. This is well illustrated by Professor Winter's book, which seeks to do for power what David McClelland has done for achievement. Winter's stated aim is to identify, measure and understand the power motive. He approaches the task from an empirical rather than a conceptual perspective, seeking, as he puts it, to avoid semantic questions and to "map the range of actions correlated with the power motive." By the standards of his discipline, he identifies and measures well. There will be those, however, who question the extent to which he understands.

The early chapters set forth Winter's conception of power (the capacity of one person to produce consciously or unconsciously intended effects on the behavior of another person) and his strategy for measuring it (recording and classifying the images and themes people set down in TAT protocols after the power motive has been experimentally aroused). These chapters are models of how a psychologist goes about explicating and operationalizing a concept. They also reveal Winter's penchant for exhaustively detailing all of the alternatives considered prior to each major research decision. His strategy, which admittedly has intuitive and logical appeal, is first to develop a cautiously constructed and elaborately validated measure of the power motive (chapter three). The next step is to correlate this measure with a wide variety of behaviors and characteristics to uncover those most closely related to the power motive (done largely with data collected from Harvard and Wesleyan undergraduates, chapters four and five). Finally, the strategy calls for using these empirically established patterns of thought and action as guides to refining the definition of power (chapter seven). Winter performs the first two operations admirably, but the third receives short shrift. He never really pulls all the empirical threads together into an integrated conception of the power motive, which is not surprising given the comparatively little attention he devotes to the effort.

Many of Winter's research findings are of interest to political scientists. Among students,

he finds significant correlations between the strength of the power motive and participation in competitive athletics, and the tendency to seek and hold campus political offices. Among adults, however, he found little difference between aspirants to political office and nonpolitical control groups as regards the strength of the power motive. Winter finds this surprising, and concludes that the traditional assumptions concerning the attraction of politics for the power-hungry may require modification. In a related study, Winter correlated *n* power with career choice among samples of Wesleyan and Harvard undergraduates. He found, surprisingly, that students high in *n* power are not especially drawn to government, politics or law, but prefer careers in teaching, psychology or the clergy. He suggests that this might be explained by the greater opportunities for individual autonomy such careers offer. Winter also attempts to discern, from his own and others' data, patterns in the strategies followed by high *n*-power people as they search for power in various settings. The data suggest that the interpersonal styles of such people include a proficiency in becoming well-known, a capacity for forming useful alliances, and a competitive disposition toward other power-oriented people.

One of Winter's most creative and interesting undertakings is his analysis of the legend of Don Juan as a device for generating additional hypotheses about correlates of the power motive. The legend is seen as an archetype of the power motive, in that it embodies such dramatic power themes as seduction, murder, challenge to honor and glory. After culling the legend for power imagery, Winter reports elements of his student data which pertain to the themes, e.g., those high in *n* power report greater frequency of sexual contact than others, and have a preference for unassertive, dependent wives. They are also resistant to illusions created by others, and report a greater desire for career autonomy, and generally for avoiding the dominance of others. At a sociopolitical level, Winter finds a moderate correlation between a country's production of Don Juan style stories and engagement in wars.

Winter's concluding efforts to examine the power motive in social and political contexts are disjointed but suggestive. He argues for example, that motive levels can affect the direction and characteristic orientation of socially or politically active groups, reporting data which suggests that high *n* power groups like black militants seek to make the system work for them, and hence support the system, while low *n* power, high *n* achievement white radicals

tend to seek fundamental changes in values, and hence the potential overthrow of the system. Winter also re-examines his and Donley's coding of power and achievement imagery in presidential inaugural speeches. He notes a high correlation between need for power and need for achievement, and suggests it is an outgrowth of the political system. This is because candidates who win the Democratic nomination have usually possessed high levels of both motives, while success as a Republican has generally implied low intensity in both motives. The system, he argues, tends to freeze out those high in one motive but low in the other.

This book will be of interest to those political scientists concerned with the psychology of power and with techniques for assessing its strength as a human motive. Those seeking an integrated conceptual treatment of power however, are advised to look elsewhere. For an important shortcoming is that in seeking the broadest range of empirical correlates of power, Winter pays a price in depth and cohesion. The work lacks integration. The disparate power correlates are never systematically pieced into a meaningful theoretical mosaic.

BRUCE BUCHANAN, II

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**5 Public Philosophies of Walter Lippmann.** By Benjamin F. Wright. (Austin: University of Texas Press, 1973. Pp. 171. \$6.75.)

In this little volume Professor Benjamin F. Wright has neatly, if at times prosaically, summarized and criticized nine of Walter Lippmann's books that are of special interest to the political philosopher. His central argument is that—in these nine (*A Preface to Politics*, 1913; *Drift and Mastery*, 1914; *Public Opinion*, 1922; *The Phantom Public*, 1925; *A Preface to Morals*, 1929; *The Method of Freedom*, 1934; *The New Imperative*, 1935; *The Good Society*, 1937; and *Essays in the Public Philosophy*, 1955)—Lippmann offers five distinct points of view concerning such issues as the capabilities of political man, the functions of government and laws (human and natural), the concept of property, the role of science, and the uses of history. Wright outlines Lippmann's assumptions, methods and conclusions as the latter moves from an optimistic progressive in the mold of Theodore Roosevelt and Herbert Croly, through a searching examination of the nature and role of public opinion, from a brief demotion of the role of government to a broker or mediator in *A Preface to Morals* to an enthusiastic endorsement of the first two years of the New Deal, then to a critique of FDR's policies in *The Good Society*, and finally to an

almost nostalgic defense of the tradition of civility and natural law in *The Public Philosophy*.

Though Professor Wright concludes with the caveat that consistency in thinking about politics is not necessarily a virtue, he nonetheless insists that Lippmann's works on political theory contain so many "interstitial variations, modifications, and reversals that it is difficult, perhaps impossible, to find adherence to any discernible basis for formulating a philosophy of politics that deserves the respect and the standing easily accorded to numerous portions of the books" (p. 135). There would be obvious benefits in seeking to understand why Lippmann modified his assumptions, shifted his focus, or changed his priorities during the forty-two years spanned by the nine volumes, but Wright has not analyzed possible explanations. Rather he has approached his subject as though Lippmann were primarily a theorist rather than a political journalist. Walter Lippmann has, without doubt, aspired to contribute to our thinking about the problems of liberal democracy, and these nine books should therefore be measured against the major contributors to American political thought including Madison, Jefferson, and Calhoun as well as the most impressive among his contemporaries. But to find him deficient because he has been less consistent, less accurate in his interpretations of history and literature, and less eloquent or substantive in his rhetorical flourishes than the Founding Fathers, is to give him less than his due as one of the most significant American political thinkers of the twentieth century.

Wright concedes that Lippmann is "the most versatile, the most wide ranging, and in some respects the most impressive" American political thinker of this century (p. 9). He finds him "the most interesting political theorist of his time," though not necessarily the most influential (p. 157). Influence in the realm of ideas usually requires a clear and consistent theme articulated over many years; it also demands that the commentator focus primarily on the dominant public concern of his era. Walter Lippmann does not meet either of these requirements. He has adjusted his focus and even his conclusions as time and events impressed him with the need to modify earlier views. No doubt he would respond to Professor Wright's complaints as he reacted to the experience of reading, during a late summer evening in 1915, an unnamed author of "one of those books on politics which are regarded as essential to any sort of intellectual respectability." In his imaginary dialogue with the pretentious and prosaic author, he observed: "You are drawing

sketches in the sand which the sea will wash away . . . you apologize for your timidity by frowning learnedly on anyone who honestly regards thoughts as an adventure, who strikes ahead and takes his chances. You are like a man . . . trying too hard to make a good mashie shot in golf. It can't be done by trying so hard to do it. Whatever truth you contribute to the world will be one lucky shot in a thousand misses. You cannot be right by holding your breath and taking precautions." ("Books and Things," *The New Republic*, 4 [August 7, 1915], p. 24).

By taking "a number of shots" at most of the central problems of political philosophy, Walter Lippmann has probably come closer to the truth than those who have carefully calculated their few dramatic attempts. Examine, for instance, his ambivalent and shifting views on the capabilities of political man: an unambiguous view of the role of human nature in political affairs would be a naive one. Some men, individually and collectively, have demonstrated their potential for sensitivity, compassion, and creativity while others have shown their propensities for irrationality, inhumanity, and destructiveness. The same ambiguity is no doubt justified in defining the functions of government and laws. Different eras demand different reactions from executives and legislators. In times of economic crisis or violent social conflict, the active role advocated in *The Method of Freedom* and *The New Imperative* will be necessary; following the resolution of such crises, a lower governmental profile—that of an occasional broker or mediator among private interests as proposed in *A Preface to Morals* and *The Good Society*—will probably be in order. Although he has not explicitly justified his shifting views of political man and the functions of government in this fashion, the style and method of Walter Lippmann's commentaries on politics suggest that this rationale of "constructive ambivalence" might well appeal to him.

JAMES LARE

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**John Locke: Problems and Perspectives.** Edited by John W. Yolton. (Cambridge: Cambridge University Press, 1969. Pp. 278. \$14.50.)

The expanding community of Locke scholars should be gratified by the appearance of a volume such as this. It provides welcome evidence of a vigorous exchange of views among five British, five North American, and two continental scholars representing a broad range of disciplines. Collectively, the contributors to this

volume approach Locke's work in its totality, rather than from within the Balkanized and zealously guarded frontiers of contemporary academic disciplines. They are generally well suited by training and experience to explore the relationships between the political, educational, religious, economic, epistemological, and other aspects of Locke's writings. Included among the essayists are four political scientists: Richard Ashcraft, Peter Laslett, Gordon Schochet, and M. Seliger; three historians: James Axtell, Esmond S. De Beer, and John Dunn; two professors of English: Hans Aarsleff, and Rosalie Colie; and three professors of philosophy: Raymond Polin, W. Von Leyden, and John Yolton. The genesis of the volume was a conference on "The Thought of John Locke," but it should be emphasized that it does not consist of unedited conference presentations, as do far too many publications of this type. The conference papers included in the volume were reworked for publication, while additional essays were commissioned by the editor, Professor Yolton, whose reputation for high standards of scholarship is attested by his contribution to this volume (pp. 183-193).

Given limitations of space, it seems appropriate to concentrate here on those essays that focus most directly on the concerns of political scientists. These essays especially remind one that the analysis of Locke has given rise to extraordinarily sharp controversy since his major works were published—mostly anonymously—late in the 17th century. Fresh and significant evidence that Locke anticipated such controversy is provided by Dr. De Beer, who provides an illuminating account of the extreme caution he took to conceal his authorship of the *Two Treatises of Government*. Locke "not only evaded his friends' inquiries; the extant correspondence . . . suggests that he was at pains to conceal his authorship from the publisher, Awnsham Churchill . . . [who] was not to see a scrap of writing in Locke's hand . . ." (p. 35).

Locke's efforts to avoid controversy proved unavailing. His "strange doctrines" provoked the dreaded "storm" of criticism to which he referred in his reply to the Bishop of Worcester, who charged that his "'new way of ideas' undermined the principles of Christianity" (p. 198). Professor Ashcraft suggests that "The general consensus of opinion among those who have read the *Essay Concerning Human Understanding* in the last two centuries would probably agree with the bishop as to the subversive character of Locke's exploration of the foundations of religious belief" (p. 194). No less a friend of Locke's than Sir Isaac Newton attacked him in a letter in which, as Newton sub-

sequently explained, "I took you for a Hobbist" (p. 199, n. 2). The charge of Hobbism was especially threatening, since, as Dr. De Beer reminds us, by "1680 *Leviathan* was completely discredited; . . . the book was an atheistic vindication of Cromwell" (p. 40). Locke always stoutly denied that he was even "well read in Hobbes,"—much less influenced by him, but attacks based on what might be termed the "hard" perspective or interpretation of his work continued during his lifetime and throughout the 18th century (p. 45).

Granting that this "hard" perspective may not provide an adequate understanding of Locke's intention, it is nevertheless striking how little serious attention it receives from most contemporary interpreters of Locke, most notably the essayists who have contributed to this collection. They thereby severely limit the scope of their interpretation. Still, to give them their due, one must observe that it is Locke himself who provided the basis for the currently dominant interpretation of his work. Locke's writings are replete with quotations from scripture and his professions of Christian piety can hardly fail to capture the attention of even the most superficial readers. One is therefore hardly surprised to observe that Professor Polin in his essay on Locke's "conception of freedom" observes that "The metaphysical and even theological dogma of eternal salvation constitutes indeed the foundation and the right justification for the existence of freedom" (p. 4). Professor Ashcraft advances this position with his suggestion that "All the roads of Lockean philosophy lead to the hallowed ground of Christianity" (p. 214). Ashcraft finds that Locke occasionally encounters obstacles on this road, but they do not prove to be insurmountable. For when "the epistemological views of Locke, the Christian, cannot be satisfactorily reconciled with those of Locke, the philosopher, it is the faith of the former which ensures the salvation of the latter" (p. 223). Indeed, "at critical points in the exploration of human ignorance" in his *Essay Concerning Human Understanding*, "Locke speaks less as a philosopher than as a prophet . . ." (p. 204).

Writing in a somewhat different vein, Professor Schochet attempts to establish the interesting thesis that "an important and persistent acceptance of what is at base Aristotelianism can be extracted from Locke's conceptions of society and government . . ." (p. 89). His failure to explain what he understands here as "Aristotelianism" makes it difficult to evaluate his argument. Aristotle would hardly have settled for the extremely vague proposition that man is "somewhat sociable by nature," (p. 92,



emphasis added), or have accepted the view that "By looking at man in his original, pre-political condition" one could properly "determine what kind of government would be most consistent with human nature," as Locke is said to have done (*Essay Concerning Human Understanding*).

Schochet also places considerable reliance on the notion that "the doctrine of trust was one of the most important components of the theory of the *Two Treatises*" (p. 98). But he fails to develop the grounds for this view. His attribution to Locke of the position that men, even outside of civil society, were "other-regarding rather than hedonistic" (p. 96) stems from his questionable identification of the state of nature and the character of rule in primitive or undeveloped communities.

Many of the foregoing themes receive their most careful and complete development in Professor Aarsleff's two substantial contributions to this volume. The tone of Aarsleff's argument is suggested by his contention that "for Locke the Bible was the revealed word and as such the ultimate authority in all human matters. To believe otherwise is to ignore the plainest evidence in all of Locke's writings" (p. 130, n. 1). Aarsleff rightly assures us however that "Locke had . . . strong reasons for not wishing to found his social and political philosophy on special revelation" (p. 105), and he finds in his work "complete harmony" between "two kinds of revelation, special and manifest . . ." (p. 105) or "natural revelation, whereby the eternal Father of light and foundation of all knowledge communicates to mankind that portion of truth which he has laid within the reach of their natural faculties" (p. 107). Aarsleff agrees explicitly with a portion of Polin's analysis (see p. 110, n. 4) and with Schochet's finding that Locke is not a hedonist (pp. 111-116). He argues that through the use of their God-given reason some parts of mankind have always been able to overcome the promptings of the passions to gratify their immediate desires and thereby to achieve true happiness. This is nothing less than "the exquisite and endless happiness" that is the anticipated reward of those who have earned Eternal Salvation, as Locke tells us in the *Essay*.

But what is the status of Eternal Salvation in Locke's teaching? Aarsleff's inspiring interpretation depends decisively on whether Locke has demonstrated the immortality of the soul. Unfortunately, according to Locke, its demonstration is beyond the capacity of human reason. The doctrine of the immortality of the soul is an "article of revelation" about which human reason can attain "to no clearness, no

certainty," as Locke concludes in his famous discussion of this point with the Bishop of Worcester (*The Works of John Locke in Nine Volumes*, 1824, III, 489). The Bishop, a "hard" interpreter of Locke, continued to probe "the subversive character of Locke's exploration of the foundations of religious belief," as Ashcraft has put it. Contemporary interpreters tend to be much more easily satisfied than the Bishop and far more generous with Locke. Thus, Aarsleff appears to help Locke overcome the critical question of the immortality of the soul through nothing less than act of faith: "Absolute proof of the non-existence of a future state is not to be had, consequently the knowledge of the mere probability of a future state with rewards and punishments is that which must direct our search for happiness" (p. 116). Whatever judgment one may make of an argument such as this, one must not forget that it was Locke who left the way open for devoted men to understand his work within strikingly different perspectives. They are worlds apart, so to speak, but are useful at least in directing us to reflect on the difference between a political philosopher and a political divine.

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**Public Policy Toward Environment 1973: A Review and Appraisal.** Annals of the New York Academy of Sciences. (New York: New York Academy of Sciences, 1973. Pp. 202. \$25.00.)

**Man and His Environment.** By Harry G. Johnson. (Washington, D.C.: British-North American Committee, 1973. Pp. 33. \$1.00.)

**Our Ecological Crisis.** By Grahame J. C. Smith, Henry J. Steck, and Gerald Surette. (New York: Macmillan Publishing Co., Inc., 1974. Pp. 198. \$5.95, cloth; \$2.95, paper.)

As the initial fervor of the environmental movement has waned, the attention of those concerned about environmental policy has increasingly turned to two fundamental questions: How do the many and varied aspects of environmental quality relate to each other and is a livable environment compatible with continued population and economic growth? These questions underlie much of the discussion in the above three publications.

*Our Ecological Crisis* is composed of three essays—one on ecology by Smith, a second on economics by Surette, and a third on politics by Steck. The essays are framed by a cohesive and well written introduction and an epilogue.

The authors of *Our Ecological Crisis* argue that "the environmental crisis is the result of

very deep forces within our society and culture" and cannot be easily understood, simply defined, or readily solved (pp. vii-viii). They perceive the central question to be: "Can modern societies—characterized by population increase, by advanced technologies, and by a growth-oriented, man-centered culture—adapt to the constraints of the environment in such a way as to ensure not simply survival, but a decent life for future generations?" (pp. 183-4). They are not optimistic about the answer. "From an ecological perspective, growth, expansion, and domination remain the central sociocultural objectives of most advanced societies and comprise the aspirations of the developing societies" (p. 186), and these objectives and aspirations will lead to disaster for mankind.

Such profound and radical conclusions require profound and radical analysis for support, and two of the three essays provide such analysis. Smith's essay on ecology is the best short summary I have read about why the environmental problem does represent a crisis and how and why doom could result from environmental neglect. He correctly focuses on global long-range problems, identifying the major threats to man's existence as changes in atmospheric composition, radiation, pesticides, toxic metals, and ocean dumping. Each of these problems is sensitively and accurately discussed within an overall ecological framework.

Surette's chapter on economics is a sharp departure from the usual economic analysis of environmental problems. As Surette observes, the standard economic approach is "a logical deduction from given assumptions that are seemingly grounded in the empirical workings of the culture. No serious question is raised about the assumptions, nor are the cultural bases for such assumptions ever examined" (p. 88). Surette does question the assumptions, and his chapter is largely an examination of the cultural basis of the economics of industrial America and how this cultural base affects the market economy and the environment. It is not as cohesive or well argued as the ecology chapter, and Surette is not the first economist to raise these questions (Ken Boulding has been posing the same challenges to economists for many years). But the chapter is insightful, challenging, and follows nicely from the ecological analysis.

Coming after the first two essays, Henry Steck's chapter on politics is a disappointment. Steck limits his analysis almost entirely to air and water pollution and largely to the period before 1970. However, after a useful history

of the environmental movement and a discussion of the difficulties of Federal enforcement and standard-setting, he does discuss the organizational and legislative changes which occurred between 1970 and 1972.

Steck's conclusion is that there is an emerging "environmental-industrial complex" whose "preeminence" in environmental policy making is assured (p. 178). "Although it is true that the crisis nature of the environmental situation will continue to produce government action and although the American public will doubtlessly become increasingly alarmed, the possibilities for a meaningful ecological politics hardly seem to exist" (p. 179). The political process is dominated by the forces of big industry and these forces oppose effective environmental action.

This conclusion may be correct, but Steck provides little evidence to support it, and his analysis of how industry exercises such great influence is superficial and misleading. The dominance of the corporations apparently does not manifest itself in legislation. Steck characterizes the 1970 Clean Air Act as "wide-ranging, even radical, in its dimensions and its possibilities" (p. 155) and says that it "represents a major step toward a reordered approach to the environmental crisis" (p. 156). He believes that the 1972 Amendments to the Water Pollution Control Act are "a serious and long-term attempt at cleaning up the nation's waterways" (p. 156) and reflect "a welcome congressional intention to overcome the uncounted failures of the past" (p. 157). The insidious influence of industry also does not show up in the Environmental Protection Agency (EPA), or the Council on Environmental Quality. These organizations provide "the means for a future Administration that may seriously attempt to redress the imbalance between man and his natural environment" (p. 136), and the EPA has "acquired a reputation for independence that most shrewd administrators would covet" (p. 135). Steck does not deal at all with the courts and touches only incidentally on the intricacies of Federal-State-local relationships.

The primary villain at the Federal level, in Steck's mind, seems to be a now-defunct advisory committee called the National Industrial Pollution Control Council (NIPCC). Steck devotes five pages to a discussion of NIPCC. He believes that "it may well have an impact on environmental policy equal to that of any other governmental agency" (p. 167) and that "because of a peak role in the authoritative decision-making process, such high-level advisory bodies as the NIPCC must be regarded

as having an influential if not decisive voice" (p. 171). Anyone familiar with environmental policy making in Washington can only regard this as nonsense. NIPCC in fact exercised almost no power. As one measure of its influence, it was not permitted even to see copies of Administration legislation until the bills were made public. In June, 1973, Congress declined to appropriate any funds to support NIPCC. The White House did not wage any battle to save it, and thus the "decisive voice" of NIPCC was effectively and ignominiously silenced.

Harry Johnson's short pamphlet, *Man and His Environment*, is a literate non-technical presentation of the orthodox economist's view of environmental problems. He believes that "alarmist fears of population pressing on natural resources are unjustified" (p. 3) and that "economic growth, and not the impediment or prohibition of it, is the way to betterment of the quality of life and the avoidance of ecological disaster" (p. 27). Perhaps the most fundamental difference between Johnson and the authors of *Our Ecological Crisis* is in the realm of history and science rather than economics. Whereas Smith et al., are inclined to believe that "in the last few years we have passed a pollution threshold in a strictly ecological sense as well as in a sense of awareness" (p. 5), Johnson says that, "[t]here is no reason to think, in the light of millennia of human history and the consistent falsification of the prophesies of successive schools of doom predictors, that at the end of the third quarter of the twentieth century everything has suddenly changed" (p. 13).

*Public Policy Toward Environment 1973* is a collection of twenty very short articles covering almost all aspects of the environment from earthquakes to genetics. The authors are mostly scientists and include such luminaries as Glenn Seaborg, Detlev Bronk, and Kenneth Clark. Although the assignment given to each of the authors included describing the goals of public policy in the area under discussion, giving criteria for judging the adequacy of the policy, evaluating achievement under the policy, and suggesting proposals for an optimum policy, few of the articles actually carry out this assignment. The best articles, such as those on climate control and food plants, deal with problems for which little or no policy exists.

It is very difficult to know who the audience for *Public Policy Toward Environment 1973* is supposed to be. The essays are much too brief and general to be of real relevance to policy makers, and yet their brevity and the lack of coherence of the volume as a whole

makes it not very useful to the student or lay reader trying to understand environmental problems. It contains a number of miscellaneous interesting facts and observations, but a better format should be found for injecting the thinking of the scientific community into the policy process.

J. CLARENCE DAVIES, III

*Resources for the Future, Inc.*

**The Politics of Continuity: Maryland Political Parties from 1858 to 1870.** By Jean H. Baker. "The Goucher College Series." (Baltimore, Md.: The Johns Hopkins University Press, 1973. Pp. 239. \$11.00.)

Professor Jean H. Baker of Goucher College argues that the disruption of American politics in the 1860s did not produce in Maryland "a durable realignment in both the anatomy and ideology of the Democratic and Republican parties" (p. xiii), but rather the postwar Democratic dominance "merely confirmed changes effected in the late 1850s" (p. xiv). To support this thesis she exploits the major extant manuscript collections, newspaper commentaries, and legislative records, and she makes effective use of the Rice index of cohesion to study legislative roll calls, and limited use of correlation coefficients to determine the relationships of variables and election return data. The result is a refutation of Charles L. Wagandt's contention (1964) that the state's civil war politics represented a "Mighty Revolution" revolving around the issue of Negro emancipation and eventual suffrage.

In the political turmoil of the 1850s which typified virtually all American state and local politics, a group of young Maryland agrarian gentry (personified by Oden Bowie of Prince George's County) gained leadership positions in the Democratic party, successfully developed a series of county committees and legislative caucuses which formed the base of their internal discipline, and then articulated the issues of Negrophobia, nationalism, and local autonomy to secure legislative control by 1858. Their opponents were more likely to be older men who had less wealth and social status, and came from commercial or business backgrounds. Drifting from Whiggery, to Know-Nothingism, to Unionism, to Republicanism, such men were decidedly antagonistic to the ideology of party discipline that characterized the Democrats. This group contained some of the most dramatic personages of Maryland political history. Congressman Henry Winter Davis was a political ideologue whose radicalism is remembered in the famous Wade-Davis reconstruction bill, his protege Senator John A. Creswell became a

patronage-hungry partisan hack, Governor Thomas Swann was a political chameleon who successfully tacked his ship of political fortune with each shifting breeze, and Montgomery Blair, Lincoln's Postmaster General, sought to form a conservative coalition from anti-radical Unionists and dissident Democrats. With all of this talent and with the ability to label their opponents as the followers of treason and disunion, Baker finds it amazing that Democratic defeats were so few and their triumphs so great by 1866. The best indicators of the Democratic party's revival were the Constitution of 1867 and the election of Bowie to the governorship with 75 per cent of the vote. Unionist factionalism, Lincoln's heavy-handed deprivation of civil liberties in Maryland, effective local Democratic organization, Negrophobia, and party loyalty, all contributed to a series of victories that culminated with Swann and Blair coming hat-in-hand into the Democratic party.

Despite the importance of her contribution to our understanding of the Free State's political development in the period, Baker makes little substantive addition to the growing sophisticated body of local history of politics during the Civil War era. Admitting that a socioeconomic analysis will not explain voting patterns (see notes on pp. 44, 106, 110), she makes no attempt to test ethnocultural variables other than the number of blacks (for which there is a positive correlation with the Democratic vote). Particular attention should have been paid to the Baltimore city wards that sent Davis to Congress. If this had been done, we might well have had another important study of early American politics like those of Lee Benson on New York (1961) and Ronald Formisano on Michigan (1971). We need to know how such variables as religious preference and national origin influenced voting. Are they as significant in Maryland as they seem to have been in New York and Michigan? Thus, while Professor Baker's work represents a significant contribution to the study of Maryland politics, it does not constitute the definitive analysis of the period.

DAVID CURTIS SKAGGS

*Bowling Green State University*

**The Skill Factor in Politics: Repealing the Mental Commitment Laws in California.** By Eugene Bardach. (Berkeley, Calif.: University of California Press, 1972. Pp. 291. \$10.00.)

Thorough, literate, and sensitive treatments of complex issues are rare in this the day of the

instant "nonbook." *The Skill Factor in Politics* is an excellent work deserving of widespread attention and serious reflection.

Bardach, a member of Berkeley's Graduate School of Public Policy, develops two broad themes simultaneously. One, captured in the title, examines in careful detail the attributes of political expertise needed to see a new policy through the legislative process. The second, noted in the subtitle, *Repealing the Mental Commitment Laws in California*, confronts termination—a little-understood aspect of the overall policy process—in the context of an emerging issue of great moment for citizens in many locales. While Bardach masterfully weaves his themes into a tapestry of beauty and impact, it is useful to separate them for review purposes.

The context is set in the book's first part by way of detailing the "attentive public" in terms of its concern for and stake in mental health policies and programs in California during the mid-1960s. The California setting is thoroughly characterized and appropriate linkages to longer-term trends in mental health practice in the encompassing environment are made. Several major fundamental issues, including prevalent, representative points of view on each, are considered: What is mental illness? Who is the "patient"? What are the causes of mental illness? What treatment methods exist? What are the rights and duties of those providing and receiving treatment? What are the trade-offs in terms of the quantity and quality of care? What institutional models are available to best meet service needs? Not surprisingly, the attentive public did not neatly sort itself out and line up into simple "traditional" and "progressive," or other dichotomized summary categories with respect to these issues. Clarifying and disentangling, "mapping" in Bardach's terms, the factions, interests, and alliances created in response to the major issues became on-going operational problems for the political entrepreneurs interested in making changes—problems whose complexity is ably captured in Bardach's *ex post facto* reconstruction and analysis of the "map."

The book's second part contains five mental health policies as seen through the eyes of the attentive public and the political entrepreneurs most interested in changing those policies. The civil rights of the mental patient are routinely threatened and abrogated by the "simple" commitment act. Bardach translated abstractions such as the denial of human rights and dignity into understandable operational terms with sensitivity and in great detail. Human rights aside, the eventual outcome is shown to rest

far more on the somewhat bizarre interplay of key legislative personalities than on any grandiose concern for absolutist ethical imperatives. Four other policy changes related to budgeting, professional licensing, the structuring and functioning of state and local mental health service provision, and realigning bureaucracies at the state level are similarly treated.

"Entrepreneurship in Practice," the third and most creative part of the book, analyzes the political attributes of the key players and the main processes by which critical mental health policies were changed.

A "good" player strives to expend his political resources—analytic, marketing, and managerial—so as to capture the authorities and to neutralize his opponents, and to do so efficiently and with due regard for the timing of events in the unfolding context. "Skill" is simply, but convincingly, defined in terms of the political entrepreneur's efficiency and inventiveness in expending his resources. For instance, "the critical resources needed for marketing a proposal are those that facilitate rapid and efficient communication with individuals and groups both inside and outside the attentive public" (p. 218).

The concept of information-as-resource is stressed in this treatment, and in a policy arena where most regard the issues to be merely "technical and administrative," the critical, catalytic role of the expert in generating, analyzing, and presenting this information is heightened. Indeed, the role played by several consultants throughout the process can only be considered decisive—as is increasingly the case with many policy issues that have highly technical and complex aspects. In this case, the "essential entrepreneurs" were able to capitalize on the slight and piecemeal understanding of the context held by those in the attentive public: "pluralistic ignorance," in Bardach's terms. Understandably, legal specialists were mainly and narrowly concerned about the legal aspects of the various policy proposals; medical specialists had little appreciation of legal details and focused on alternative treatment methods to the near exclusion of all else; and most politicians, i.e., the "average legislator," did not know or care much about anything having to do with mental health policies at all. (Bardach could find only four senators and at best two assemblymen who "knew enough to care.") In this setting, lawyers were encouraged to concentrate on and support changes in treatment practices, doctors were made to focus on changes in the law, and politicians were "sold" in any way possible to get their support. Watch-

ing and documenting the players' responses as they began to realize and react to the possible indulgences and deprivations flowing from various proposed alternatives is a fascinating lesson in compromise, evolution, and bargaining—in short, a rich lesson in real politics as seen through the eyes of a careful observer, not the pinched view provided through the bunghole of a sterile theorist's  $2 \times 2$  matrix or 2- or 3-variable regression equation.

Termination, the book's second theme, is evident throughout. Termination refers to the adjustment of policies and programs that have become dysfunctional, redundant, outmoded, unnecessary, and so forth. We do not know much about termination, although movements to "deinstitutionalize" and "debureaucratize," such as those captured in this book, are drawing attention to this neglected phase of the overall policy process. *The Skill Factor* provides the reader with a clear view of some of the difficulties one may encounter in attempting to terminate. Some of these difficulties are suggested in answering questions of the following sort: Who will suffer (benefit) from the termination? What kinds of compensation must be considered—no compensation, small compensation, equivalent, premiums? When should compensation be made in terms of what has been principally lost, e.g., wealth for wealth in the case of hospital attendants who stood to lose their jobs? When should it be in alternative values, e.g., compensatory respect for those professionals denied treatment "rights" in the licensing case? What can be learned from termination in a given setting that might inform and improve the initiation of policies and programs in the same or related fields and settings? For instance, closing down an antiquated and inhumane mental hospital may generate different, albeit equally inhumane, practices for those souls who need and are unable to receive services where they are eventually relocated. The list of pertinent questions is long, but seldom asked and less frequently answered. Bardach provides us with the raw materials needed to do both.

*The Skill Factor* has its inevitable shortcomings, but they are few and detract imperceptibly from its many strengths. Most weaknesses follow from the fact that the cases considered are extraordinary "success stories." In most settings, one would expect the entrepreneurs to make many more mistakes, to be less motivated, and to have less luck. No would-be entrepreneur should expect to use this work mindlessly as a cookbook; no neophyte analyst should delude himself into thinking that the same events have occurred in other, even closely related, settings.

A convincing argument, in my view, could be made that the principals in this little drama were just plain lucky.

GARRY D. BREWER

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**Escape From Anarchy.** By Alan Bent. (Memphis, Tenn.: Memphis State University Press, 1972. Pp. 196. \$6.95.)

This book attempts to use systems models and cybernetic analysis to outline a planning process rationalizing the urban intergovernmental system. Specifically, "... it believes that rational inputs into a rational decision making system should result in rational policies ... ." (from the introduction by George Blair). Planning, apparently physical, social and governmental, is seen as the linkage between the political system and the urban system.

The introduction outlines a cybernetics theory which "... applies the theoretical framework of systems models and cybernetics analysis in building an intersystem linkage between the urban system and the political system ... ." (p. 6). The first chapter elaborates, based on Buckley and Deutsch, a systems/cybernetics model.

Chapter II is a routine listing of major "urban discontents" emphasizing population and pollution. Chapter III lists the options for metropolitan government unification and success to date. Chapter IV lists the "goal seeking outputs" of the federal government to date, which have mainly been limited to grants-in-aids. Chapter V argues for vast, centralized "systems" planning, to "... transform amorphous agglomerations, that are typical of urban areas, into cohesive, unified organic wholes ... ." (p. 121); and Chapter VI considers existing regional planning agencies. Chapters II through VI, however, do not make use of the cybernetics model outlined in Chapter I.

Chapter VII is the heart of the book, containing Professor Bent's proposals for governmental reorganization, presumably based upon his systems model. It outlines the structure of an intergovernmental system, cybernetically linked by planning and government management. Highly centralized, the five levels are federal, federal region, state, regional council, and locality. In a rigid hierarchical setting, the Office of Management and Budget, with regional directors, serves as the nerve center. The levels apparently are to serve both as goal-setting devices and as part of the feedback system. The regional director is to have extension discretion in disbursing funds, since fiscal

resources are the control mechanism by which the "... linkage-centers effect control over the actions and performance of their charges" (p. 177).

Some will regard this as an important work, since it attempts to apply concepts such as cybernetics to the planning process. As a theoretical work, in laying out certain issues, it may be a step in this direction. Those interested in applying systems models to physical planning or government planning (the book does not always separate them) should read it.

The book has, however, some substantial shortcomings. First, the traditional nature of the final proposal could easily be based on the "democracy, economy, and efficiency" reformer ideals for centralization in vogue during the early part of the century. They do not seem to rise out of any knowledge or insight gained by the use of cybernetics.

This objection leads to the second shortcoming. The parameters and specifics of the cybernetics model are never spelled out. Bent fails to specify exactly what is part of the cybernetics system, the feedback loops, or any "black-box" processes. This omission is troublesome even given the abstract nature of system theory. The model is rarely related to the author's description of the planning process. Where it is, as in the last chapter, it is distressingly vague or excruciatingly tantalizing to those who are interested deeply in planning or cybernetics or both.

Finally, the proposals are politically naive and often seem superficial. Short shrift is given to the advantages of the present decentralized system: the author merely states his preference for a more centralized, rational system. The work does not cite any of the "political economy" or "public choice" literature which argues that a market system of many competing governmental actors operates more efficiently and democratically than a centralized bureaucracy. Works by Ostrom and others are not cited. Centralized hierarchical rationality is sought, without noting local political values, federal level bureaucratic inefficiencies, failures of planning to date, or difficulties in implementing the model.

JOHN REHFUSS

*Northern Illinois University*

**Private Pressure on Public Law: The Legal Career of Justice Thurgood Marshall.** By Randall W. Bland. (Port Washington, N.Y.: Kennikat Press, Inc., 1973. Pp. 206. \$9.95, cloth; \$3.95, paper.)

As the author indicates, this is not a biography but an analysis of the legal battles of

Thurgood Marshall, perhaps the foremost civil rights lawyer of our history. The coverage is of the cases and controversies in which Marshall has participated as an advocate for the NAACP, as Solicitor General of the United States, as a judge of the court of appeals, and now as an Associate Justice of the Supreme Court.

The treatment is "cool." The author outlines the issues and provides the background for understanding the strategy of the advocate as well as the decisions of the judge. From the study emerges a picture of a courageous, persistent, and ideologically consistent champion of civil rights who, although recognizing the differences between the roles of advocate and judge, remains steadfast in his commitment to the principle of equal justice. Professor Bland concludes that Marshall's accomplishments as an advocate outshine those as a jurist, although conceding that it is too early to make a definitive evaluation of his contributions from the bench. A revised doctoral dissertation, and still bearing some of the marks of such a product, the book provides a most useful review of the civil rights movement at a time when it was directed at destroying the legal edifice of segregation and in providing juridical protections for the victims of discrimination. (As late as 1919, as Bland points out, 12 blacks were sentenced to death, 75 to long prison terms, after a forty-five minute trial for their participation in a "riot" in which 200 blacks but no whites were killed.)

There is always the danger of our reading history backwards and of accepting what has been accomplished as the inevitable working out of economic, social, cultural, political and similar "historical forces." There are those who denigrate the works of men like Marshall and his companions as being ineffective. True, the abolition of forced segregation did not make civil rights champions out of most white Americans, and the securing of equal justice does not by itself ensure that blacks will secure their fair share of the economic funds of this world. It is also true that to win a favorable Supreme Court decision against a discriminatory practice is not the same thing as having that practice abolished throughout the length and breadth of the land.

On the other side, we should not underestimate the advantage of having the courts on the side of equality rather than on that of those who wish to impose racial segregation. We should not minimize the courage and contributions of Marshall and his colleagues. With the help of a handful of dedicated black attorneys and white liberal allies, and a few courageous plaintiffs and some scared defendants, they

risked their lives and careers in a life long effort in behalf of the poor and the dispossessed long before it was the fashionable thing to do. They were the human instruments of the converging economic, political, and social forces, but in every day terms they took on the "power structures." They forged the legal doctrines. They used the courtroom to arouse gradually the conscience of court and country to provide the legal openings through which the modern civil rights movement came marching through.

Marshall is a champion of civil rights. He is a man of the law. He sees these as one and the same thing. For his conviction, shared by many, is that the forces of the law, the arguments of reason, and the structures of democracy are more apt to protect the rights of unpopular minorities, racial and otherwise, than is the resort to extralegal tactics. There is much in this history of the pre 1960's civil rights movement as described in this volume to support Marshall's position. Bland reminds us by the story of Autherine Lucy, an early victim of "student unrest," that the young are not always on the side of the poor and the black; by names like Governor Orval Faubus, that civil disobedience can be the tactics of the bad guys as well as the good ones. For it was Faubus who claimed that when the law was immoral, as he insisted was true of the Supreme Court's decision in *Brown vs. Board of Education*, that the proper recourse for the "decent" people was to take to the streets. By massing themselves in front of the schools and shouting abuses at the black children, Faubus insisted, these citizens were merely engaging in noncoercive protests. It was the white segregationists who insisted that under no circumstances should the police or the military ever be allowed to enter the school building or a college campus in order to protect the rights of black students to attend. We are also reminded by this history that the most persuasive and effective appeal used to secure eventual compliance with the civil rights decisions was to law and order—"you may not be in favor of integration, but it is now the law of the land."

Thurgood Marshall for decades has been in the forefront of the never ending legal battles to secure for all persons, and most especially for blacks, the rights promised by the Declaration of Independence and proclaimed in the Constitution. It is good to have this recording of his—and the nation's—accomplishments. We should not need to be reminded of how far we still have to go.

J. W. PELTASON

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**Politics and the Administration of Justice.** By George F. Cole. (Beverly Hills, Calif.: Sage Publications, 1973. Pp. 234. \$10.95.)

One of the most complex areas of the Political System is the legal system. The myths surrounding the system, and its complicated rules, make it a very intricate field of politics. This complexity is analyzed by Professor George Cole, using the tools of role theory and administrative behavior. His primary goal is to "examine the administration of criminal justice within the context of the local political system" (p. 19). He does this by examining the overall administration of justice, relying on models of the criminal justice system presented by Herbert Parker, James Klonoski, Robert Mendelsohn, and others; then showing how these models have been used to explain the criminal justice system.

After reviewing some of the organizational and role-theory literature, relying for the most part on Talcott Parsons and Robert Merton, Professor Cole states that his approach entails an analysis of the "exchange relationship" of actors within the criminal justice system, and the bargaining of these actors to produce "criminal justice." "With a bargaining rather than an adversary model as a basic assumption, the study emphasizes the connections among law, politics, and administration" (p. 47). Cole accomplishes this by grouping the major actors within the criminal justice system—police, prosecutor, defense attorney, and court—and giving a detailed analysis of the functions played by each actor in the production of criminal justice.

Professor Cole examines each set of actors within the criminal justice system in a very logical manner. He begins by discussing police functions, and their relationship to the community. Next he treats the role of the prosecutor and his relationship to the other actors within the criminal justice system. This is followed by an examination of the defense attorney's role in this process. Finally, the court and its actors (clerks, judges and jurors) are thoroughly examined. Throughout his analysis, Cole reminds us of the relationship of each actor to the other, and to the general public.

The most interesting feature of this volume is Professor Cole's emphasis on bargaining and the "exchange relationship" between the actors within the criminal justice system, as his explanation of the plea-bargaining system which involves the prosecutor, the defense attorney, and the court. He also explains the bargaining power of the police in terms of the prosecutor's reliance upon them for "raw material" as evidence for prosecution, and their referring

clients to defense attorneys who cooperate with them. "A police captain is probably going to be less likely to hand the attorney's business card to a prisoner if, on the basis of past experience, he has found that the lawyer is not cooperative" (p. 177).

Professor Cole concludes with a brief discussion of "due process in an administrative system" of criminal justice, and the "dilemma" of the "need for order and the preservation of civil liberties" (p. 226). Here he calls our attention to the Warren Court's revolution in civil liberties, and the changes brought on with the inception of the Nixon administration and his Court appointees. Cole asserts in the end that criminal justice reflects the "dominant values" of the larger political system.

This is a valuable volume for anyone wanting to understand some of the complex features of the criminal justice system. It gives a thorough review of much of the current criminal justice literature. It fails, however, to suggest any alternatives to the criminal justice system, and especially to some of its actors. Cole tells us how the present system could be strengthened, but he does not sufficiently demonstrate a need for the present system. What would be the conditions within a community without a police force? Or a court or prosecutor? These are some of the questions that must confront those of us interested in studying the criminal justice system.

NOLAN E. JONES

*The University of Michigan*

**The Truman Doctrine and the Origins of McCarthyism: Foreign Policy, Domestic Politics, and Internal Security, 1946-1948.** By Richard M. Freeland. (New York: Knopf, 1972. Pp. xii, 419. \$10.00.)

**Seeds of Repression: Harry S. Truman and the Origins of McCarthyism.** by Athan Theoharis. (Chicago: Quadrangle Books, 1971. Pp. 238. \$6.95.)

Revisionism, at its best, challenges received wisdom, forces reappraisal of things always accepted, adds new knowledge, and advances understanding. Its victories in the soft science of politics have not been less in their meaning for man than those in the hard science of physics. The revolutionary thought that governments exist for the governed and that it is not the other way around is not less formidable than the realization by some Greek that it was not the trees that stirred the wind but the wind that stirred the trees, nor the Copernican conception that put the sun and not the earth at the center of the solar system. But revisionism



in history writing has a way less of establishing new truth about what was, than of ratifying today's predilection with yesterday's evidence, reconsidered. It is in this mode that the books by Professors Theoharis and Freeland are written.

The assumption of both the books is that President Truman and his principal surrogates were responsible for the phenomenon of McCarthyism. Both books go even farther than this and say that there were no substantial differences between Truman and McCarthy. As Theoharis puts the proposition, "The Senator and the administration differed not so much over ends as over means and emphasis" (p. vii). As Freeland puts it, "The practices of McCarthyism were Truman's practices in cruder hands, just as the language of McCarthyism was Truman's language, in less well-meaning voices" (p. 360).

It is assumed by both that America started the Cold War. For Theoharis, it began when Truman, immediately upon taking office, rejected what Theoharis says was Roosevelt's approach to foreign affairs as represented by the Yalta agreements—willingness to negotiate in good faith with the Soviet Union, his belief that Soviet promises could be trusted, the consent to compromise—and set forth instead upon a policy of containment and superior military strength (p. 70). "In contrast to Roosevelt's sophisticated international approach," Truman's policy stressed the primacy of power in international relations, and was "shaped by his World War I experience, his active participation in the American Legion, and his antipathy to the isolationism and antimilitarism of the 1930's" (p. 31).

The Freeland thesis seems to be that before Yalta there was "deepening tension" between the two powers as war came to a close. By February 1945, the Soviet intention to dominate Eastern Europe was becoming clear (p. 39). It was not a trust in idealism that led Roosevelt to praise the Soviet Union at the time of Yalta but rather a propaganda strategy designed to promote a policy of American international commitments through membership in the United Nations and economic aid. So, Roosevelt hid his distrust and anxiety over the Soviet Union and did nothing but praise it, especially after Yalta (p. 41). Truman continued the propaganda throughout 1945, concealed the tensions with the Soviet Union, and like Roosevelt, raised false hopes about both the UN and the Soviet Union (pp. 43–45). But the economic levers that were supposed to have been the principal instruments of postwar policy for both Roosevelt and Truman had no

effect upon Soviet policy in Eastern Europe. By the beginning of 1946, Secretary of State Byrnes gave up the effort to use them, and the policy of containment began. The Truman Doctrine was the "first official American renunciation of U.S.-Soviet cooperation" (p. 168).

To support the proposition that Truman began McCarthy, both authors stress the Truman loyalty program in 1946–47, the announcement of the Truman Doctrine in April 1947, the activity of the Department of Justice in the enforcement of internal security, and the Marshall Plan. What Theoharis and Freeland both show is that Truman used a hard anti-Communist line to get the Congress to approve aid to Greece and Turkey in 1947 and to enact the European Recovery Act in 1948. The record certainly indicates that he did do just that. Still, one can admit that Truman used hot rhetoric to advance his policies and yet feel that it was something more profound than Truman's hyperboles that filled McCarthy's sails and sent him on his short voyage into infamy. But the authors find Truman's rhetoric not only hyperbolic but diabolic, full of craftiness and design. The hard line against the Soviet Union required a hard line against suspected subversives at home, and the hard line against suspected subversives at home helped to promote the hard line against the Soviet Union, and thus it secured the passage of aid to Greece and Turkey and the European Recovery Program which was presumably intended to benefit commercial and other American interests. This seems to be the essence of the Freeland thesis, with which Theoharis is in general accord.

Freeland says more than once that it was only nine days after the Truman Doctrine was announced that the Federal Employee Loyalty Program was launched. He acknowledges that a case can be made for the view that the Federal Employee Loyalty Program was really intended to head off a tough assault on the Executive that had been building since the Dies Committee was created in 1938—an assault which had been frustrated by Roosevelt and which could hardly be stemmed when the 80th Congress turned up Republican in both houses (pp. 123–127) after the election of November 1946. But Freeland does not believe it. If Republican politics were the only reason, he speculates, Truman would have been defiant! Because Truman was not defiant, it must mean then that the Administration's foreign policy "may well have been" the inspiration for the Federal Employee Loyalty Program, and not the 1946 elections (p. 128).

One difficulty with this argument is that it

seems to assume that had there been no Truman Doctrine there would have been no Federal Employee Loyalty Program. But Freeland also says at one point that the Administration was unable to resist the congressional Republicans on internal security (p. 201, 202). It can reasonably be asked then whether, in the absence of the Truman Doctrine or the Marshall Plan, congressional Republicans would have been passive, their sense of danger dormant, their energies unstirred. The evidence is to the contrary. The evidence is that with a majority in both houses for the first time in over a quarter of a century, shut out from the White House in four successive presidential elections, uneasy over the toleration of radical groups allowed by Roosevelt, bent on equating the New Deal with un-Americanism, the Republicans and their Democratic allies in the 80th Congress would have forced even more painful measures than the Administration used. An example is the McCarran Act with its provision for detention camps that Truman vetoed, only to have the measure passed over his veto.

According to Freeland, Truman produced a war scare in 1948 in order to get Congress to enact the European Recovery Program. Now it is true, he says, that the Soviet Union did impose a unilateral military accord on Finland and that the Communists did take over Czechoslovakia, but he thinks that the Czech coup was of no importance because the Soviet Union had dominated the country all along, and besides it was in the natural sphere of influence of the Soviet Union; and, he feels, the Finnish accord changed nothing (p. 278). The war scare was manufactured to get ERP through Congress and used to help Truman's chances of re-election (pp. 280 and 299). Theoharis does not go so far as to say that the "war scare" was manufactured by Truman for ulterior reasons but is content to say that Truman used the coup in Czechoslovakia, Soviet pressure on the Finnish government, civil war in Greece, and the chance of a Communist electoral victory in Italy to support a tough foreign policy critical of the Soviet Union (p. 54). Perhaps some would not find it unreasonable even now to think that there was much in Soviet foreign policy in 1948 to be critical about.

Ultimately, it seems to me, both these books fail in their purpose. Their common theme is that Truman produced McCarthy, but they do not demonstrate that he did so. What they demonstrate is that Truman had a difficult time with a reactionary Congress and a competitive Soviet Union and that he sometimes used the second to overcome the resistance of the first.

The notion that the congressional dogs were sleeping on the issue of internal security until Truman aroused them is just contrary to fact. The assumption that the World War I veteran member of the American Legion flew like a hawk to tear the throat out of the dove of peace is an unwarranted piece of psychologizing.

Both books impose a greater degree of rationality and conscious manipulation on events than is allowable by the record, which shows that the Executive and the Congress were divided and hostile; that some of the executive agencies like State, Commerce, and Justice were either pushing their own policies independently of the president and of each other (like State and Commerce while Wallace was head of the latter), or that they were split within (like Justice and the FBI under Hoover); that party identification provided no unity since the Southern Democrats and the Republicans voted with each other; or that whatever unity of effort the war had provided disappeared when the war was over, both at home and abroad. The three years after the death of Roosevelt were the aftermath of war with all reins now tangled and slack, the horses spent, trembling and sweating, momentum lost, while a new driver was suddenly pitched into the driver's seat without experience at the reins or even knowledge of the road. It is just possible that the really hot story about these years is not that Truman managed to get foreign aid through the Congress but that he was not impeached; but then, perhaps, there was no need for such drastic action since it was expected widely that there would be a change of drivers in November 1948.

Both accounts fail to give Truman and the administration proper credit for the actions against reactionaries that he did take. He was, to be sure, not one of the leading libertarian presidents (how many have there been?), but he was better than most. He did hold off the ravenous reactionary coalition in the Congress for two years—from the time of his accession to office until the Federal Employee Loyalty Program—and he introduced that measure only after he had lost both houses of Congress. He and his Attorney General made sure that the Mundt-Nixon Communist Act was buried in the Senate Judiciary Committee and never reported out. As was said above, he vetoed the McCarran Act of 1950. He defended Dr. Edward Condon who was under attack by J. Parnell Thomas, Chairman of HUAC, as "the weakest link in America's atomic security." He fought the HUAC on Bentley and Chambers, ✓

calling the committee's investigations a "red herring." After the election of 1948, Truman and Attorney-General Clark began a determined effort to have the HUAC abolished. Although failing in this, they succeeded in blocking the reappointment to the committee of John Rankin and F. Edward Hebert, who had given Southern comfort to the Republicans, and had the moderate John Wood of Georgia appointed as chairman. In the four years of his term after the period covered by Freeland, Truman threw his support behind the Tydings Committee which tried to discredit McCarthy's attack on the State Department. He faced down McCarthy on the matter of Philip K. Jessup, and when the latter's formal appointment to the Sixth General Assembly of the UN was blocked in the Senate, Truman gave him a recess appointment instead. For the authors, these acts have some weight, but too little to preponderate.

The books fail to establish Truman as the author of McCarthyism for reasons special to each—Theoharis is too broad, and Freeland is too narrow. Theoharis often refers to people he calls "conservatives" or "McCarthyites," terms that cover all those disposed to take a hard line on the Soviet Union and on internal security, but such people existed before Truman, as he himself says (pp. 14 and 15); they had a base independent of McCarthy's; and they used him in their insistent assault upon the New Deal. The "McCarthyites" therefore existed before McCarthy, were in the Congress before he arrived, and remained there after his condemnation by the Senate. In view of this, it is hard to see how Truman could have created McCarthy or McCarthyism.

Freeland on the other hand proves too little in general, and nothing in particular of the substance promised by his title, the "Origins of McCarthyism." He concentrates upon two years, 1947 and 1948, in working changes on his principal thesis that the Truman Doctrine was invented to get aid for Greece and Turkey through an isolationist and reluctant Congress and that he had to go after domestic "subversives" to make the Truman Doctrine credible: if we are chasing the Reds abroad, how can we ignore them at home? And this is what led to McCarthyism. He does not discuss McCarthyism at all and never even defines it except by indirection—i.e., the anti-Communist emotions of the 1950s carried to excess (p. 4). But without ever examining the phenomenon of McCarthyism, he asserts that Truman and McCarthy were more or less the same in speech and practice. His book is not really

about the origins of McCarthyism at all. It is about some aspects of the politics of two principal measures—aid to Greece and Turkey and the European Recovery Program.

Both writers reject sociological, psychological, ethnic, religious, geographical, and metaphysical interpretations of McCarthyism as incomplete, at least. The most probable interpretation, they say, is the political one. Having said so myself nine years ago, I am inclined to agree. But politics did not begin in 1947 and end in 1948, and Freeland's book needs to display a broader and deeper understanding of American political movements than it does if the two years he examines are to be put in proper perspective. As for Theoharis, he knows in some inchoate way that the anti-radical excitements of the Truman years were part of something more deeply basic in the American political and economic system than this or that piece of legislation. But some of his generalizations are unacceptable on their face. He says, for example, that "Until the 1920's . . . popular anti-radicalism had not been essentially anti-communist in character" (p. 6). Since there was no Communist movement of any consequence in America before then, this is hardly surprising. The best chapter in Theoharis (and it is a good one) is the one on Yalta (Chapter 4) which was evidently his doctoral thesis.

Both the books give less account than they should to the international moves of the Soviet Union and Freeland especially neglects this obligation. He says that he is not writing a diplomatic history and that he makes no serious attempt (that such a history would require) at analysis of Soviet intentions and policies (p. 12). He is concentrating, he says, on the way international developments were perceived and articulated by various groups in the United States. To leave out evaluation of Soviet foreign policy and to concentrate only upon American perceptions of that policy is to make it appear that the American responses were probably invalid, or at least explicable only in some term that leaves out half the equation, and is therefore incomplete.

To return to an earlier figure—the authors would like to prove that Truman was the wind that stirred motion in some dendrological McCarthy. It is possible to conclude, on their own evidence, that the same wind stirred both the President and the Senator, and that when it had blown itself out, McCarthy was down on the ground, and Truman was still growing.

EARL LATHAM

*Amherst College*

**To Live on This Earth.** By Estelle Fuchs and Robert Havighurst. (Garden City, New York: Doubleday and Company, 1972. Pp. xiii, 390. \$8.95.)

**One Hundred Million Acres.** By Kirke Kickingbird and Karen Ducheneaux. (New York: Macmillan Publishing Company, Inc., 1973. Pp. xv, 240. \$6.95.)

The rediscovery of the American Indian is a phenomenon of the late 1960s. The publishing industry, not slow to capitalize on a new marketable resource, has issued innumerable anthologies of immortal Indian prose and poetry, occasionally illustrated by photographs of vanished days of glory. These efforts are marked by two common characteristics: non-Indian authorship, and a singular lack of appreciation for the highly politicized context of contemporary Indian life. Perhaps more crucially, they totally ignore the strong commitment of Indian people in the United States to the continuing struggle for self-determination and cultural autonomy. As an Indian I consider most recent "Indian books" personally offensive and socially destructive, for they perpetuate the dangerous myth of the inevitable disappearance of a distinct Indian community in the modern world, and they reinforce the myth of the pre-eminent expertise of the non-Indian observer/commentator. As a social scientist I consider the bulk of this current "scholarship" both intellectually parochial and politically naive. Given the overwhelmingly political nature of Indian policy in the United States, it is especially unfortunate that political science has largely followed the tradition of relegating the investigation of Indian affairs to anthropology, where Indian communities have remained trapped in the never-never land of the ethnological present. The fact that the *American Political Science Review* is now turning its attention to the review of Indian-oriented materials is a promising sign that this state of affairs is beginning to change, to the mutual benefit of the scholarly and Indian communities.

*To Live on This Earth* and *One Hundred Million Acres* are both essentially sources of solid reference material; both draw attention to critical dimensions of the American Indian community; both hint at the complexity and importance of political analysis of Indian status. This, when added to their general conscientiousness and seriousness of purpose, is enough to draw favorable attention from the Indian community, and among scholars interested in Indian affairs. For political scientists, however, the message is slightly different. These books

point toward new and exciting areas of political research, reaching far beyond the issue of Indian status alone. Indian affairs have been ignored altogether too long in the emergence of an extensive literature on political development, political economy, ethnicity in politics, political socialization processes, etc. I hope that these two books will stimulate such research, suggest fruitful new directions for research, and support attempts to generate theory capable of dealing broadly with the dimensions of political mobilization in tribal, minority, and industrial communities.

*To Live on This Earth* is one of the few systematic and wide-ranging scholarly attempts to develop and test basic hypotheses about Indian education in the United States. Professors Fuchs and Havighurst examine the several types of schools serving Indian clientele, and investigate such vital policy areas as Indian attitudes toward schools, teachers, curricula, etc. They also examine such generally held notions as the existence of pervasive low self-esteem among Indian students (which they deny), cultural insensitivity among teachers and administrative personnel (which they partially substantiate), etc. In large part, the book is a straightforward report on research; at the end, however, it proposes certain approaches to the development of Indian educational policy which grow out of its findings. A particularly useful inclusion is the appendix, which gives details of the survey schedule used, the nature of staff composition, and so on. All in all, this study is an unusually conscientious approach to Indian research.

This very conscientiousness, however, in a sense camouflages the basic weakness of the book from the point of view of Indian and politically oriented scholars. Ordinarily, it must be admitted, political scientists tend to view education per se as peripheral to their primary interests. Yet education is a primary agent in political socialization and acculturation; it may have decisive influence on the very process of individual identity formation. Indian education, then, is central to the analysis of the political status of Indian affairs as it reflects the interests and goals of the dominant community vis-à-vis Indians. In addition to its politics, content, Indian education has been administratively shaped in the explicitly political environment of Washington, D.C., so it is doubly political. Since the political nature of education applies in all communities, it is especially unfortunate that the authors of *To Live on This Earth* give so little attention to these aspects of educational policy and practice.

The book also fails to confront the issue of the general political and cultural status of

American Indian society today, and of the future of that society. The authors in fact eschew any intention to take up this issue, stating that such decisions must be made within the Indian community. I agree. The thrust of their analysis, however, is to emphasize and reinforce the status quo, a situation in which political constraints prevent precisely such Indian self-determination. Thus, evidence from interviews with Indian students, parents, tribal officials, and others is cited in support of an emphasis on training students in skills relevant to integration with the dominant society, at least in the economic sphere. It is difficult to determine whether this attitude reflects an intrinsic positive evaluation of such integration and adaptation, or whether it represents resignation to the status quo. Urban, industrial, Western society (and social theory!) has hammered away at this theme of inevitable cultural amalgamation for so long and at such a critical level that isolated tribal communities are extremely vulnerable to the introduction of just such social objectives; indeed, that is precisely the intent of educational manipulation. Authors like Vine Deloria, Jr. (in *We Talk, You Listen*) argue that alternative social and political structures are possible for tribal communities and for industrial states, and that the tribal tradition may yet play a creative role in the emergence of these new forms. Whether or not one agrees with this position, it is important to emphasize the cost of locating analyses of minority and/or ethnic communities solely within static visions of the contemporary political world. Akzin, Enloe, Deutsch, Barth and others have called attention to the complexity of the ethnic issue, and social scientists ignore it at their peril.

Kickingbird and Ducheneaux's *100 Million Acres* suffers a similar failure to explore deeply the political dynamics behind the formulation of political policy affecting Indian people, although it does offer revealing anecdotes about the roles of political personalities in particular policy decisions. It is, however, in general a more significant book, albeit less "scholarly" in style. The two Indian authors, a lawyer and a journalist, consistently offer careful legal research into the status of the land rights of Indian communities, and, coming from "within" the Indian community, dare to tackle the introduction of a systematic positive program of action designed to lead to the political re-emergence of viable Indian communities. They see consolidation of the Indian land base as the vital first step in the development of realistically self-determining Indian politics.

From the point of view of theoretical inter-

est, the work suffers from certain omissions. The political analysis is largely anecdotal and peripheral; the internal dynamics of decision making are referred to only in passing. Two kinds of work are needed here: First, there is a need for research on the activities and interests of political actors involved in the formation of Indian policy. Perhaps even more fundamental is the need for investigation of the whole area of interaction between "tribal" minorities (which exist, for example, in the USSR, China, India, Australia, as well as Africa and South America) and industrializing nation-states. Such studies often reveal as much about the politics of dominant systems as about the problems of the tribals themselves. As an early Commissioner of Indian Affairs once remarked, Indian Affairs is a bellwether of the health of the American political system. How fruitful then might a new political concern for Indian affairs become?

FRANCES SVENSSON

*The University of Michigan*

**Judge Learned Hand and the Role of the Federal Judiciary.** By Kathryn Griffith. (Norman: University of Oklahoma Press, 1973. Pp. xi, 251. \$8.95.)

John P. Frank has said that "most expert judges of judges would choose the Court of Appeals for the Second Circuit . . . as the ablest court in the United States" (p. 24). From 1924 to 1951 Learned Hand was a member of that court, and his service earned him the reputation as one of the most distinguished jurists of the twentieth century. Judge Charles E. Wyzanski argued that Hand was "universally acknowledged as the greatest living judge in the English-speaking world" (p. 9). In spite of such acclaim, Hand was never elevated to the Supreme Court, although Presidents Harding and Franklin Roosevelt considered the possibility. The work of the Second Circuit, "Learned Hand's Court," in matters of constitutional interpretation, immigration and naturalization, patents, subversive activities, obscenity, and criminal procedure, provides ample opportunity to understand Hand's pervasive influence on American law.

As its title indicates, this book is principally concerned with Hand's conception of the judicial function. Profoundly influenced by his teacher, James Bradley Thayer, Hand falls squarely in the tradition of judicial restraint. "He thought that a judge must decide in accordance with decisions arrived at through the political process without attempting to impose his own values. When it seemed appropriate to

him, he deferred to the higher court, to the legislature, to administrative tribunals, and to the spirit of the time" (p. 84). So thoroughgoing was Hand's deference to popular institutions that during his entire tenure he never voted to invalidate congressional legislation. The importance he attached to judicial limitations did not vary from issue to issue. Like Oliver Wendell Holmes, he argued against the use of substantive due process as a protection for economic and property rights. Through his intellectual association with Holmes, Harlan Fiske Stone, and Louis Brandeis before the "Constitutional Revolution of 1937," Hand's opinions were accorded the label of liberalism owing to his intense efforts for judicial humility. But when American appellate courts turned to issues of free expression and civil liberties after 1937, Hand's reverence for judicial restraint continued with iron-clad consistency. As Professor Griffith explains, his "disagreement with the most active judicial defenders of liberty was not with its necessity to human life but with the proper function of a judge in a democratic society. He . . . did not assume the role of social or moral reformer" (p. 41).

The "new activism" after 1937 was concerned with the judicial protection of civil liberties essential to the maintenance of free government. Its source was Stone's statement on "preferred freedoms" in *U.S. v. Carolene Products Co.* [304 U.S. 144 (1938)]. Just as Hand refused to accept the doctrine of substantive due process as constitutional support for private property, he did not approve the doctrine of preferred freedoms. With such opposition to the new activism, Hand assumed "what by modern standards is a very conservative view of the court's role" (p. 41). In the 1940s judicial restraint was synonymous with a rejection of claims of civil libertarians; during this decade, the judicial philosophies of Hand and Felix Frankfurter were congruent. Yet Frankfurter was willing not only to profess the virtues of judicial limitations in civil liberties cases: he often added to such opinions his personal predilections about the wisdom of a claim. Hand was unbending, more consistent in his devotion to judicial restraint and deference to popular institutions even than Frankfurter. As Griffith explains, however, within the contours of judicial limitation, Hand allowed himself some latitude for creativity. Judges "must remember the pragmatic origin of the law which they seek to interpret, and, while they must not go contrary to it and cannot legitimately go beyond it, they must be a party to its growth and development . . ." (p. 180).

Many will find the author's approach old-

fashioned. For example, she startles the reader, especially the political scientist whose specialty is judicial behavior, by stating that "[f]ew students of the American judiciary would take issue with Alexander Hamilton's assertion that 'the judiciary . . . may truly be said to have neither *force* nor *will*, but merely *judgment*'" (p. 83). Legal realists have shattered the myths of judicial objectivity. Judges are important policy makers regardless of whether they adopt roles of judicial activism or restraint. Throughout the book Griffith's writing manifests an uncertainty about the policy making function. Her inclination, as the choice of Hamilton indicates, is to deny the importance of individual beliefs or will in judicial decision making. And, looking to the future, she says it is "increasingly imperative that judges be thoughtful and self-conscious about their role and refrain whenever possible from policymaking decision" (p. 164). Yet to assume that judges, even by adopting roles of passivity, can avoid policy decisions is an untenable position. Griffith herself is forced to reach such a conclusion as she discusses civil liberties: "Questions arising under the Bill of Rights invariably relate to policy and choice" (p. 134).

Had the author utilized some contemporary concepts in the analysis of judicial behavior, she might have avoided some of the difficult, and occasionally inconsistent, conclusions about judicial beliefs, will, function, and policy choice. As with Frankfurter, an analysis of Hand's court behavior requires an analytic division between personal attitudes or values and role definition. In the concluding chapter, Griffith begins to come to grips with such a distinction: "Learned Hand had a rare capacity to distinguish between his personal philosophy and his duties and responsibilities as a judge . . ." (p. 230). Such a distinction could well have been a more explicit theme throughout the book.

This book is limited to Hand's conception of the judicial function; its materials come almost exclusively from *The Spirit of Liberty*, a collection of Hand's essays published in 1952, and from court opinions. The author has made no attempt to analyze intra-court relations, personal correspondence and memoranda, and other primary source material often important in biography. In this regard, and because it eschews contemporary methods of analysis, the volume is far less significant than Marvin Schick's *Learned Hand's Court* (Baltimore: Johns Hopkins Press, 1970), published three years earlier.

ROBERT G. SEDDIG

*Allegheny College*

**To Be a Congressman. The Promise and the Power.** Edited by Sven Groennings and Jonathan P. Hawley. (Washington, D.C.: Acropolis Books Ltd., 1973. Pp. 258. \$8.95.)

This book is a collection of eleven chapters on various aspects of the congressional process written by former congressional fellows. The topics covered include campaigning, constituency casework, informal clubs, committees and committee assignments, the seniority system, and leadership. The authors bring to their efforts extensive information and insight about Congress, and this is both the major strength and weakness of the book.

A number of the chapters reveal substantial sensitivity to the intricacies of Congress and present a surprisingly large amount of important information in a very readable fashion. But too many chapters take on the tone of insiders telling outsiders how Congress really works with much of the information presented being simply too specific and detailed and devoid of political significance to have much interest for the general reader. For example, the chapter on campaigns is replete with statements like "The headquarters should be attractive and reflect *esprit de corps*" (p. 11), and "A pleasant and cheerful voice symbolizes a friendly, interested, and optimistic candidate" (p. 11), and "Similarly, a banjo player or band can help attract an audience" (p. 16). In short, the campaign chapter reads like a cookbook recipe on how to get elected, with scant attention given to the role of elections in elite recruitment. Similarly, the chapter on freshman socialization devotes too much attention to a specific freshman and does not adequately address the more general problem of legislative adaptation.

The most successful chapters—in the sense of presenting basic information in a systematic way and drawing some implications about congressional behavior from this information—are those on seniority and committee leadership by Jonathan Hawley, leadership by Jean Torcom, informal clubs by Sven Groennings, and Congress and the press by Delmer Dunn. The Hawley chapter on seniority manages to present the standard arguments for and against seniority in a surprisingly interesting fashion and covers quite well recent developments in seniority reform in the House and Senate. The Torcom chapter on the leadership of Everett Dirksen provides an insightful addition to the growing body of literature on leadership styles. Likewise, Groennings's chapter on the House Wednesday Group adds to our knowledge about the functions and operation of informal legislative clubs. Finally, Delmer Dunn's con-

tribution on Congress and the press raises some thought-provoking questions about the role of the press in agenda setting and the comparative use of various media by Representatives and Senators.

The remaining chapters are not as successful, often getting bogged down in unessential detail. Given the many readers available on congressional politics, I would argue that *To Be a Congressman* does not stack up very well for a number of reasons. First, there is no focus or organizing theme to the chapters beyond an attempt to present "an introduction to the practical aspects of congressional life" (p. viii). Second, numerous topics receive little if any coverage; for example, policymaking and legislative oversight are practically unmentioned. Third, the kind of information presented is generally so practical that any notion of Congress as a political institution operating in a political environment tends to be lost. The professional student of Congress will find useful a few of the chapters praised previously. But overall the book remains a disappointment for those hoping to gain important insights into the workings of Congress. As a minor point, the prospective user of the book should not rely on the book jacket as an indicator of the contents of the book. On the jacket are listed such potentially muckraking topics as "The little-known but effective 'Clubs' in Congress," "How the Kennedys win elections with the 'Kennedy Secretaries'" and "Does Congress have any ideas of its own?" The reader expecting to be titillated by such topics is in for a sharp disappointment.

HERBERT B. ASHER

*The Ohio State University*

**Representative Government and Environmental Management.** By Edwin T. Haeefe. (Baltimore: The Johns Hopkins University Press for Resources for the Future, 1973. Pp. xii, 188. \$8.95.)

In his Foreword to this book, Joseph L. Fisher, president of Resources for the Future, says that it "takes us into new dimensions of problems we have long been dealing with along other lines" (p. v). This statement, I think, pretty clearly indicates the importance of the book. It moves *Resources for the Future* (and, one hopes, its next-door neighbor, Brookings) firmly into what we might call the "public choice" arena. In this book, Haeefe not only examines the problems of environmental management but also, as the title says, representative government. The focus is on the design of institutions of government that have some reasonable probability of producing good deci-

sions about the environment. Even those who do not agree with Haefele's argument in detail will agree with his stance and approach to the problem.

The bulk of the book is a set of mildly altered papers which Haefele has read at meetings or, in some cases, has published elsewhere. Like all such collections of papers, it suffers from a certain lack of focus and some repetition. These defects, however, do not greatly reduce the value of the book. Each problem in the book is discussed in a way that brings the political decision-making apparatus into the foreground of an environmental problem. As those who have been following Haefele's formal work in public choice will anticipate, there is a considerable emphasis on logrolling as a method of reaching suitable conclusions. Indeed, his mathematical logrolling model is presented here in greater detail than anywhere else I have seen it. Looked at from the standpoint of specialists in public choice, this mathematical model which permits investigation of various logrolling activities is the most interesting part of the book. At the practical level, perhaps his suggestion of the "general purpose representative," a representative for a given geographic area who serves on a number of different boards dealing with different subjects and having different geographic coverage, is perhaps the most interesting innovation.

Both those who have been working on environmental management and know little or nothing about the modern approach to politics and those who have been working on politics will find much in this book to think over carefully. I think that it would be most useful for graduate courses in environment and even for some advanced undergraduate courses.

GORDON TULLOCK

*Virginia Polytechnic Institute  
and State University*

**Black Civil Rights During the Johnson Administration.** By James C. Harvey. (Jackson: University and College Press of Mississippi, 1973. Pp. xiv, 245. \$4.95.)

This account of the administration which saw the strongest commitment to equality for black Americans in our history offers a compact and useful summary of a historic period of change. Relying extensively on a wide variety of published materials, Professor Harvey gives a chronological account of the forging of the major statutes and summarizes some of the policy disputes and results of the enforcement process. The book makes extensive use of excerpts from contemporary accounts and speeches.

The study underlines the unique importance of the civil rights decisions of the Johnson period and makes one wonder why they have received so little serious attention from political scientists. The extraordinary use of federal power to transform some basic social and educational practices of a vast region of the country and to raise fundamental issues about our urban future has generated little scholarly inquiry. The experience is often dismissed on campus with a cliché, usually a false cliché. Perhaps this book will begin to stir interest in the research that needs to be done.

There are several reasons for the thin research base, I suppose, when civil rights is contrasted with a field like environmental protection. President Johnson is hardly a sympathetic figure for most academics, and it is often difficult for people mobilized against the Vietnam War to concede the elemental importance of President Johnson's civil rights record. In a discipline with little surviving tradition of public administration research, few political scientists have either the training or the interest necessary to study adequately the administrative enforcement process which has touched so many important aspects of American society. Finally, research has been hampered by ideological disarray. The emergence of the black power movement and the development from the simple issues of lunch-counter segregation and voting discrimination in the rural South to the very complex and emotional questions of school and housing segregation in northern metropolitan areas may have discouraged research. At any rate, Professor Harvey's broad search for data has turned up little significant work by political scientists.

While this book takes the Johnson contribution seriously, it is in no sense an exercise in hero worship. The narrative is laced with comments of contemporary civil rights critics. Each forward motion, of course, tended to reveal more basic difficulties. The book illustrates the difficulties of liberal officials attempting to cope with problems which simply would not yield to the incremental approaches so basic to the American political and administrative styles. Although the administration was achieving major breakthroughs, its best critics knew that the results would be flawed unless much more was done, sometimes more than the President had the power to do. Harvey moves increasingly toward identification with the critical view as the book progresses.

The principal asset of this book is also its weakness. Its comprehensive character and its effort to incorporate a wide diversity of sources makes it useful as an introduction to the period



or a reference source more than as a definitive analysis. The author relies very heavily on secondary sources, often giving substantial weight to speculative political commentaries. The book does not build a unified interpretation, and it offers little discussion of the significance of the period for our general understanding of the political system. This lack of a perspective at times leads to an underestimation of the Johnson accomplishments.

At a time when the presidency is discredited and political cynicism is rampant, reviewing President Johnson's civil rights record is a refreshing and restorative experience. While it is true that President Johnson offered only tentative first steps toward resolving the intense segregation of our great cities or the crippling economic crisis of poor blacks, he did move decisively and effectively against particularly vicious forms of segregation through law in the South. Blacks vote and hold office in the Deep South and attend schools substantially more integrated than those in the North because of processes his administration set in motion and sustained in the face of enormous political pressure. LBJ knew that this record was his central gift to the country, and he knew that the achievement was incomplete. His last public appearance was devoted to a passionate appeal to the country to finish the social revolution he helped set in motion.

After President Johnson died, many whose lives were touched by his Administration paid tribute. As his body lay in state in the Capitol, people waited in a line a half mile long on East Capitol Street at 3:00 A.M. Many of those waiting were black parents holding the hands of small children. They were people who could remember a southern President telling Congress about his early experiences of discrimination and pledging that "We shall overcome." They waited to honor a man who shared with Martin Luther King much of the credit for the Second Reconstruction. This book will help the reader understand why they were there.

GARY ORFIELD

*Brookings Institution*

**Unwanted Mexican Americans in the Great Depression: Repatriation Pressures, 1929-1939.** By Abraham Hoffman. (Tucson: The University of Arizona Press, 1974. Pp. 207. \$9.75, cloth; \$4.75, paper.)

During the Great Depression, many Mexican nationals were repatriated from the United States. This bitterly controversial event has been almost completely neglected by scholars. Professor Hoffman's study is the first book-length account of it. For research sources he

has relied heavily on original papers and documents from a variety of archives. The result is an eminently readable book containing a wealth of new information.

Before discussing repatriation itself, Hoffman surveys the movements of Mexicans to the United States before 1929. Pushed from Mexico by extreme poverty and political instability, these immigrants had little opportunity to bargain for economic and other rights in the United States. The fact that they were forced into the lowest rungs of American society aggravated pre-existing anti-Mexican stereotypes. According to Hoffman, widely held stereotyped assumptions about Mexicans were major components of the rationale for repatriation.

Considerable attention is devoted to the vigorous but unsuccessful interest-group effort of the 1920s to amend the immigration laws to rigidly limit Mexican immigration. According to Hoffman, the onset of the Depression markedly improved the political influence of restrictionist interest groups. The development of widespread unemployment made exclusionary arguments increasingly persuasive, and the national government came under massive pressure to expel illegal Mexicans. Hoffman correctly emphasizes anti-Mexican prejudice as an important source of this pressure, but that theme is so pervasively woven into the book that other possibly valid explanations for repatriation are underemphasized. For example, the reader of Hoffman's book will get little appreciation of the extent to which employers used illegal Mexicans to displace American workers, to fight unionization, and to freeze wages and working conditions. These considerations were important sources of repatriation pressures, and they were not always connected with prejudice against Mexicans.

Hoffman is critical of the federal government's repatriation role. He notes that government authorities had been indirectly responsible for the large accumulation of illegal Mexicans during the pre-depression years. In deference to politically influential employers, there had been little effort to eliminate the widespread and well-known use of illegal Mexican labor. The American government is portrayed as suddenly reversing itself and punishing Mexicans for doing what it had at least indirectly been encouraging them to do for years.

Immigration authorities staged "raids" from coast to coast. When apprehended, illegal Mexicans were given a choice between voluntary departure and deportation. Many American citizens of Mexican ancestry were also

detained for questioning, and many American-born children (who were automatically American citizens) were sent to Mexico with their parents.

Important as the deportation drive was, Hoffman notes that it was not directly responsible for the exit of most returnees. Many left the United States simply because they had lost their jobs. Many others left under the prodding of state and local governments; this pressure commonly took the form of denying certain jobs or welfare benefits to non-citizens. Hoffman presents an extensive case study of the ambitious repatriation drive conducted by Los Angeles County. In addition to focusing on the evolution and operation of this program, he devotes considerable attention to clarifying its relationship to repatriation policies of the American government and to non-governmental drives sponsored by private interests in the Los Angeles area. Hoffman provides a good deal of new information in a chapter on the repatriation role of the Mexican government.

The title's suggestion that the book is simply about pressures to repatriate "Mexican Americans" is misleading. That concept commonly refers to American citizens of Mexican ancestry (and, except on p. xiii, Hoffman follows that usage). Although American-born children were sent to Mexico with their parents who had illegally entered the United States, the overriding concern of most repatriation interests was the departure of *illegal* Mexicans, and that is in fact the predominant concern of this book.

GEORGE C. KISER

Illinois State University

**Inequality: A Reassessment of the Effect of Family and Schooling in America.** By Christopher Jencks et al. (New York: Basic Books, Inc., 1972. Pp. 399. \$12.50.)

The tie between policy and school was once a traditional concern in the study of politics, but, for reasons now unclear, political science has been turned away from that connection for most of this century. Our thin interest explains why recent controversial education studies have lacked contributions by political scientists. These studies (the "Coleman Report" in 1966, U.S. Commission on Civil Rights' *Racial Isolation in the Public Schools* in 1967, Mosteller and Moynihan's *On Equality of Educational Opportunity* in 1972) reached a climax in late 1972 in the volume reviewed here. Of these analysts Moynihan is the only one with an understanding of for-

mal political science, although he has not claimed this often or loudly in public places.

Yet this volume by Jencks, both in what it does and does not say, relates to our current emphasis upon policy outcomes. For this book challenges an explicit assumption of policy macroanalysis, namely, that policy can significantly affect the distribution and redistribution of social resources. Rather, *Inequality* claims that an ostensibly powerful institution—the school—has little independent influence upon citizens' "success," as measured by income or occupation. Indeed, the labyrinth of its tables and regression analyses, relating independent measures of school resources to school outcomes, seemingly suggests that "nothing affects anything," as Lester Thurow noted.

The press interpreted the results to mean that "schools made no difference" in improving life chances. While false in conclusion, this judgment precipitated political action. Taxpayer groups, already resisting the school costs in referenda contests, could find solace in a scholarly volume. In Washington, Moynihan, discouraged by his findings that the marginal benefits of using school investment for equalizing incomes were low, encouraged the Nixon Administration rather to consider the family assistance plan. That, too, however, failed for lack of will in the White House and opposition in Congress.

Nevertheless, the book became an instant source of criticism among diverse social scientists (but never among political scientists) because it touched so many fields. Yet these critics always found some element of analysis of conclusions with which they agreed. Partly that is because the book covers so many interactions of school and society that one is bound to find *some* agreement *somewhere*. Thus some conventional wisdom is confirmed, such as the high association between family status and academic achievement. The agreement also arises from the realization that school reforms of the 1960s did not come up to expectations, whether because of inadequate design (a familiar Moynihan criticism) or inadequate resources.

But this book is most contentious because it challenges scholarly notions that schooling can serve as a major vehicle of upward mobility, thus making for "success" in the American culture. Jencks finds, when the unit of analysis is the individual and not groups of students (as in the Coleman Report), that "schools serve primarily as selection and certification agencies, whose job is to measure and label people, and only secondarily as socialization

agencies whose job is to change people. This implies that schools serve primarily to legitimize inequality, not to create it" (p. 135).

In this review, I will raise only a few points. First, in focusing upon success and certain measures of income inequality, how can the authors know that they have chosen the "correct" or, indeed, the only output which schools may influence? Or, why do they assert (without citation) that this or other social institutions serve—or should serve—*equalizing* functions in a democratic society?

On the question of outcomes, Jencks does note that there may be more important things than academic success achieved in school, such as happiness, the reduction of racial antagonisms, or other affect qualities. Indeed, they claim that school resources may be justifiable on these other accounts alone. Of course, the affect domain is harder to quantify and validate than the cognitive, but efforts which focus only on the cognitive effects of schooling do not give a full account, and possibly do not treat the most important aspect of education. In this volume, it is true, there is some coverage of school influences upon affect qualities, but it is remarkably skimpy. The defense that cognitive effect data are the most easily quantifiable is too thin when we recognize that major policy recommendations (some implicit and yanked out of these scholars' work by others) are being based upon these data. That is, such recommendations might not be supportable if we knew more about the positive influence of schools upon non-cognitive aspects such as attitudes and emotions.

Another assumption of this volume is that schools ought to be able to redistribute the social resource of "success" more equitably. But it is basic sociology that schools, like any institution, here and elsewhere, are conserving mechanisms, reinforcing the existing distribution of resources and values. Certainly political scientists are familiar with this distributional inertia in the prevalent findings of the small ratio of outputs to inputs in the political system, whether one measures municipal ordinances, judicial appeals, congressional actions, or even the ubiquitous school board actions.

Why then should we expect to find that the school system increases income for most individuals when that may well not be its purpose at all nor the reason most students attend? Schools, themselves political systems, have always distributed rewards and penalties. Plato explicitly set out a reallocative mechanism in *The Republic*, and while schools today may not contain the direct equivalents of his gold,

silver, and brass, they still reallocate valuable resources. Some students get high grades, some make the student council or band, become cheer leaders or class presidents—but most do not. The failure to receive good grades has bred, as Lewis Dexter notes in *The Tyranny of Schooling*, a society-wide fear of being regarded as stupid, testifying to the reallocative—not equalizing—nature of that policy.

Further, because attendance is everywhere compulsory, we cannot assume that all students attend in order to "better" themselves by achieving subsequent "success." What they find is a miniature of society. One need not call for radical reconstruction or "deschooling" to understand that the schools continuously offer students a working model of inequality. Twelve years of participating in that inequality surely cannot condition many to expect much more in the postgraduation reality.

Besides questioning basic assumptions, I should also like to ask whether political scientists might not approach the same data in any way different from these authors? The book's paradigm relates characteristics of students and their schools to measures of economic success, but it is silent on the role of the political system, aside from some early and brief statistics on the variation among states and school districts in expenditures, without relating it to the major dependent variable. What is not conceptualized is the difference, if any, made by the political qualities of schools.

Part of the omission may be due to the authors finding that there is far more variation *within* schools systems than *between* them, as did Coleman. If so, a hasty inference might be that the political factors are independent of these intraschool variations. But the pattern of variation *within* schools is not identical from system to system, and so political qualities might be hypothesized as precedent factors shaping those intersystemic variations. As with the most recent macroanalysis, the political system may be found more influential in redistributive policy results than was thought a decade ago. Certainly redistribution analysis is needed to show what states do with the school funds allowed by their economic environments. Similarly, at a substate level we need to know what effects upon school outcomes and subsequent success are traceable to: the political climate of the classroom, school dependency upon the local government, the impact of the elections surrounding schools, group conflict over locally hot issues like desegregation, curriculum reform, taxation, and so on. These

are questions not touched upon in *Inequality* or in much current research by political scientists.

Nor is much attention given in *Inequality* to the particular combination of public policies which *does* have an effect. The recent controversies over such school matters as race, unionization, student rights, community control, and finance are alike in creating inordinate waste because of the typical randomness by which Americans approach any reform. But, in the end, *some* policies are successful even though the bulk of them are not. Thus in matters of desegregation, Smith and Downs have shown workable methods of *Achieving Effective Desegregation* (1973), while studies of the U.S. Office of Education by Robert Crain and others, have described combinations in the South that depress racial polarization and increase achievement among students. But in the basically pessimistic mood in which *Inequality* proceeds, nothing seems to explain much of anything that is measurable, and hence the authors need to fall back upon the extraordinary nonexplanation of "luck" as the cause of "success."

Given the poverty of theory suggested by this nonexplanation, another strategy might be to employ grounded theory. This begins by examining policy alternatives which *do* seem to accomplish their program objectives. Such an approach, while lacking the elegance of inductive theory, seems far more preferable when induction only leads to speculating about "luck." In another era, we would be talking about "God's will," always an uncertain source of prediction.

*Inequality*, while important for those in the public arena of school policy, is also a contribution to the sociology of knowledge. Ultimately, it seeks to deal with the best available theory and methods to explain the interactions of individuals and institutions. Given all that, however, it is a highly sophisticated demonstration of how little we really know about that interaction. Many will not be as pessimistic about schools as the authors of this volume, because the institution provides a means for at least some citizens to improve their life chances, and possibly that is the most one can hope for. Jencks, in the book's conclusion, is dissatisfied with even this gain. The reform tradition of improving individual chances by "ingenious manipulations of marginal institutions like the schools" will not do, he argues, for "we will have to establish political control over the economic institutions that shape our society. This is what other countries

usually call socialism. Anything less would end in the same disappointment as the reforms of the 1960s."

Despite the question raised here, I must emphasize that many of the insights and policy recommendations are very impressive and address a major social issue in an ambitious way. A book is important in social science not because its authors are "right" but rather because they try to deal with major questions of public life by using broad theory and sophisticated methods. In those terms this is surely an important book.

FREDERICK M. WIRT

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**The Knapp Commission Report on Police Corruption.** (New York: George Braziller, 1973. Pp. 283. \$8.95, cloth; \$3.95, paper.)

This is a narrow, specialized, practical book, a book which was not intended to raise general, theoretical issues, but which does so in spite of its own best efforts to the contrary. It is easy to say what this book was intended to do—namely, to describe, in great detail the pattern of police corruption in New York City and to prescribe some ways to reduce or eliminate it. It is, however, not so easy to grasp what it accomplishes along the way. Mixed in with the rather dry and often repetitive prose which seems characteristic of reports of this kind, one encounters interesting insights into the nature of public organizations in general, and police organization in particular, problems in the administration of justice and the difficulty of analyzing political corruption.

In terms of its stated purpose, the report succeeds, in spite of one significant flaw, in providing a set of striking and startling findings. The flaw involves the Commission's failure to define what it means when it talks about police corruption. Nowhere in its report is there a single standard or definition which the Commission employs to judge what is or is not corrupt behavior on the part of the police. As a result, when it states as its conclusion that corruption is so widespread as to be "... indulged in to some degree by a sizeable majority of those on the force and protected by a code of silence on the part of those who remained honest" (p. 61), the reader is given no idea of the basis upon which this judgment is reached. Instead of providing a definition or standard which would serve that function, the Commission merely describes a great variety of activities, ranging from burglary to accepting free lunches, which it believes inap-

propriate forms of police behavior. Readers looking for a precise delineation of the parameters of police corruption will be disappointed.

In spite of the problem of this imprecision in its use of the term corruption, the Commission is able to provide a fascinating description of the situations in which police become involved in clearly questionable practices. These situations include providing protection for illegal gambling establishments, selling narcotics, overlooking building code violations on construction sites, and tolerating illegal parking in the commercial district. According to the Commission, most of these situations, in which the opportunities for corruption are realized, result from an overextension of legal regulation. This overextension in turn is produced by laws against victimless crime and ordinances which create unnecessarily complicated regulatory procedures. These types of laws invite corruption because they require no complaining party; they enable police to exercise unchecked and unsupervised discretionary power. This power, which can be used to facilitate as well as disturb the activities of both legal and illegal enterprises, is itself a valuable commodity, which the Commission believes, gives the police something important to exchange. Because they have this power, many policemen are able to arrange continuing, cooperative relationships with gamblers, prostitutes, and others for whom police forbearance is an important asset; because laws against victimless crimes and complicated regulatory ordinances are on the books, policemen can act as entrepreneurs, offering to "look the other way" and to under-enforce laws which many people believe should not be enforced anyway.

Believing as it does that bad laws make bad police, the Commission sets out as a necessary first step in the fight against corruption the repeal of laws against gambling, prostitution, certain forms of drug use and many outdated regulatory ordinances. While there is considerable merit in this recommendation, it is supported by the wrong kind of reasoning. The Commission sets out what is, in effect, a plan to tailor the making of laws to suit the practices and preferences of those who are supposed to enforce them. Such a plan would seem to put the cart before the horse. Furthermore, if the opportunity for corruption inheres in the exercise of police discretion, as the Commission believes it does, then merely changing a few laws will not get at the heart of the problem. If social science research on

the police has taught us anything, it is that the exercise of discretion is part of almost every routine police activity. The Knapp Commission, by equating the exercise of discretion with the enforcement of laws against victimless crime, ignores this lesson.

The Knapp Commission, in addition to studying the effect of certain kinds of laws on police corruption, examined the way in which the organization of the New York City police contributed to the problem. It describes in considerable detail the way the N.Y.P.D. is structured and pinpoints three main problem areas in department organization. The first involves the fragmentation of authority and the difficulty of command supervision. The strength of precinct organization in the department makes it very difficult, according to the Commission, for police officials with a "cosmopolitan" perspective to oversee police activity at the grassroots. As a result, corruption often characterizes entire precinct organizations from top to bottom. Second, the absence of an effective intradepartmental system for disciplining officers suspected of corruption reduces greatly the risks involved in engaging in corrupt activities. Third, and most important, the Commission suggests that the special outlook on the world which characterizes the department encourages the development of what might be called a "culture of corruption." This culture is one offshoot of the more general police culture which emphasizes group solidarity and interpersonal loyalty against a hostile and suspicious outside world. Police attitudes, according to the Commission, condone corruption as something everyone does, as a way to compensate for the lack of appreciation which is the policeman's lot.

While the Knapp Commission makes recommendations that seem to speak quite well to the first and second of the organizational problems, the Commission has little of value to say about the third. The Commission's study of corruption is disappointing in at least two other respects. First, the Commission makes no effort to identify and examine linkages between police corruption and corruption in other government agencies or in politics generally. It treats as an isolated phenomenon what may, in fact, be quite general in scope. Second, the Commission presents no evidence as to the extent and nature of police corruption in other cities. As a result, while true to the Commission's limited mandate, its report can serve as no more than a single case study of what may be a widespread problem. In this regard as in much else, the Commission's re-

port raises as many questions as it answers and seems not to have raised many of the hard questions which needed to be asked.

AUSTIN SAPIAT

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**Professors, Unions, and American Higher Education.** By Everett Carl Ladd, Jr. and Seymour Martin Lipset. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973. Pp. 124. \$1.75, paper.)

Collective bargaining among faculty in higher education, though proceeding less extensively and rapidly than among public school teachers, is developing. It is an important subject but one that thus far has occasioned more heated than illuminating analysis. All the more welcome, therefore, is the present volume, slim but meaty, which usefully treats some central concerns. Effectively drawing upon a massive Carnegie Commission survey of faculty in 1969, a close inspection of available campus experience, and selective coverage of the relevant literature, Ladd and Lipset provide instructive description and interpretation.

Among the greatly varied units of higher education, collective bargaining to date has been disproportionately attractive to two-year community colleges. This suggests an explanatory variable emphasized by Ladd and Lipset, namely, that the least profession-like faculty sectors are most receptive to faculty unionism. Two-year colleges provide almost an exaggerated instance of the rule; their status is closer in many ways to the K-12 public school system than to higher education, leading their faculty to support of unionization to enhance their professional standing.

A changing environment helps explain the growth of faculty unionism in four-year colleges and universities, allowing for the fact that a sizable segment of the professoriate remains opposed to unionization on traditional grounds. Readers need no reminder that the boom period for higher education has ended and that economic retrenchment is the order of the day. Faculty concern over protection of its prerogatives and standing has been further heightened by the student power thrust and by the enveloping mood of egalitarianism and antielitism. In the public higher education sector, the site of nearly all the faculty collective bargaining that has occurred, two additional explanations may be offered. One is the increasing scale of higher education units, including gigantic multicampuses, which has promoted bureaucratization and impersonalization, has boosted central authority at the ex-

pense of campus autonomy, and has reduced collegiality between faculty and administration. The other is the enactment by nearly half the states of legislation authorizing collective bargaining by public employees and/or those in higher education, which has spurred organizing elections on some public campuses. Although faculty at private institutions have had the right to organize under NLRB jurisdiction since 1970, collective bargaining has made little inroads in that sector.

The broad lines of faculty support and opposition on faculty unionism follow class interest: "the lower the tier of academe, in terms of security, income, prestige, and involvement in the graduate scholarly-research culture, the stronger the vote for unionization, as represented by a regular union body [like the AFT or the NEA]; the higher the level, the greater the likelihood of votes for 'no representation,' or for the least 'union-like' faculty organization on the ballot" (p. 49). In addition, say Ladd and Lipset, the political attitudes of faculty members comprise an independently influential explanatory factor: liberal-to-left professors are more open to collective bargaining than are faculty of a politically conservative outlook. The two variables interact in interesting ways. Faculty at elite universities, for instance, tend to be more left-wing politically than faculty elsewhere (which moves them to favor unionism in principle), but at the same time they are most deeply attached to academic norms of achievement and merit (which move them to oppose unionism and its leveling ideology). Thus far, Ladd and Lipset show, their conflict has been resolved mostly in favor of class interest, i.e., no unionism. In contrast, generally the younger and untenured faculty find their political beliefs and their class interest reinforcing, which makes them as a group more receptive to the advent of faculty unionization. Interestingly, the class origins of faculty fail to explain intrafaculty variation on unionism; the authors' analysis indicates that professional socialization seems to be far more controlling.

Intrafaculty differences on unionism underscore the practical importance of unit composition to the outcome of organizing elections. The election results may turn, the authors remind us, on whether, in a multi-campus university system the unit is defined as the regular faculty in each campus separately or in the system as a whole. The same comment applies to situations in which the university faculty and the faculty of the other public higher education institutions are assigned to separate units or combined into one. Similarly, differ-

ent election outcomes are likely if the regular faculty members are merged with other teaching/research/professional staff in a single unit or are permitted to comprise their own unit. The determination of the unit can be set directly in the state legislation authorizing collective bargaining or can be assigned to a state board, subject to criteria specified in the legislation. Whichever is the case, faculty organizations would be wise to move early to shape a legislative position on unit definition protective of their particular needs and interests.

What are some expected effects of faculty unionism, especially if an industrial union model is approximated? Only experience can tell, but in considering the speculations of Ladd and Lipset and of other writers on this subject, the following union means or objectives seem to me to be highly plausible probabilities: (1) to eliminate salary differentials (merit raises, etc.) among those in a given job category, with the exception of a recognition of seniority in service; (2) to make reappointment, tenure, and promotion more automatic in terms of time served, with the institution assigned the burden of proving a negative decision; (3) to insist on open personnel files, easier grievance procedures, and a more formalized process for review of faculty; (4) among more marginal faculty groups (lecturers, researchers, etc.), to move to attain the status and privileges of the regular faculty; (5) to increase an adversarial posture between faculty as "employees" and administration as "management," though it should be noted that some redistributions of power would be from the senior tenured faculty to the junior faculty; (6) to pursue higher salaries and other economic matters by direct dealings with the legislature and governor; (7) to develop a type of faculty leadership different from the "institutional oligarchs" in charge of faculty senates, and to hasten the decline of such traditional modes of institutional and faculty governance; finally (8) to reassert and maintain faculty prerogatives on a wide range of institutional and policy matters, and from a vantage point not necessarily consistent with the interests of students or of the public.

Whether faculty collective bargaining is viewed as boon or bane, as degrading or promoting professionalism, its potential for altering higher education and the role and place of faculty cannot be denied. Hence political scientists would do well—in the manner of Ladd and Lipset—to monitor its evolution closely and perhaps even to shape its development by helping to construct a new type of unionism

appropriately sensitive to what so many of us refer to as the "special" character of higher education teaching and research.

ALLAN P. SINDLER

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**Community and Regional Planning: Issues in Public Policy.** By Melvin R. Levin. (New York: Praeger Publishers, 1972. Pp. 362. \$15.00.)

Levin's book is a collection of fourteen of his essays, written primarily during the mid-1960s and updated by occasional reference to more current events for this second edition. Its main focus, however, is programs, events, and issues of the 1960s. The somewhat dated quality is counterbalanced by the opportunity the volume gives us to see what was on the mind of a professional planner and how he defined and responded to the public policy problems of that era. The range of topics Levin covers is very wide: the role and availability of intellectual talent in state government, PPBS and its antecedents, planners and metropolitan planning, transportation factors as independent variables in institutional planning, aspects of distressed regions and regional commissions, and various problems of state and local governments.

Even the above listing does not indicate Levin's scope. He is a discursive, undisciplined, and unedited writer. For example, in his essay on "Costs, Benefits, and Social Indicators," he connects PPBS to its ancestors in the cost-benefit analyses of water projects in the 1940s and 1950s, then discusses indicators in general, then revenue sharing and the concept of governmental decentralization, slips off into an analysis of New York city problems, and ends with a case study of what is wrong with administration in Massachusetts state government, with details of capital budgeting and data processing shortcomings. Or, in "The States and Urban Programs," Levin comments upon: current events in the Nixon Administration during 1971-1972, Patrick Moynihan's ten-point urban program, the creation of the Domestic Council, the ambiguities of the word "policy," the problems of urban political action, and the role of the private sector and of research in problem solving. He then archly declaims (p. 297): "Now that the federal government has been properly discussed, it is time to start on the states. Clearly, any discussion of state problems runs the risk of generalizing the way sophomores do about women or some people do about Negroes, Chinese, or other seemingly discrete groups or cate-

gories." Levin then moves on to discuss the weaknesses of state governments and to deliver some prescriptive maxims for policy-makers (policy should run *with* rather than counter to developmental trends, there must be more emphasis on public affairs education in the schools).

This book will be maddening to any political scientist hoping to find hypotheses to test against data or any rigorous use of empirical methods. In essays on distressed regions, there are solid data on employment and population mobility. But throughout the essential mode is that of the "authority" describing any aspect of the passing scene that strikes his fancy and rendering strong *ad hoc* judgments and prescriptions.

The causal diagnoses that result, and their philosophical implications for the planning discipline are very disturbing. Levin's constantly reiterated conclusions amount to one point: the problems of state governments, of the regional commissions, of federalism, stem from an absence of "talent." He says (p. 307) that "this may neither be the first nor the last time in history that noble concepts and imaginative plans were degraded by faulty execution, but there is something rather odd in the spectacle of major programs in a major nation faltering for want of a few thousand trained professionals."

How do we know there has not been enough "talent?" By what absolute or relative standards can these judgments be made? There are no data in this book on the characteristics of personnel in terms of education, experience, or attitudes. There are no indications why "talent" is the explanatory variable in assessing the impact of federal-state, state, or urban policies, or, indeed, what the criteria for evaluating these policies as either successes or failures might be. Although Levin is aware of the impact of bureaucratic rivalries upon policy in the abstract, there is little attention paid to political processes in setting and administering programs.

What we have then is an unintended substantiation of Alan A. Altschuler's thesis in *The City Planning Process* (Ithaca: Cornell University Press, 1965). While planners assert judgments upon all aspects of public life, their perspective has no explicit theoretical underpinnings. What comes forth are aggressive assertions without substantiation, the net effect of which is argumentation in defense of the planner's own biases. One can be intrigued with and even sympathetic toward the values of Professor Levin. But it is hard to see how

the essays of *Community and Regional Planning* advance our understanding of public policy at any level or of the rationale for planning.

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**American Labor History and Comparative Labor Movements: A Selected Bibliography.**

Edited by James C. McBrearty. (Tucson: University of Arizona Press, 1973. Pp. 262. \$7.50.)

This work organizes 3,187 sources under two major headings, American and Comparative. American listings are grouped by chronology, although the "Recent" section is topical as is a lengthy "Special Topics" section. These topical sections include subjects such as "public employees," "foreign policy," "immigrant and minority groups," "cities and states," "labor and the law," and "labor and politics." The Comparative listings are grouped by country, except a brief listing of general surveys. This whole system of categorization is repeated twice: once for books and once for articles. I found this feature a drawback, since it necessitates looking up a given topic twice. An index of authors is also included.

McBrearty excludes foreign-language publications, and, in general, the book is weighted toward American labor studies. Emphasis is on academic sources, although business, professional, and radical sources are given attention. Primary sources are not included in the survey. McBrearty, an economist and professor of labor law, gives due weight to these subjects in particular.

Apart from general categorization, there is little to guide the unfamiliar user. Citations are not annotated, nor are there analytic introductions to sections. For articles, there is considerable overlap with *Labor History's* annual review, though both references may be consulted with profit. *Labor History*, it may be noted, reviews *Dissertation Abstracts*, which appears to be omitted from McBrearty's volume. *Labor History* also seems to me to be a better source for articles on relations of labor to the left. Overall, this volume provides a helpful but intentionally not comprehensive starting point for researchers laboring in these fields. Political scientists, notorious for their benign neglect of labor studies, may benefit particularly from the basic sources conveniently set forth in this reference work.

G. DAVID GARSON

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**Independence on Trial: Foreign Affairs and the Making of the Constitution.** By Frederick W. Marks, III. (Baton Rouge: Louisiana State University Press, 1973. Pp. 256. \$10.00.)

The rejection of the Articles of Confederation and the subsequent adoption of the Constitution in 1787 have been viewed historically as dissatisfaction with socioeconomic conditions in the early stages of this nation. This position has been made popular through Charles Beard's classical study, *An Economic Interpretation of the Constitution of the United States* (New York: Macmillan, 1913). Some political historians, however, are beginning to consider the impact of foreign affairs as perhaps the most important variable in the Constitution's drafting. Beard's assumptions and methodologies also have been criticized by Robert E. Brown in *Charles Beard and the Constitution: A Critical Analysis of "An Economic Interpretation of the Constitution"* (Princeton: Princeton University Press, 1956) and Richard B. Morris, "The Confederation Period and the American Historian" *William and Mary Quarterly*, 13 (April, 1956), 139-56. Moreover, other authors are starting to reinterpret the significance of Shay's Rebellion in both its impact and possible reflection of foreign intervention by the British in 1786.

Frederick Marks III in *Independence on Trial* does not seek to minimize the role that socioeconomic conditions have played. In fact, he states that the traditional theory still endures and is the subject of a most recent book by Gordon S. Wood, *Creation of the American Republic, 1776-1787* (New York: Norton, 1972). But the author contends that problems of foreign affairs—British trade restriction, national security factors, patriotism—should be viewed in conjunction with the theories of Beard and Wood.

The Articles of Confederation sufficiently decentralized the power of government so that Congress could not provide for the national defense of the country. It lacked, according to Professor Marks, the "... two prerequisites for a satisfactory national defense under the Articles. The first was exclusive control of foreign relations; and the second, an independent source of revenue" (p. 47). As the author indicates, although Congress had the treaty power, state governments acted independently in violating British-American agreements and federal Indian policies. Furthermore, since Congress could not tax and raise revenue, the entire range of national security problems—including troop size, military nego-

tiations, and the national debt—suffered. It was the weakness of "national insecurity" which proved to be a major factor in the demand for constitutional reform.

The British, according to Marks, were aware of Congress's inability to act on military and trade problems. Lord Sheffield contended in *Observations on the Commerce of the American States with Europe and the West Indies* (London, 1783) that:

No treaty can be made with the American states that can be binding on all of them . . . When treaties are necessary, they must be made with the states separately. Each state has reserved every power relative to imports, exports, prohibitions, duties, etc. to itself (p. 55).

The trade advantages were apparent and they partially closed the West Indies to American ships on July 2, 1783. Marks contends that when the individual states later sought to counter with their own protective tariffs, they found their efforts conflicting. Each state sought trade advantages vis-à-vis the other and the British were able to benefit.

It is the author's view that the failure to set a common trade policy led to the Annapolis Convention in September, 1786. Because of the absence of some delegations, major decisions were postponed. Those that did attend, proposed a constitutional convention to be held in Philadelphia. Hamilton argued, in the final report at Annapolis, that the new convention should not be restricted to trade factors, but should also consider the "other parts of the federal system" (p. 93). Foreign trade problems were viewed as being interrelated with the general need to broaden national power. Professor Marks states that a consensus developed whereby the federal government would be given increased authority in "taxation, military establishment, regulation of foreign commerce, and treaty enforcement" (p. 143). Although there were differences in particular plans, the need for reform was recognized in Philadelphia. The final document was viewed as a "tonic for the national spirit," as it would alleviate the major problems of the Articles.

Marks asserts that the weakness of the American defense and the commercial disadvantages that were apparent under the Articles were two of the primary positions used by the Federalists on behalf of ratification. Madison, Hamilton, and Jay argued in the *Federalist Papers* on the need for a more unified government that would stress common interests and be capable of negotiating with foreign powers. The anti-Federalists, according to

Professor Marks, did not address themselves to questions of foreign affairs nor issues of national security. The author cites works by Richard Henry Lee, George Mason and Elbridge Gerry as examples of anti-Federalist writers who were concerned more with domestic considerations and the possibilities of governmental centralization. Social and economic issues were important, he feels, but the increased national power was ultimately justified because of the necessities of foreign affairs and national security. The benefits of ratification were an improvement in the nation's military, economic, and trade positions.

*Independence on Trial* is a well-written and highly informative study of the impact of foreign affairs on the writing and ratification of the Constitution. The author provides an interesting interpretation, good documentation, and a solid bibliography. This rewarding work is a fine contribution to political science.

CARL L. FIGLIOLA

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**Managed Integration: Dilemmas of Doing Good in the City.** By Harvey Luskin Molotch. (Berkeley: University of California Press, 1973. Pp. 250. \$8.95.)

Professor Molotch's *Managed Integration* is a sobering and instructive book. It is sobering because it documents the futile efforts of a citizens' commission in Chicago's South Shore to "manage" in-migration to their racially changing community and to maintain its solidly middle-class character by retaining white households. The book is instructive in several respects: (1) some of the tactics used by the South Shore Commission are transferrable to other areas; (2) some of the commission's strategic errors pointed out by Molotch could be avoided by community groups elsewhere; (3) the commission has not attained its original goals in South Shore, but several of its achievements have substantially improved the neighborhood environment for newly arrived residents; and (4) the South Shore example adds to the evidence of the limited capability of local neighborhoods to change—or, often, even to influence—the direction of powerful metropolitan-wide trends.

South Shore, as the name implies, fronts on Lake Michigan in the southern part of the City of Chicago. It is below Hyde Park and, like that community and other lakefront areas to the north of the central business district, was an affluent neighborhood of mansions, high-rise buildings with large apartments, and substantial middle-class homes and apartments. Unlike the lakefront areas north of

downtown, South Shore was bounded on the west by Chicago's expanding black ghetto. Unlike Hyde Park just to the north, which contains the University of Chicago, South Shore had no established, expanding institution to generate white residents, employees, and visitors.

In 1960, South Shore's population was 99 per cent white. It is now estimated to be 80 per cent black. As Molotch effectively shows, this rapid change was *not* the result of white flight. Instead, whites stopped moving into the area, and most of the market vacancies were filled by blacks. Since nearly 20 per cent of America's households move each year, the occupancy of a community can change quickly without any acceleration of the normal turnover rate. Renters tend to move more often than homeowners, and half the units in South Shore are in multifamily buildings; so occupancy has always changed in a high proportion of the community's housing units each year. The rate did not increase during the period of racial change.

To evaluate the comparative stability of the real estate market in South Shore in 1966, Molotch performed a careful analysis of single-family home transfers in Rogers Park and South Shore. The two communities are directly comparable, except that Rogers Park is on the north lakefront—at about the same distance from the central business district. Molotch found that, in comparison with Rogers Park, South Shore had no more home transfers, no more "for sale" signs, and no price decreases during the period of rapid racial transition. His conclusion after this analysis was, "South Shore is stable; there are no signs of flight by homeowners from the racially changing neighborhood" (p. 161).

Molotch's evidence was treated with skepticism by community residents, who perceived outmigration as being a major problem in South Shore. Presumably, this mistaken perception arose because the in-migrants were visibly different, and residents were made more conscious of migration patterns that were less obvious when movers-in were of the same general socioeconomic and racial mix as their predecessors.

Except for the western edge of the community, most of the black households entering South Shore were middle class. Their overall net worth was probably below the white households they replaced, and more of the couples were joint wage earners. However, the community concerns and desired neighborhood amenities of the blacks moving into South Shore were similar to those of the white resi-

dents. It is six or seven years since Molotch analyzed South Shore, and the population has become predominantly black. Now the community is fighting a new battle—to retain its middle-class character and to maintain the property values that prevailed at the time of racial transition. Although home prices and rentals did not initially decline (according to Molotch's evidence, they may even have increased), there have been secondary effects of racial transition. The business and financial communities have not written off South Shore, but neither have they continued the level of reinvestment that is usual for a middle-class white community. Thus, today's residents are anxiously working to keep the area a viable and healthy middle-class black area.

In the last 20 years, the focus of the South Shore Commission has changed substantially. At its inception and through its energetic period in the mid-sixties, the Commission, according to Molotch, "attempted to create neighborhood conditions it thought important to attract whites and to make these neighborhood assets generally known to prospective white residents. Secondly, an effort was made to directly facilitate white move-ins by recruiting white, middle-class residents through a tenant referral service and other means" (p. 83). The direct accomplishments of the Commission in this period include: establishment of good relations with political leaders in the City of Chicago, construction of a new South Shore High School, generation of community concern in local school administrators, creation of a central screening service for apartment applicants, execution of a stiff city building-code enforcement program, and publication of a monthly newsletter that was sent free to 25,000 families and businessmen in the community. These accomplishments did not stem racial change, which was a goal of the Commission. However, they certainly contributed to preservation of physical and social amenities in South Shore.

The South Shore Commission is still an active force in the community, but its goal is not so much to achieve integration as to prevent economic deterioration. The Commission managed to orchestrate purchase of the large South Shore National Bank to avoid the loss of the only neighborhood bank. It is now working on purchase of the failing South Shore Country Club by the Chicago Park District to expand the recreational and educational opportunities in the community. The Commission is now more representative than it was when Molotch was studying it, but its primary concern is still neighborhood preser-

vation. The emphasis is no longer preservation of a biracial community (Molotch demonstrates that South Shore was never really integrated), but preservation of a middle-class community.

Integration was probably a hopeless goal in South Shore, given the community's location on the edge of the black ghetto and the many housing options available to middle-class whites in the Chicago metropolitan area. Despite the apparent failure of the Commission's original intent, however, Molotch's analysis of the organization is valuable in terms of both achievements that are possible and limitations that are inherent in a citizens' action group.

M. LEANNE LACHMAN  
ANTHONY DOWNS

*Real Estate Research Corporation*

**Corporate Power and Social Change: The Politics of the Life Insurance Industry.** By Karen Orren. (Baltimore: The Johns Hopkins University Press, 1974. Pp. 204. \$10.00.)

Despite the efforts of governmental institutions at all levels, the problem of urban decay and minority advancement have stubbornly resisted solution. According to Professor Orren, the impact of government has been marginal because the causes of the problems are to be found in institutions and practices over which it has exercised little control. Since many of these mechanisms are the province of corporate business, Professor Orren's book sets out to investigate whether voluntary corporate action can constitute an effective instrument for social change.

While there is a wide range of corporate activities that might be subject to voluntary modification to bring about reform, this book focuses on the mechanism of credit. The life-insurance industry has been chosen as the focus of the analysis both because it controls vast amounts of investable funds and because its \$2 billion Urban Investment Program has been one of the most prominent examples of voluntary corporate social action.

Since the nature and extent of corporate voluntarism are critically dependent on the relationships between business and government, the book begins with an analysis of those relationships in the case of the life-insurance industry. Utilizing the state of Illinois as an example, the author documents the lobbying activities of the companies and indicates how they have attempted to protect themselves from incursions into their corporate autonomy. That discussion is followed by a chapter on "the life insurance enterprise," which analyzes

the characteristics of the life insurance business. The two subsequent chapters investigate life insurance company mortgage investment patterns in the Chicago area and consider the nature and results of the Urban Investment Program.

These latter two chapters constitute the primary contribution of the book. In the chapter on mortgage investments, 129 census tracts were examined over the thirty-four year period 1935 to 1968 to determine the incidence of life-insurance company mortgages in various parts of the city and its suburbs under changing socioeconomic conditions. This analysis provides significant evidence of institutional racism. Even when factors such as the income of the mortgagor and the age and value of the properties are taken into account, the data indicate that life insurers have consistently favored whites over blacks in their mortgage lending activities.

The discussion of the Urban Investment Program credits the companies for their efforts and acknowledges their sincerity. It also demonstrates, however, that the program must ultimately be regarded as a failure. Plagued by the lack of clearly defined goals and the absence of an authoritative coordinating body, the program led to disillusionment among the companies and resentment among blacks. One of the problems was the decision by the companies not to lend funds under the program at less than market rates. As the author points out, "while neighborhoods may be built, maintained, and even allowed to decay at market rates, it takes something more—or, more precisely, something less in the way of private profits—to put them back together again" (p. 160). Thus, instead of providing a real impetus for the solution of the urban crisis, the companies "channeled funds straight to the most profitable or peripheral segments of the ghetto community, most of which were already served adequately in financial markets" (p. 183). These considerations lead the author to suggest that the purely voluntary approach is not the final answer to the problems of the cities and that a greater degree of governmental coercion will probably be necessary in order to harness the resources of the private sector for the public good.

Unfortunately, the author's accomplishments in the chapters on mortgage investments and the Urban Investment Program are not matched in the chapters on life insurance politics and the nature of the life insurance business. While the chapters on politics provide some revealing glimpses into life-insurance industry lobbying activities, they contain several highly questionable theses. For instance, one

of the primary contentions is that "a particular mode of legislation—the mandatory investment law—has largely determined the companies' preference for state or federal regulations" (pp. 34–35). To demonstrate this the author indulges in proof by omission, i.e., she shows that there has been concern among companies over mandatory investment laws but virtually ignores other aspects of regulation that may have influenced company opinion. In fact, most observers would argue that mandatory investment laws have constituted little more than a peripheral issue in the broad scheme of regulatory conflict.

Another dubious conclusion is that in insurance regulation "safety is not a policy but rather a pseudopolicy . . . devoid of clear procedural or substantive comment" (p. 67). Here again the author has drawn a sweeping generalization based on only partial evidence. While one can raise legitimate questions with regard to the success or indeed the fundamental design of existing mechanisms for solvency regulation, there can be little question that safety has long been the overriding concern of insurance regulatory law and practice.

Unlike the chapters on life insurance politics, which are partially redeemed by a number of insightful comments, the chapter on the life insurance enterprise has little to recommend it. That discussion is characterized not only by half-truths and outright errors but by misinterpretations of those facts which have been recorded correctly. The problem here as in the regulatory chapters seems to be that the author has overlooked several of the most significant prior scholarly treatments of the subject matter.

As serious as the defects of the regulatory and business enterprise chapters are, however, they do not detract from the value of the chapters on the racial implications of life-insurance company mortgage investments and on the Urban Investment Program. The book is clearly worth reading for those two chapters alone. On the other hand, for the reasons stated above, the chapters on life insurance politics should be read selectively; and the reader might be well-advised to avoid altogether the chapter on the life insurance enterprise.

J. DAVID CUMMINS

*Wharton School, University of Pennsylvania*

#### **The Future of the United States Government.**

Edited by Harvey S. Perloff. (New York: George Braziller, 1971. Pp. xxiii, 338. \$7.95.)

This book is a product of a working group of the Commission on the year 2000 of the

American Academy of Arts and Sciences. Nineteen papers are presented in the volume as well as six transcripts from the working group's discussions. The essays contained in this work are essentially speculative, predictive, and prescriptive in character and no attempt is made to separate fact and value in any definitive way. The volume is given balance by including papers contributed by individuals with business and governmental experience as well as by scholars from several disciplines.

Professor Perloff, editor and contributor, in an excellent introductory essay provides the framework for the working group's essays and discussions. According to Perloff, key problem areas of the future include pressures on the individual, the danger of deterioration of the internal cohesion of society, deterioration of the environment and mass politics, and the domestic strains of world responsibility. Fundamental to any effort to solve these and related problems, in his view, is a basic alteration in our public philosophy. Society must first broaden its conception of civil rights to include for providing each individual the opportunity to develop his potential to its fullest. A new public philosophy must also provide for more freedom for individuals and groups. This might take the form of communities within communities maintaining their own "life-style" groups through self-discipline. Coupled with this new freedom is the need for a better-defined and stronger sense of social obligation among individuals in society. Our foreign policy, he believes, must also be restructured toward a more pervasive concern for human rights and dignity. Finally, the United States should become more aggressive in promoting world economic integration since a well integrated economic community will be less prone to political conflict.

Taking this framework as their point of departure, most contributors focus on how the American political system can be reorganized and restructured to maximize such values as full opportunity and greater personal freedom. Among the many prescriptions for change suggested in the volume, several emerge as overwhelmingly important. American political institutions, it is argued, must become more sensitive to individual needs and more opportunity for innovation must be created at all levels of government. Addressing himself to this issue in one of the volume's more interesting essays Donald A. Schon argues for the development of "pools of competence" based on skills of competence (e.g. statistics and computer programming) and process (e.g. planning and program management) to

deal with problems which do not fall within the boundaries of established agency structures. Several of the essays project that by the year 2000 distinctions between public and private organizations will become increasingly blurred and private organizations will preform more activities which have traditionally been considered public in nature. Government, according to an essay by Robert A. Nathan, will by the year 2000 be primarily involved in setting broad standards and policies aimed at achieving self-regulation in the private sector while the private sector will be the principal agent for economic and welfare activities. Many of the essays also stress the point that in future decades political participation will need to be broadened to include individuals and groups such as the poor, the aged, women, and the socially disadvantaged who are now only marginally politically active. Also, many contributors believe that state governments now in the process of atrophying, should become more rigorous, while local governments should decrease in number, consolidate many of their functions and become somewhat more autonomous.

Although there are many positive aspects to the individual papers, the volume taken as a whole has certain problems. Perhaps most significant is the fact that the book's scope is too broad. It is difficult to conceive of a volume dealing with the American presidency in the year 2000, let alone the entire structure and process of the American political system. As a result, some of the subject areas are treated in a thin and superficial fashion. A second problem involves the tacit assumption by the working group that with certain minor fluctuations the American economy will remain healthy, and real economic growth will continue unabated. Clearly in an era of double-digit inflation and economic slow down this assumption with all its implications can be severely questioned.

Viewed as a ground breaking effort, however, the working group's essays are food for thought. Political scientists in particular and social scientists in general who are interested in the area of political and social change will find them worth lingering over.

GRANVILLE J. FOSTER

*Columbus College*

**Old-Age Politics in California: From Richardson to Reagan.** By Jackson K. Putnam. (Stanford, Calif.: Stanford University Press, 1970. Pp. 221. \$7.50.)

This brief but well-documented historical study recounts the growth of welfare legislation for the aged in California as well the

often emotional politics, both conventional and unconventional, accompanying that growth. Only the Depression period, from Upton Sinclair's EPIC (End Poverty in California) campaign in 1933 to the demise of the Ham and Eggs movement in 1940, is covered in detail. The fifty years preceding and the thirty years following that period are treated summarily in two chapters. The most original contribution of the monograph is the description of the Ham and Eggs movement, which has never before been studied systematically.

Although claiming the historian's right to be conceptually eclectic, Professor Putnam attempts to place California's old-age politics in the context of some social theory or theories. How does one explain the at times bizarre old-age movements with their mixture of religious revivalism and crass commercial promotion tactics? How could some of these movements come so close to taking over the welfare machinery of the state? Why did they leave such telling marks on California politics? More generally, what accounts for the saliency of the old-age problems in the political life of California? Putnam argues that the purported high concentration of old people in California (note that, since 1960, California's proportion of persons over sixty-five years of age is below the national average) constitutes at best a very partial explanation. As for the notion that the presumably blocked activity needs of the aged come to be expressed in political participation—this explanation seems, in the absence of direct evidence, at least highly doubtful. Putnam applies similar strictures to explanations based on personality variables or feelings of isolation and estrangement from society. Finally it is unclear, as the author points out, why the disengagement hypothesis—the need of the aged to reduce their contacts with the social world—should ever have been considered in connection with increased political participation.

Elsewhere, Putnam shows that conventional politics—constituency and interest-group pressures in the legislative process—have yielded more benefits to California's aged than have the various special interest movements, from the Townsend plan to the California League of Senior Citizens of George McLain and Myrtle Williams. Thus, certain legislators, such as San Francisco assemblyman William B. Hornblower in the 'thirties, have consistently drafted, and marshaled the necessary political support for, bills liberalizing old-age grants and programs. Putnam states repeatedly that during the period he considers, a politician's

stand on old-age benefits could make or break him, since a substantial number of electoral constituencies favored higher grants and more liberal eligibility provisions. He offers no explanation for this peculiar state of affairs. The answer may well be that the process of age segregation was particularly rapid and severe in California, and that this process created numerous constituencies with high concentrations either of impoverished old people or of younger couples concerned about the economic situation of their distant parents. This hypothesis, if true, might also help to explain why old-age issues should be so much more prominent on the political landscape of California than on that of other states—a problem with Putnam entirely fails to confront.

If the aged have had some political success in California—so Putnam concludes—it is because they were able to form an effective interest group. But whether the activism of old people has actually contributed much to their cause remains open to question. California's old-age movements, at any rate, have been typically neither initiated nor run by old people. They have typically taken the form of proprietary organizations operated by their founders ("younger" people for the most part) with the help of organizers or speakers working for a share of the "take" on collection plates or in membership fees. In addition, plans and solutions advocated by these organizations, whether ridiculous or sublime, have tended to be utterly impractical. The opprobrious term "pension promoter," so common in the political vocabulary of California, was therefore not entirely unjustified. Putnam correctly points out that pension organizations have become more and more responsible; indeed, the lobbying efforts and pensioner service activities of the League of Senior Citizens have been competently organized and carried out. Still, to the extent that pension organizations have given up their millenarian appeals, they have lost the mass followings they once enjoyed.

One can only conclude that it is not the activity of pensioners in their own interest that accounts for their modest success. Rather, the problems of the aged were placed high up on the political agenda and remained there because of the clear definition of the political constituencies that are sensitive to them. The point is worth making because it may apply, in varying degrees, to other diffuse interests, such as those of battered children or consumers.

FRANK A. PINNER

*Michigan State University*

**State Legislative Innovation: Case Studies of Washington, Ohio, Florida, Illinois, Wisconsin, and California.** Edited by James A. Robinson. (New York: Praeger Publishers, 1973. Pp. xvii, 281. \$17.50.)

This anthology brings together under a single cover diverse reports having to do, not with innovation, but with fairly mundane legislative reforms and other changes. All of the changes involved are well-worn ones, and the essays in this book are analyses of legislative changes in selected states. Douglas S. Gatlin writes about changes in the legislative party system in Florida, focusing on the 1967 session. Alan J. Wyner deals with changes in the 1960s in the California legislature, and especially with reforms associated with the leadership of Jesse Unruh. Samuel K. Gove and James J. Best report analyses of the effects on public policy of legislative reorganization and reapportionment in Illinois and Washington. Alan Rosenthal analyzes the effects of increased professional staffing upon state legislative performance in Wisconsin. Finally, Thomas A. Flinn considers the extent to which the Ohio legislature has become a "professional" legislature. Quite apart from the fact that they are not integrated in any way, and no conceptual or theoretical synthesis is attempted, these reports do provide valuable and useful material on individual state legislatures.

The following seem to be the main findings which can be drawn from these reports of research in individual states:

(1) Change from a single-party legislature to a two-party legislature, as in Florida, requires more than simply the election of a critical mass of opposition party members. Two cohesive legislative parties can develop fully only if (a) party leaders adopt and assert clear and distinctive policy positions, and take on conflictful party role behaviors, (b) legislators strongly identify with their party, (c) rewards and sanctions are distributed in accord with the party's policy positions, and (d) party caucuses and leaderships reinforce a consistent party program.

(2) Changing a legislature in the manner in which the California legislature was "reformed" in the 1960s—providing a substantial staff, having annual sessions, streamlining committees, strengthening party leadership, raising legislators' salaries, providing adequate facilities—may result in (a) only modest relative increases in costs for the legislative institution, (b) some increase and better distribution of the workload of bills, and reduction of the end-of-session "logjam," (c) a legislature more independent of the executive and private

interest groups, (d) lower turnover of members, (e) the effective end of the part-time, "citizen" legislator, and (f) no clear, substantive policy effects attributable to the reforms.

(3) A legislature resistant to internal reorganization in the direction of greater "professionalization," as in Illinois, is one in which marginal procedural changes are especially unlikely to have discernible policy impacts.

(4) When a state has been egregiously malapportioned, as Washington was prior to 1966, reapportionment may have a policy impact in improving the chances for enacting urban-oriented legislation. But, in enlarging the representation mainly of suburban areas in northern urban states, reapportionment may have as one of its main effects a considerable, and perhaps appropriate, enhancement of Republican membership in the legislature; and the landslide 1964 presidential election obscures the pure effects of reapportionment to a substantial but unknown extent.

(5) The expected unintended or latent consequences of substantially increasing the party and committee staffs of a state legislature like that of Wisconsin—increased workload and increased partisan conflict—can, indeed, be shown to develop, so that increased professional staffing may result mainly in making the legislature more partisan, particularly by enhancing the capability of the minority party to oppose the majority.

(6) The "professionalization" of a legislature like the one in Ohio in terms of the standard reforms of the formal structure may not have associated with it some of the informal changes which might be anticipated by virtue of the construct "professionalized legislature." For instance, the Ohio legislature clearly is more professionalized than it used to be, yet turnover of membership remains high, and the level of aggregate positive attitudes toward specialization among legislators has not increased. There is some evidence that the occupational status of legislators, legislative role orientations, distance between legislators and interest groups, legislators' willingness to support governmental activity, and legislators' feelings of competence covary in the expected directions with changes in legislative structure toward a more professional legislature.

Some of these findings are more interesting than others, but what they all do is to reinforce the urgency of conceptual work on processes of institutionalization and other organizational changes. In perhaps rudimentary ways, the research reported in this collection focuses upon institutions changing over time, a

much-needed perspective for political analysis. But the *modus vivendi* for each of these projects is largely conventional, and the analytical results are sometimes tedious and always conjectural. Without some theory about changes in institutions over time, a deficiency of the discipline more than an inadequacy of these investigators, it is difficult to give larger meaning to the research presented here. If, as the editor of the anthology suggests, an important yield of this research is the rediscovery that "the more things change, the more they stay the same," we shall not have gained much. If, however, these studies, ingenious and insightful in many respects, contribute to a renewal of interest in constructing empirical theory of institutions and organizational change, then they will have made something of a mark.

SAMUEL C. PATTERSON

University of Iowa

**Position Classification: A Behavioral Analysis for the Public Service.** By Jay M. Shafritz. (New York: Praeger Publishers, 1973. Pp. 133. \$12.50.)

Position classification in government is beloved by few, denounced by many, but necessary to most. In this brief new volume, Professor Shafritz adds his whip to the scourging party that has gone on ever since systematic job evaluation was first tried in government. His message is mainly that classification is one feature of bureaucratic management that prevents optimum relationships among employees and optimum development of the individual. This is a good point, but Shafritz pounds it into the ground with repetition and (for my taste) overdocumentation. This scholarly overkill is not all bad. His footnotes and bibliography are well-selected guides to the literature of organization theory as well as that of public personnel administration.

After briefly stating his hypotheses and outlining the history of classification, the author moves to the "inherent dysfunctions"—how flexible people are poorly served by the job-based inflexibilities of the system. Management's ability to use pay as a reward for good work performance is inhibited by the difficulty of reclassifying jobs. Coming to organization theory, he shows how position classification supports hierarchical organization which suppresses conflict, discourages vertical communication, and generates anomie.

The theoretical discussion is then interrupted by a chapter on "Philadelphia as a Case Study." The pressures and manipulations required to get a raise for somebody in the em-

ploy of that city are outlined in some detail to show that the classification audit system is bent to get raises for those favored in the organization's internal political system. This chapter demonstrates how the classification process in that government fails three tests of a formal structure suggested by the writings of Herbert Simon: (1) encouraging constructive formation of informal organizational relationships; (2) setting limits on the development of informal relations; and (3) preventing the development of deleterious organizational politics. This chapter is not needed by readers who have been personally exposed to job evaluation processes within governmental bureaucracies, but it is a useful dip into realism for those without such experience.

Returning to organization theory in the sixth chapter, Shafritz shows how classification doctrine and process impede the type of people-centered administration urged by Frederick Herzberg and Douglas McGregor.

The work concludes with a chapter called "Position Classification in the Postbureaucratic Era." How's that? If we ever have such an era, it's not likely to feature classification. Yet Shafritz's solution is to urge that classification be put in the hands of program managers as a tool of administration and that the personnel or civil service staff serve as consultants in its use.

This book is useful as far as it goes—mainly in showing how classification can get in the way of effective management. The author fails, however, to carry far enough his discussion of modern organization theory. This would bring him inevitably, I think, to the conclusion that people-centered, task-oriented, highly flexible management will require a greatly "weakened" job evaluation system. Such a system would feature fewer and broader classes, fewer barriers to movement between classes, and lessened dependence of pay upon job ratings.

My final criticism, I admit, puts me in the classification (that word again!) of reviewers who say "this is not the book he should have written"—but only to the following extent: Shafritz (except for a few words early in the book) does not face the implications of the developing power of public employee unionism on pay practices and levels, including classification, as a partial determinant of pay. Unions have seen to it that blind alley jobs are opened up, some lower jobs upgraded, and new jobs created—all through bargaining and other pressures. Yet Shafritz's discussion implies that management can unilaterally decide what the nature of the classification process will be. Moreover the treatment of organiza-



tion theory ignores the management-union adversary relationship and the growing influence of unions on assignments, promotions, and employee discipline. Professor Shafritz needs to prepare a sequel to deal with these matters.

DAVID T. STANLEY

*The Brookings Institution*

**Seeds of Extinction: Jeffersonian Philanthropy and the American Indian.** By Bernard W. Sheehan. (Chapel Hill: The University of North Carolina Press, 1973. Pp. 301. \$11.95.)

Most writing on the American Indians has been based on two sources. The earlier and more pervasive is the information arising through contacts by explorers, traders, travelers, artists, soldiers, missionaries, government agents, and residents of the frontier. The other is the analysis of Indian cultures that anthropologists derive from the evidence provided by Indian informants, artifacts, and archeological remains. Most of our understanding of Indian-white relationship will continue to rest on these two bases.

In *Seeds of Extinction* Professor Bernard W. Sheehan turns to the American leaders in the first decades of the Republic and asks how they perceived the Indian? Clearly these perceptions were not based on intimate contact socially or in business or politics. Two centuries after Jamestown, the Indian frontier had receded from the Atlantic seaboard, and most Americans knew Indians only indirectly.

One method of concept-building about these original Americans was philosophical, and that is what this book pursues. It explores American thought on the origin of man and the first populating of this hemisphere. It asks what effects are attributable to environment. Buffon and others, as Sheehan indicates, posited a deficiency in America which consigned the Indians to "hopeless inferiority," an allegation which Jefferson was at pains to rebut.

From theory the debate moved to the policy options that white America might elect. As here denominated, these were incorporation; manipulation, violence, and disintegration. Incorporation is the code word for assimilation, civilizing, acculturation—the process, sometimes prolonged and painful, by which Europeans became Americans. Manipulation included such options as intimidation, coercion, adding to "civilized goods," and intermarriage. Intermarriage availed less than might have been expected in "Americanizing" the Indians, because it usually linked a white man and an Indian woman and that usually meant

that the children would be brought up in the Indian culture. Incorporation and manipulation thus were nonproductive.

White-Indian relations were marred by recurrent atrocities in both directions, atrocities which exerted an extraordinary fascination on those who wrote on the Indians and through these writings on the American philosophers. Aware also of the Indians' greater susceptibility to diseases brought by the whites and allegedly to alcohol, the philosophers assumed that the Indians were doomed to disintegration if not extinction. Given that frame of reference, even well-meaning friends of the Indian could turn to segregation (removal was their word) whereby the remnant of the eastern Indians could find refuge beyond the settlements where the Great American Desert began and there continue their distinctive way of life.

As in the literature on which it is based, *Seeds of Extinction*, tends to stereotype the Indian more than is accurate, especially Indian culture. The book serves a useful function in analyzing higher, abstract, almost esoteric thinking on the Indians and spelling out the rationalization for removal. By the early nineteenth century mundane forces had already substantially dispossessed the eastern Indians and now pressed for removal.

JOHN CAUGHEY

*University of California at Los Angeles*

**An End to Hierarchy! An End to Competition! Organizing the Politics and Economics of Survival.** By Frederick Thayer. (New York: New Viewpoints, 1973. Pp. 232. \$9.95, cloth; \$2.95, paper.)

The business corporation is re-emerging as a legitimate focus of political analysis. Political scientists, who seem to have an uncanny instinct for identifying important political issues after they have become public, are showing renewed interest in the principles which govern the production and distribution of "private" goods and services. We are beginning to shed the illusion that these principles can be studied in isolation from those which inform the allocation of power and privilege in what we still misleadingly label "the political system." Professor Thayer's book contributes to the contemporary American rediscovery, pioneered largely by radical economists, of the once respectable tradition of political economy. Its thesis is the interdependence of the two central principles around which contemporary capitalist democracies organize their governmental and corporate systems, namely competition and hierarchy. These principles

are considered obsolete and pernicious: competition, whether conducted by firms or political parties, invariably reduces participation and creates authoritarian institutions, and hierarchial structures, in turn, undermine individual autonomy and foster alienation. The author proposes an alternative organizing principle: structured nonhierarchial social interaction. Its unfelicitous terminology should not distort its simple message: all decisions in society should be made cooperatively, and no one should be in a position to dominate anyone else.

Thayer judges a variety of ideologies and political theorists according to this rather demanding standard: few escape criticism. In barely two hundred pages, we are exposed to the inadequacies not only of liberal democracy, socialism, and capitalism but also of many of the writers who have sought to explicate some of the complexities of these systems. Thayer charges theorists as diverse as Dahl, Schumpeter, Lowi, Wolin, and Marx with still living under the shadow of Thomas Hobbes and Frederick Taylor; they are each accused of denying some aspect of human autonomy. Dahl, for example, is criticized for restricting his advocacy of worker participation to the corporate sector, while Marx's conception of the revolutionary class is attacked because it implies the potential domination of one group by another.

Thayer clearly speaks to a sense of uneasiness about the performance of the institutions of corporate capitalism and liberal democracy which is shared across a wide political and cultural spectrum. The importance he attaches to increased participation reflects the contemporary concern with the relationship between alienation and participation expressed by such recent developments as the reform efforts of the Democratic Party and the interest of corporations in the restructuring of work. Moreover, Thayer has identified an important difficulty for those seeking to democratize the workplace: competition, even in the context of public ownership, *does* invariably produce inequalitarian relationships in the firm. Efforts to change the governance of the corporation, whether through changes in the method of selection of the board of directors (as Ralph Nader and Campaign GM propose) or via worker self-management (as suggested by G. David Garson and Robert Dahl) will have only a minimal impact on business policies as long as the hegemony of the market remains unchallenged. If enterprises are forced to compete for labor and capital, serious constraints are placed on their ability to transform themselves into republican institutions. Corporate

reformers will ultimately have to confront the paradox of a publicly accountable firm in a competitive market system.

Ultimately, however, Thayer reveals more about the inadequacies of much of what passes for social criticism in the United States than he does about the evils of hierarchy and competition. As our political and economic problems become more unmanageable, the urge to find a single explanation to all our difficulties becomes more difficult to resist. This impulse must be especially appealing when the magic formula can be condensed into the title of a book. Like the devotees of the numerous religious cults who are currently making the rounds of our campuses, Thayer contends that he has discovered the key to our salvation; his obsession with the evils of hierarchy and competition dominates the book like a religious chant. No sense of history nor of the limits of human perfectability are allowed to confuse Thayer's argument; the conventional wisdom of politics and economics is dismissed as irrelevant to our future.

Thayer's criticisms of economic and political competition, while not particularly original, are often well argued; what undermines them is their author's disinterest in politics. For all its merciless criticism, *An End to Hierarchy! An End to Competition!* is fundamentally an apolitical book. Those who think that systemic social change entails conflict and discipline—and even disappointment, will find Thayer's observation that, "everything argued herein either exists already or can easily be accomplished" (p. x) reassuring. The possibility of social transformation without political struggle is a recurrent American fantasy; in this Thayer owes as much to Thoreau and Charles Reich as he does to Adam Smith and Ricardo. In his book, Thayer, like many of the more fashionable environmentalists, is reluctant to confront the radical political implications of his analysis.

His refusal to identify the principles of hierarchy and competition with real political interests—an identification which might have made the book a useful one—leads to some confusing observations. Thus Thayer gives McDonalds' Corporation considerable praise for providing its executives with a room equipped with a waterbed, psychedelic lights and stereophonic music at the corporation's headquarters. This room purportedly symbolizes "an emerging organizational revolution" (p. 3) that will transform the American system. To equate this admittedly unusual and imaginative innovation with an increase in worker autonomy and participation

is to confuse the style of decision making with its substance. Corporations are increasingly encouraging small-group cooperation and more informal authority relationships at the management level. Yet, it is precisely the "hip" and creative atmosphere at McDonalds' executive headquarters which is responsible for the firm's spectacular success in translating the monotony and boredom of assembly-line production to retail dining. The "unalienated" labor of McDonalds' executives is directly linked to the alienation of the corporation's unskilled employees. That the corporation responsible for successfully marketing Chaplin's automatic eating machine may serve as a model for our future should cause concern, not celebration. Prior to ending competition and hierarchy, we must first move beyond sophomoric analyses of politics and economics.

DAVID VOGEL

*University of California, Berkeley*

**The Politics of Lying: Government Deception, Secrecy, and Power.** By David Wise. (New York: Random House, 1973. Pp. 415. \$8.95.)

Deception and politics have always been closely linked in the minds of Americans, and the polls suggest that many people find it hard to tell them apart. Thus, the evidence presented in this book on lying by government officials and organizations will hardly surprise a large part of the public for which it is written. Americans commonly assume that a politician will tell a few small lies and perhaps an occasional big one—as, for example, that he is competent to hold the office he seeks.

As part of their political education Americans may well develop a capacity to discount lying when it takes the form of exaggeration or distortion in party combat. It is hard to remember a recent presidential campaign that has not featured something resembling a "big lie" at the center of what is euphemistically called the political dialogue in the United States. Recall, for example, Kennedy's charge of the "missile gap" in the 1960 campaign or Nixon's promises of "law and order" in his pre-Watergate salad days.

Presumably, the fact that Americans are constantly exposed through advertising to inflated claims for the goods and services they purchase in the market-place helps build some immunity against analogous deception in party politics—not to mention a certain amount of tolerance for the deceit to which they are subjected by politicians.

The question that haunts this book is how

to determine when lying is lethal for the norms and institutions of democracy. It is never directly addressed by the author, David Wise, a Washington journalist who has written extensively on the secrecy apparatus in the United States. At times Wise seems to argue that all lies by political leaders should arouse the same measure of moral indignation. He cites, for example, Lyndon Johnson's spurious claim that his grandfather fought at the Alamo as a prime illustration of the fraud American politicians habitually practice on their constituents. It is, however, possible to take the Johnson speech much less seriously—to look upon it as the kind of harmless boast about the achievements of ancestors in the settlement of the United States in which a great many Americans indulge. Presumably it helps relieve feelings of personal insecurity, though it never seemed to do very much for Johnson in this respect.

Surely, lying of this sort is not on a par with the gross deceptions of modern American politics—those for which Vietnam and Watergate have now become code words. The problem we confront is the task of telling when lying has serious effects, so that we can draw a line between practices of deceit that are consequential for the health of a democratic society and those that are not. If we look to Mr. Wise for help in making distinctions of this sort, we look in vain.

What sorts of distinction might be drawn? One could, for example, argue that the setting in which lying occurs is critical. Surely, lies told in a stump speech on the campaign trail are far less consequential than lies included in an official report by a government agency or a formal address by a President. On the campaign trail some kind of adversary process prevails, and no particular authority is attached to the statements of rival politicians seeking office. An official report, on the other hand, is often based on data solely in the government's possession and carries with it an air of special legitimacy to which a citizen might ordinarily be expected to defer. However, the neatness of this distinction is impaired by the obvious danger of allowing political candidates unlimited latitude to disseminate falsehood about their opponents.

Nor does the author indicate, alternatively, that democratic regimes have sometimes distinguished between lying to foreign adversaries, which is generally felt to be acceptable, and lying to their own citizens, which is not. This distinction—based on the nature of the audience to which lies are directed—has collapsed in modern times, as democratic gov-

ernments have fallen into the vice of pretending to lie to enemies abroad in order to lie to their own people.

But while Wise does not provide very much help in developing what might be called a general theory of lying, the book does open up some new vistas on government information practices. As organized, the book reveals in considerable detail how practices of secrecy and publicity can be linked together by government officials in a frequently successful effort to manufacture the consent needed to legitimize their power. The first section of the book concentrates on secrecy, and the latter half focuses on techniques of news management. If a major task of the journalist is—as Mencken once put it—to serve as a scout for scholars, then Wise performs his role admirably.

He shows, for example, that the destruction rather than the withholding of data is now the most serious form of information pathology. As a result, the shredder has replaced the classification stamp as the chief instrument and symbol of government concealment. Technology thus keeps pace with the need not only to conceal but also to obliterate records and documents. In 1966, when the Defense Department undertook a review of its classified files, it declassified 710 and destroyed 355,300 documents—a step that some officials must have surely regarded as the “final solution” of the information problems.

Equally interesting—in light of the recent Watergate revelations—is Wise’s demonstration that in spite of all the classification procedures available to them, modern presidents still find it necessary to operate a special secrecy system of their own. The growth of secrecy in government has been widely regarded as a product in large measure of bureaucratization. Wise suggests a quite different possibility—that the increasing obsession of the White House with concealment has made bureaucracy the enemy as well as the ally of executive secrecy. In pursuing its private organizational interests by leaking information on presidential plans it dislikes, an executive agency opens the affairs of government to much greater public scrutiny than they would otherwise receive.

Finally, Wise shows very clearly that high government officials often come to regard information they withhold from the public as a form of private property—to be used at their personal discretion and for their own monetary enrichment when they eventually come to publish an account of their career in government. In this way members of the executive

elite can derive wealth as well as power from their privileged access to information. “Honest graft” has come a very long way from Plunkitt’s day.

If Wise’s book does not wrestle successfully with some of the major issues posed by the mendacity of modern democratic government, it excels in the clarity with which it demonstrates how officials can derive both personal and political benefit from their ability to control the flow of information to the public.

FRANCIS E. ROURKE

*The Johns Hopkins University*

**Latin American Political Parties.** By Robert J. Alexander. (New York: Praeger Publishers, 1973. Pp. xxv, 537, \$28.50.)

Political scientists familiar with Professor Alexander’s work will find this volume, his fourteenth in little more than two decades, his most ambitious effort to date. Aware of Alexander’s extraordinary enthusiasm, Frederick Praeger suggested that the author’s relatively modest proposal to study the region’s radical political parties (“Jacobin Leftists”) might be expanded into a descriptive guide to virtually every major Latin American political party, past and present. Alexander obliged: From the *blancos* in Uruguay to the *colorados* in Paraguay, from Chile’s Traditional Conservatives to Bolivia’s Left Revolutionaries, from the torrid *Movimiento 26 de Julio* to the temperate *febristas*, he discusses three *alianzas*, four *falanges*, thirteen *frentes*, thirty *movimientos*, and no fewer than 249 *partidos*. This is a reference volume—a “discussion of all the important parties of recent decades, as well as the outstanding ones of the period since independence” (p. vii).

Following a brief introduction to the role of parties in Latin American politics, the volume is divided into six parts: personalist and traditional parties (personalist parties, Liberals, Conservatives, and Radicals), the Latin American socialists (in Argentina, Chile, Uruguay and, briefly, in nine other nations plus Puerto Rico), the national revolutionary parties (Peru’s APRA, Venezuela’s *Acción Democrática*, Costa Rica’s *Liberación Nacional*, Bolivia’s MNR, Puerto Rico’s PPD, Mexico’s PRI, Cuba’s *auténticos* and *ortodoxos*, and the Dominican Republic’s PRD), the Christian Democratic parties (in Chile, Venezuela, and sixteen other nations), totalitarian parties (orthodox Communist parties, schismatic Communist—Trotskyite—parties, the Jacobin left, Castro communism, and fascist parties), and miscellaneous parties (nearly all of the Brazilian parties and the Peronist party and

movement). There is no concluding statement.

Alexander's approach is descriptive. He relies heavily upon current periodicals—particularly the *New York Times* and the *Hispanic American Report*—and upon an enviable collection of personal interviews with party leaders. Anyone who has attempted the frustrating task of elite interviewing will marvel at the wealth of data the author has accumulated from talking to party officials during the past thirty years. In no other single volume would we find the responses of Presidents Betancourt, Caldera, Perón, and Prío Socarrás alongside the 1947 opinions of Cleofe Tupac Yupanque de Sáenz, Head of Women and Children's Section of the Secretariat of Indian and Peasant Affairs of the *Partido Aprista Peruano*.

As a compendium of party chronologies, however, Alexander's effort fails to take advantage of the enormous expansion of data on Latin American political parties during the past two decades. It is therefore probable that many students of a particular Latin American party or party system will view Alexander's commentary contentiously, just as I, for one, find criticism of the treatment of Peronism difficult to resist. Although the author warns us in his preface that he is "most disposed toward parties of democratic reform" while studying an area where both Western-style pluralist democracy and Hirschmanian reform-mongering are in increasingly short supply, he also insists that he has "tried to be as objective as possible in dealing with all kinds of parties" (p. vii). Yet the chapter on Peronism reveals clearly that his earlier (1951) appraisal of Perón as a self-serving demagogue successfully hoodwinking millions of unwitting workers has not been altered. This may be due to Alexander's somewhat immodest belief that his earlier effort remains "perhaps the best description of [the] early period of Peronismo" (p. 503), or because the only other sources he consulted were María Flores' 1952 anti-Evita polemic, George Blanksten's competent but dated (1953) analysis of *justicialismo*, a labor union propaganda sheet, an interview with a neo-Peronist labor leader, and the *Hispanic American Report*. Have we learned *nothing* from Smith, Kirkpatrick, Kenworthy, Snow, Díaz Alejandro, Germani, Luna, and a whole generation of younger Argentine scholars led by O'Donnell, Ciria, Cantón, and Catterberg?

In addition to this disconcerting attempt to impose the author's ideological preferences upon an analysis of Argentine political culture and the disregard of two decades of scholarship, a number of factual errors—Frondizi's

"impressive majority" (p. 475) in 1958 was 43 per cent of the popular vote; it was Law 13,010 of September, 1947, and not the Peronist Constitution of 1949 which gave Argentine women the same political rights and obligations afforded men; Perón's first vice-president was named Juan Hortensio, not Horacio; Peronists never celebrate June 4th as a party holiday—further detract from the value of his discussion.

Perhaps these are inevitable criticisms of one-person studies of any major Latin American political phenomenon undertaken without the assistance of some unifying theoretical perspective. Moreover, today it is all but impossible to accumulate sufficient expertise to present accurately even a journalistic appraisal of more than 300 political organizations in twenty-one nations. Having attempted just such a task, Professor Alexander must now welcome the opportunity to return to less ambitious investigations and to build upon his valuable studies of Latin American communism, organized labor, and the political systems of Argentina, Bolivia, and Venezuela.

LARS SCHOULTZ

*Miami University*

**The European Administrative Elite.** By John A. Armstrong. (Princeton, N.J.: Princeton University Press, 1973. Pp. xii, 406. \$20.00, cloth; \$9.75, paper.)

The canons of book reviewing in scholarly journals is as stylized in the 20th century as was the ceremonial at the court of the Sun King in its own days. Ritual requires that each review consist of three parts: an opening section stating the nature of the volume and praising it; a second section, usually beginning with a phrase such as "However, there are serious weaknesses . . ."; and a third and final section introduced with a "Nevertheless . . ." phrase. Musically inclined members of the profession will, of course, immediately recognize this as the classical a-b-a sonata form: statement of the theme(s); development section; and restatement plus coda.

Most books fit this format comfortably: there is always something to praise and something to criticize. Occasionally, however, a book comes along which does not permit recourse to the comfortable, albeit by now rather tired, clichés. Professor Armstrong's study of European administrative elites is just such a book. It is exciting, eminently learned, theoretically sophisticated, greatly successful in its historical sweep, yet based on a wealth of sources, traditionally historical as well as quantitative. It is all of this and more; yet it is

also opaque in its overall structure; determined to carry an interpretation beyond what the data and alternative interpretations will bear; ending on a note which doubly confounds everything the author has labored to demonstrate in over three hundred pages; and committing itself to a future built on ever more centralized planning and control in the hands of an administrative elite whose qualities of responsibility and responsiveness the author considered not sufficiently significant to explore, except tangentially, an administrative elite whose nearly total history since the seventeenth century, he claims, has been one of shaping affairs to fit its group interests.

In short, it is a book that tries the soul of the reviewer, that challenges him on nearly every page. Yet it is clearly a study that will be required reading for several social sciences and certainly for political scientists devoted to comparative politics and administration, as well as those who try to understand processes of development and modernization. They will want to read it, argue with it, and surely assign it to their students, if only to watch them struggle and agonize in their turn.

The study's central focus, according to its author, is the relation of administrative roles to economic development, or put differently, the role of administrative elites as development interventionists. Its geographic reach is the administrative elites of France, Britain, Russia, and Prussia (adding the German Federal Republic as the contemporary successors of the Prussian administration.) Temporally speaking, the study goes back to the pre-industrial period of these societies, though serious investigation begins with take-off, the author utilizing Rostow's "stages" as the basis for his periodization. However, there is no forward time limit, with contemporary data reaching into the 1960s being utilized and contemporary trends being examined. It is here that a rather serious problem arises. A political science reviewer is most likely to be able to deal with contemporary, or at least post-World War I data with some authority. The present reviewer is no exception, and so he must report considerable uneasiness with the treatment of contemporary developments, especially in Britain, France, and the Federal Republic, which rest on a data base much less solid than the earlier historic periods. To take only one example: Professor Armstrong adjudges the present German elite to be still predominantly legally trained and the dominance of the *Juristen* unbroken (p. 203.) The work was researched in the middle and late 1960s and was written in 1972. I believe that not even

then—and certainly not today—could one say that this legal dominance is unbroken. Again, the author concludes that the West German administrative elite does not occupy a central position in the societal elite as once did their Prussian forerunners (p. 213.) After having observed the Federal Republic closely for a year, with attention focussed on the federal administrative elite, this reviewer would judge the situation to be quite different. The senior administrative elite today enjoys financial and social privileges which make it a highly advantaged group with much prestige and visibility. To be sure they do not play a role comparable to that of the *Junker* elite—but then nobody does anymore in the 1970s. Thus by refusing to set a precise time frame for his study, Professor Armstrong has fuzzed the temporal focus of the work and has weakened it unnecessarily. Given the clearly longitudinal approach to the study, nobody could have quarreled with a decision to set a forward time limit at either World War II or soon thereafter.

Though the professed focus of the book is role perception, and specifically development interventionist role perception, I came away from the work with the impression that the author was determined to show how in all four countries the group self-interest of the administrative elite was the sole motive force in shaping everything from family socialization, peer-group socialization, and schooling at all levels through the selection, entry, and career pattern of the higher administrative services. This shifting of the work's focus, of its general tone and argumentation is the result of two fundamental decisions which, to Professor Armstrong's great credit, he has stated clearly at the outset and to which he has adhered scrupulously throughout the study. First, he has adopted a conflict model of social action in preference to a functionalist one (pp. 8, 202); and second, he has had recourse to an ideology/false consciousness explanatory framework, taken from Marx though generally lifted out from the remainder of the Marxian analysis.

It should be said immediately, that it would be difficult, if not impossible, to quarrel with the proposition that the European administrative elites are generally "closed corporations" that arrange matters to suit themselves, largely in defiance of politicians and people; Brian Chapman, among many others, demonstrated that quite convincingly fifteen years ago in his *The Profession of Government: The Public Service in Europe* (London: Macmillan Co., 1959). But where Chapman's essentially de-

scriptive approach (though surely normative disposition!) avoided the constant obtrusion of the basic theoretical points, Professor Armstrong's more theoretically sophisticated and self-consciously coherent analytic stance led him to hammer away at it page after page, mercilessly throwing his false-consciousness floodlight on all objects along the way and thus making them all appear grotesquely alike, all being bathed in the same color medium. The *Rechtsstaat* is merely a device to maintain the *Juristenmonopol*; Whitehall and civil service clubs are bucolic enclaves in metropolitan London; law and classics are useless exercises from a pre-industrial past, akin to the poetry writing of the Chinese mandarins. Does Professor Armstrong really suggest that penal code reform in contemporary West Germany bears "little direct relation to societal patterns" and that the legal framework of that, or any other European society is nothing but a "graceful pattern of words and categories" (p. 173)?

Can everything the administrative elite does really be fully explained by group interest and justified through ideology and false consciousness? For example, in his effort to demonstrate the ideological nature of the role of a classics education on the secondary levels (chap. 7), Professor Armstrong shows (Table XI, p. 139) that in the German classical-type secondary school between 72 per cent and 82 per cent of the curriculum was devoted to humanistic studies during the periods from take-off to the present. This figure is arrived at by categorizing history as a humanistic subject and by including, without special explanation, instruction in the grammar, writing skill, and literature of the native language. But from the post-World War I period onward, at least geography was added to history as a social science block; and another source about this type of curriculum in Germany and Austria presents this distribution: social sciences, 18 per cent; natural sciences, physical sciences and mathematics, 26 per cent; and humanistic studies, 55 per cent (of which 18 per cent is "Deutschunterricht" i.e., grammar, composition, and literature of the native language). Presented in this manner, the curriculum appears much less onesided.

The same sort of ideological treatment is accorded higher education in chapter 8. Again, Great Britain and Germany bear the brunt of the critique here because their higher education for prospective elite administrators (law and the Oxbridge honors program, respectively) reflect "quasi-aristocratic anti-specialist values" that presumably foster "the ability to

handle people" (p. 167). Why an ability to handle people is necessarily quasi-aristocratic is not clear; it would seem that any good old self-made American business tycoon would applaud such values. I'm afraid that in pursuit of the conflict model, Professor Armstrong tends to see many connecting links as sufficient ones, when they constitute at best necessary linkage.

The British generalist is seen as based on aristocratic modes that are no longer either valid or legitimate, and the attack on the German *Juristenmonopol* is cut from similar cloth. But at the very end of the study the author makes a plea for a new form of generalist—but a generalist, just the same—one who is trained basically in economics, but one who also appreciates technology, the social sciences, and the humanities (p. 318). It will be this administrative elite to which will be entrusted the ever more highly centralized and planned societies of the future without, seemingly, any concern that all this might again be conducted solely for the group interest of the new elite and that an elaborate ideology might again be constructed to justify this new form of elite domination. This tension between the bulk of the evidence mustered by Professor Armstrong and his concluding glimpse of the future is difficult to accept or come to terms with.

It was said earlier that the structure of the study is opaque; this statement requires some modification. There is no doubt about the purpose and the structuring of the core of the work, chapters 5 through 11, which trace the shaping of these four administrative elites from family through career patterns and prospects. Chapter 12, too, raises no problem. There the author deals with the only specific set of administrative institutions which bear very directly on development—territorial administrative structures. In the two concluding chapters, Professor Armstrong examines two challenges common to the four administrative elites, railroad construction and industrial mobilization for World War I (chapter 13) and skillfully draws out a number of implications of the development interventionist role (chapter 14). These two chapters do not summarize in any conventional sense but nevertheless bring the study to a full and satisfactory conclusion: we see where we have been and what are the consequences of these particular developments. The opening four chapters are less sharply drawn. Both chapters 1 and 2 are essentially definitional—and rightly so. The purpose of chapters 3 and 4 is least clear. The ideas developed there are diffi-

cult to follow and seem to lack connection either to what went before or what came afterward. These chapters are tough going intellectually, and it might well be that more concise formulation would have helped.

Touching on a few issues, here and there, can convey only very imperfectly the sweep of Professor Armstrong's argument, the strength of his analysis, and the stimulation the volume has given the reviewer. One is forced to reexamine a whole host of ideas, conceptions, assumptions, and just plain facts; and even if one does not, in the end, accept everything he has to offer, one is nevertheless immensely enriched by the exercise. Indeed, a brilliant and exasperating book; is there any higher praise?

ALFRED DIAMANT

*Indiana University (Bloomington)*

**Colonialism and Underdevelopment: Processes of Political Economic Change in British Honduras.** By Norman Ashcraft. (New York: Teachers College Press, 1973. Pp. 180. \$8.50, cloth; \$3.95, paper.)

In the approximately two and one-half decades since the question of national political, social, and economic development first became of concern to the students and practitioners of statescraft, conceptual approaches to the problem have practically come full circle. During the first few years after World War II the problem of the colonial areas was largely understood in terms of their political and diplomatic dependence. Break the links of dependency with the mother country (after establishing proper parliamentary and administrative institutions, of course), and the former colonial area would take its proper place in the pantheon of sovereign states.

What was thought at first to be primarily a political and diplomatic problem very quickly was seen to be an economic one. When, however, economic stagnation, balance of payments problems, despotism, military rule and general political and social turbulence failed to give way before the Alliance for Progress, direct international investment, or economic stabilization missions, it was argued that the developmental process involved a complex series of sociological, psychological and cultural transformations. But, now that the United Nation's "Decade of Development" is behind us we are back to the theory of dependency.

In this very interesting study of the political economy of British Honduras, anthropologist Norman Ashcraft brings the unique perspec-

tive of his discipline to the study of dependency. By employing social field theory—an "approach which permits investigation and in-depth analysis of people and their behavior at the local level, while at the same time revealing the context of that behavior" (p. 16)—he is able to delineate clearly the interaction between the individual peasant and the communal, national, and international socioeconomic environment. As such the study will be helpful both to the general Latinamericanist and to students of political economy and international economic relations.

Ashcraft portrays the British Honduran peasant as responding rationally to the constraints imposed by that environment, an environment which has its roots in the early development of the colony as an appendage of the developed world. The original economy was based on the provision of forestry products to England and the United States. Since the most efficient way to provision a relatively small population of slaves was with imported food, agriculture was simply a supplementary activity engaged in during spare time on the periphery of the campsite. After the decline of the forestry industry during the 1930s the by then emancipated workers found it difficult successfully to change to agriculture. The addition of the urban population to buying imported food, the reluctance of urban merchants to handle locally produced fruits and vegetables, and the lack of a well developed marketplace for the local grower all worked together to restrict the market even though the nation has had a perennial food deficit. Consequently, because the peasants could not support themselves through agriculture they felt compelled to take on as much daily wage labor as possible, thereby retarding further their development as viable small farmers. Furthermore, the colonial government encouraged the shift of investment from a declining forestry industry into commercial agriculture which earns much of the foreign exchange needed to import food.

As an anthropologist, Ashcraft sees the perpetuation of milpa agriculture not as archaic or uneconomic, but rather as an efficient means of adapting to an economic situation far beyond the limited capacity of the peasant to control. "There are many farmers who are fully knowledgeable of the merits of permanently cropped fields. . . . They will tell anyone who will listen that changes require heavy expenditures of capital, not only for land improvement, but for equipment as well. They would ask how they could get the increased production to market efficiently. And . . . once



they are there, where are they going to find the market?" (p. 170).

Ashcraft concludes by pointing out that underdevelopment in British Honduras is a part of a world system of development/underdevelopment; that it is impossible for small nations whose only marketable resource is agriculture to have any control over the course of their own development. This conclusion may be less valid given the recent awareness by the periphery of its power to withhold resources from the metropolis. Thus the theory of dependency may be around for some time to come, but with a whole new cast of characters.

ROLAND H. EBEL

*Tulane University*

**Municipal Administration in India.** Edited by A. Awasthi. (Agra, India: Lakshmi Narain Agarwal, 1972. Pp. viii, 539. Rs. 40.00.)

Professor Awasthi has undertaken the herculean task of surveying the state of municipal administration in India. His volume is divided into four parts: (1) description of the problems of urban administration including training in municipal administration, national control and assistance to municipal bodies, and public participation; (2) a survey of municipal administration in sixteen states; (3) analysis of the Bombay, Calcutta, Delhi and Raipur municipal corporations; (4) concluding reflections.

G. Mukharji's statement of the problems of urban government presents no new insight but is rather a compendium of the all-too-familiar ills which beset India's cities. Deva Raj's review of training in municipal administration is a valuable summary of the efforts of Government and the universities to develop training materials and programs to help municipal administrators coping with the mammoth problems of Indian urban life.

Professor Awasthi's essay on "Government Control and Assistance to Municipal Bodies" will aid students of Indian government to understand the relationship between the union and state governments on one hand and municipal bodies on the other. Awasthi traces the early development of such control and assistance in education and revenue promotion, through the broader British objective of promoting responsible government to the present. Widespread arbitrariness of municipal administrators is documented by judicial decisions overturning such actions and by the numerous state supersessions of local administrations for corruption, inefficiency or arbitrariness. Although state governments in India have begun to wrestle with their urban prob-

lems, the present collectors and commissioners supervising urban administration still conceive of these problems as secondary to their major statutory duties. Crucial to the understanding of state treatment of municipal problems is the realization that state officials continue to focus their major efforts on the legalistic and fiscal aspect of urban problems rather than on the programmatic ones.

In attacking the problem of public participation in local government, A. P. Awasthi has emphasized such broad cultural factors as poor newspaper coverage of local affairs, low literacy voter rates, lower levels of prestige for involvement in municipal affairs and paternalism in state government. He advocates a three-point program which includes organizational change, the creation of voluntary associations and a broad-based attempt to educate citizens.

The major portion of this volume is a survey of municipal administration in sixteen states and in selected cities. The treatment of the states is similar and includes a historical statement, a description of the structure of municipal government, municipal powers and functions, the extent of municipal services, municipal finance, municipal personnel, and a conclusion section. The reader is thrown into this state-by-state survey without any introduction or guide. As a result, the wealth of material presented is confusing. An introductory section could have described the well-defined municipal models which actually exist. One such model is the Bombay variety, which places executive authority of a Municipal Council in a single chief executive officer. This model is widely applied throughout India. A second model places executive authority in the head of the municipal council; while this model was adapted in Uttar Pradesh, Madhya Pradesh, and Tamilnad, it was abandoned after the conflicts it produced between the municipal council members and the head of the council. A third type is the commission form of municipal government, which was in existence in Calcutta between 1923 and 1951. In a variant of this model, executive power is delegated within the municipal council by means of standing committees as illustrated by the Madhya Pradesh Corporation Act of 1956.

Although a great deal of valuable information is presented in the editor's concluding reflections on the state of administration in India, its effectiveness is lost because it comes too late in the volume. If future editions are contemplated, it would be desirable to have the concluding section precede the survey by states.

The state surveys of municipal administra-

tion are completely dehumanized in their presentation. The approach is legalistic, structural, and descriptive. One will search here in vain if he seeks to relate municipal administration to the problems of urban living in India. There is no discussion on the relationship of administration to such vital areas of human life as health and sanitation, housing, transportation, pollution, education and employment. The struggles of the citizens of Bombay as they crowd aboard antiquated buses, the suffocating results of diesel pollution in Delhi, and the failure of educational goals in Calcutta are completely ignored in this volume. From a methodological point of view, the authors have failed to use modern scientific tools in their evaluation. No effort has been undertaken to develop financial yardsticks by which to measure the costs or benefits of service, nor have programs been analyzed in terms of modern management objectives. It is hoped that future scholars of Indian administration can wrest their subject from its traditional moorings to achieve a creative treatment that this book sadly lacks.

BENJAMIN N. SCHOENFELD

*Temple University*

**Mao Tse-tung and Gandhi, Perspectives on Social Transformation.** By Jayantanuja Bandyopadhyaya. (Bombay: Allied Publishers, 1973. Pp. 156. Rs. 24.)

It used to be popular to characterize India and China as engaged in some sort of competition to validate contrasting modes of government by the excellence of their economic performance, this competition presumably to have an effect on what happened in other parts of the world where leaders awaited the verdict. India and China, of course, never were "democracy" and "totalitarianism" personified, and the rest of the world was not waiting with bated breath to see which of these two countries could raise its living standard faster. The entire construct was a cold-war fantasy.

J. Bandyopadhyaya, Professor of International Relations, Jadavpur University, Calcutta, has now attempted to pose the old India-vs.-China question in new, more promising terms. Bandyopadhyaya has broadened the focus, structuring his essay as an attempt to contrast two movements, Chinese Communism and Indian Nationalism. This permits comparisons extending backward in time beyond the formal attainment of political power by these anti-imperialist movements, and including the entire range of issues posed by the social transformations of these two countries. As Bandyopadhyaya shows, useful comparisons can best

be made by examining the full sweep of the political experience of India and China from the days of imperial exploitation to the present.

Bandyopadhyaya's perspective makes possible an appreciation of the essentially nationalistic nature of both movements. Both Gandhi and Mao, the author observes, attempted to mute internal class antagonisms to present a united front against imperialism. And while we are unaccustomed to comparisons between Gandhi's technique of nonviolent struggle, Satyagraha, and Mao's strategy of "people's war," Bandyopadhyaya shows that both reflected complex and relatively successful attempts to transform ancient societies thrust into temporary disarray by unexpected Western challenges.

Bandyopadhyaya asks many of the right questions, but his answers are often unsatisfying because his primary concern is to inquire whether Indian nationalism and Chinese communism are, in the author's words, movements "truly conducive to the promotion of liberty, equality and fraternity" (p. 127). Bandyopadhyaya's commitment to those goals he considers ideally preferable hampers his understanding of the dynamic nature of the particular movements he is examining. Bandyopadhyaya, for example, criticizes Gandhi's identification with Hinduism. He describes Gandhi's solution to the Hindu-Muslim problem as "ahistorical and simplistic." He concludes that "the solution of the communal problem in India required a completely secular and multi-pronged offensive, involving the attempt to achieve economic, political, and cultural integration of the two principal religious communities" (p. 89). Gandhi is similarly censured for an indifference to economic development through state initiative. Bandyopadhyaya suggests that once a high level of economic development has been attained, Gandhi's philosophy will become more relevant. "High technology . . . combined with non-violence . . . may yet bring us close to some of the utopias . . . which rare and visionary human beings have dreamt of" (p. 140). A heavy burden of proof must rest on one who contends in this fashion that a great leader misunderstood his situation and that his techniques would be better adapted to some other situation. Gandhi, like Mao, was successful because he worked within a context. Many of Gandhi's seemingly simplistic statements must be seen, not merely as diagnosis, but more importantly, as exhortations forming part of a total program of action. Gandhi and Mao were not scholars seeking objectivity, but leaders attempting to motivate action.

Bandyopadhyaya advocates for underdeveloped countries "a libertarian political system . . . combined with a nonviolent programme of mass mobilization and conflict resolution for the establishment of socio-economic equality, as advocated by Gandhi, as well as with a broad non-Gandhian programme of industrial and technological progress" (pp. 126-127). This is all very well, but suggests the abstractness on which the essay ultimately founders.

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**The Ordeal of Nationhood: A Social Study of India Since Independence, 1947-1970.** By

Krishan Bhatia. (New York: Atheneum, 1971. Pp. 390. \$10.00.)

Krishan Bhatia, a journalist of long standing, with the experience of covering Gandhi's visits to Delhi, traveling with Nehru and holding top positions in Indian journalism, including editorship of *The Hindustan Times* for some time, has tried in this book what he calls a case study in social dynamics of a mass society in transition. He has attempted the task neither in the style of prophets of gloom who had started spelling out India's dangerous decades when she had hardly set out on her career, nor in the spirit of functional political scientists who try to rationalize everything, but in the sympathetic and balanced way of an accomplished journalist. The narration is impartial and fascinating. Descriptions are detailed and vivid. Criticism is cautious; comments careful, never carping; and appreciation unstinted, but not effusive.

In 1947, India, with a population of 561 millions, was still a colossus, a giant, as Nehru called it, but she was faced with serious challenges to her national unity—in the form of communal differences, language barriers, regionalism, untouchability, and various other political, constitutional and emotional obstacles. Bhatia has not tried to analyze the factors of language, communalism, or parochialism from the angle of a political sociologist, even though he claims to be making a social study of the country. His assessment that, while the disintegrating factors were there, they were not so imminent in the early years has been borne out by subsequent events. Linguistic agitation took a virulent form only in 1953 and led to a redistribution of the provinces in 1956. Hindu-Muslim riots were revived in 1961. Regional parochialism, leading to the rise of Shiv Sena in Maharashtra and other militant organizations elsewhere, is a still more recent phenomenon.

Bhatia has made a good study of the Indian

political system and its leadership. Though India's political system was a cumbersome transplant, and an imported model, it made a good start and was working more or less satisfactorily. India has had regular elections, in which the electorate has shown maturity, in spite of 76 per cent illiteracy, and the political processes have been characterized by a high degree of sophistication; changes of prime ministers have taken place more or less smoothly; and political stability uncommon in Asia has established itself. Independence of the judiciary and the existence of a free press have further strengthened the political system. The fact that the real authority rests with the prime minister and not with the president, that the Parliament is a real check on the Cabinet, and that, in spite of there being no challenge to the dominance of the Congress Party, it has behaved on the whole with temperance and restraint, are quoted as examples of the viability of the Indian political system. The author is aware of the danger-signals which could threaten the existence of the political system later on: the planning commission often planning unrealistically and setting targets well beyond financial and physical competence; states, not subject to any authoritarian direction from the center, disregarding priorities and targets; bureaucracy proliferating while its character remains unchanged; the party system new to the country and leading to frequent floor-crossing and toppling of governments; and political parties themselves lacking any political integration.

The Congress Party, "the mother of all political parties," is discussed at some length. The author is aware that the Congress Party, physically wearied even in 1947, had become intellectually and morally exhausted within a decade, but when we see what happened in Pakistan and Burma because of the absence of an effective political party, we can understand the contribution the Congress has made to political stability in India. Bhatia is aware of the party's amorphous character. He mentions Gandhi's advice for the dissolution of the Congress Party without comment, but appreciates Nehru's policy of using its well-rooted national organization for maintaining a stable administration in the country. It is, however, clear, as Bhatia convincingly demonstrates, that the Congress has become increasingly complacent and slothful, it has lost touch with the masses and allowed vested interests, landlords, businessmen, even princes, to creep in; its leaders are ostentatious in living and vulgar in the exercise of power; and they have made ruthless compromises for winning elections. Bhatia has picked up, like a good journalist, some major

political events for more detailed comment. He has discussed the Kamaraj Plan in all its implications, including Nehru's alleged desire to use it for the ultimate rise of his daughter as prime minister—a point which he has contradicted by strong arguments—and the 1969 split in the Congress Party, along with the possible alternatives which could have followed it.

His chapters on the Indian army and on India's foreign policy, in its regional and global aspects, offer lucid description and balanced analysis. His discussion of the mistakes that India has made persistently in the field of economic policies especially is thorough and forthright. India's economic development, according to Bhatia, has been exceedingly poor, as a result both of inadequacies of leadership and imperfection of techniques employed. Her land-reforms have been a miserable failure. The top 10 per cent, owning 56 per cent of the cultivable land, and not the small owners or tenants, have received all the advantages of economic development in the country. Nationalization—of aviation, life insurance, road transport and, later, of banks, Bhatia makes it very clear, has been the result not of any deep-rooted commitment to socialistic goals but of political expediency. Where Bhatia seems to have faltered is in analyzing the causes of India's failures, political and economic. His chapters on "Stamina for Survival" and "Values and Attitudes", the weakest in the book, could have been used for a more thorough analysis of the socioeconomic infrastructure from which the Indian political system draws its sustenance. The fact that the book was brought out in 1971 has saved the author partially from the responsibility for this kind of analysis. Developments in India since 1971 have highlighted the strains and stresses, mainly economic, to which the political system is being growingly subjected. India's dangerous decades are not over. They lie ahead!

S. P. VARMA

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**Politics in Rhodesia: White Power in an African State.** By Larry W. Bowman. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 206. \$9.50.)

Since its conquest and occupation by Cecil Rhodes's British South Africa Company in 1889, Rhodesia has been "... a country where much seems to happen but nothing seems to change" (p. 151). Thus Professor Bowman sets out his fundamental thesis that elements of continuity have largely determined white minority rule in Rhodesia. The author spent 1965-67 as Lecturer in Government at Univer-

sity College of Rhodesia, where he carried out thorough selective interviews and documentary research. The result is a succinct and insightful analysis of the Rhodesian political system in historical perspective.

Bowman focuses on three distinct yet overlapping levels of conflict. The settler/indigenous cleavage, the most critical, is paramount in determining the persisting structural arrangements; the settler/metropole cleavage determines the scope for strategy which can be pursued by the dominant settlers (and conversely the narrow scope confronting any indigenous opposition); and the internal differences within the settler community indicate the success or popularity of any given strategy pursued or set forth by the local government in maintaining domination over the indigenous peoples (or in dealing with metropolitan interests).

In Professor Bowman's view the structure of Rhodesian society was determined by initial British imperial decisions. Since Britain delegated political and economic responsibility for the new Rhodesian colonies to the chartered British South Africa Company, the possible forms of development open to this society were delimited. Given the scope of autonomy ascribed to the chartered company, it was clear that the settlers themselves would be the heirs of that autonomy.

As Colin Leys has previously argued, the sole issue for the settlers has always been the maintenance of power and privilege over the indigenous African population. Bowman thoroughly and effectively amplifies this view in the context of political development after 1958. In addition to his references to the Land Apportionment and Industrial Conciliation Acts of the early 1930s as the foundations of white domination over African land and labor, he also directs our attention to the more recent policies and actions of white Rhodesians which have reinforced their hegemony against the challenge of African nationalism.

In restating that the saliency of race has engendered single-party domination, Bowman also points to the significance of the British regard for the settlers' own preference for internal self-government (as expressed in 1923) as the most important act in Rhodesia's political development. Hence, Rhodesian settler politics have revolved around the strategies and tactics of (1) preserving white rule, (2) expediting political independence, and (3) expanding white privilege. Despite a vastly inferior numerical position vis-à-vis the indigenous peoples (now 250,000/5,250,000), Rhodesian whites have been able to experiment with two distinct strategies in their pursuit of white-

dominated independence: (1) the broader framework of the Central African Federation, and (2) the narrow nationalism of a unilateral declaration of independence (UDI).

Perhaps the white Rhodesians' supreme achievement was to overcome British objections to federation with Northern Rhodesia and Nyasaland in 1953-54. Although the settlers proclaimed the theme of "partnership" to the British, their actual goals in federation were "stability + economic development + the British Way of Life." The seemingly moderate United Federal Party's advocacy of this configuration did not meaningfully differ, says Bowman, from the policy goals of the Rhodesian Front ten years later, though the operational base was different. From 1933 to 1962 the UFP was ruled from the top in a paternalistic/autocratic manner (indeed, this style extended into the Rhodesian Front Government of Winston Field). When Ian Smith took power in 1964, a collegial/authoritarian "movement-regime" was established with effective controls emanating from the settler grassroots. Bowman's chapter on the structure of the RF fully elaborates this difference.

On Rhodesian African nationalism, Bowman's assessment is perhaps a corrective to other interpretations. Most observers have been critical of the Rhodesian African nationalist leader, Joshua Nkomo, for not accepting the conditions laid down by the British and white Rhodesians at the 1961 Constitutional Conference in Salisbury. But Bowman concludes that none of it made that much difference. He does indicate, though, that Nkomo and his nationalist colleagues frittered away the one opportunity that they have had to negotiate from a position of at least minimal disadvantage. Bowman argues, however, that this would have brought about the establishment of the same sort of white nationalist party as that of the Rhodesian Front. Further, this entire crisis (for the Africans) was intensified by African nationalist misperceptions of strategy. While they saw the Zambian or Kenyan decolonization models for their solution, they might have benefited more from looking at the Algerian one.

The British, even beyond federation, have consistently inclined toward white Rhodesian rather than African interests. The British objection to UDI has thus been on legal rather than political grounds, i.e., the British have not so much objected to Rhodesian independence under white minority rule as they have to the illegal seizing of such independence by that white minority.

Perhaps there is a comparative dimension

which could furnish us with an even fuller understanding of the Rhodesian configuration. From an objective comparative angle, there have been other settler-founded societies which broadly resemble the Rhodesian structure. The author indirectly reminds us of this when he predicts that Rhodesia would probably become either a black state via the Kenyan experience or else more permanently white through the South African one. In terms of the subjective comparative view, white settlers under imperial Britain have expected independence and Britain has done little to discourage this feeling in Rhodesia. Local government (and autonomy) was to the settlers a "British right" guaranteed by the precedent of the Durham Formula in Canada. This point has given them a sense of self-confidence in asserting their own authority over the indigenous peoples and in challenging any British assertions to the contrary.

Professor Bowman's analysis will probably stand as a definitive political history for some time to come. His keen insight and painstaking research have borne much fruit, and it is becoming increasingly difficult to carry out political research in Rhodesia. Certainly Bowman's work ought finally to dispel the myth of British support for democratic government in Rhodesia.

BARRY M. SCHUTZ

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**Planning and Budgeting in Poor Countries.** By Naomi Caiden and Aaron Wildavsky. (New York: Wiley, 1974. Pp. 371. \$14.95.)

"If we were asked to design a mechanism for decisions to maximize every known disability and minimize any possible advantage of poor countries, we could hardly do better than comprehensive, multi-sectoral planning. It calls for . . . information . . . , knowledge, and a political stability . . . undreamed of in their existence" (p. 293). Apply keen insight, ruthless candor, and intellectualchutzpah to the subject of planning and budgeting in poor countries and you get statements like this—and more.

This is an antidotal book, an agnostic book, an incisive and provocative book. It is brazenly intolerant of certain forms of human foolishness. Some people won't like it, and not all of them will be without understandable reasons. Yet this book should be absorbed by students of development, by planners and budgeters in poor countries and would-be practitioners of those arts, by anyone interested in the actual mechanisms of governance in the service of social change.

Much of the voluminous literature of planning addresses intrinsic features of the subject, sometimes in ways that can only inspire awe in ordinary mortals. Likewise, a lot of the literature of budgeting deals with techniques and intents as seen from within the domain of the process. This book looks across boundaries. It examines planning and budgeting in the context of a particular sort of environment—the poor countries. It focuses upon the interplay of process and setting, and shows how the latter influences the effects of the former. The study also treats the interplay of planning and budgeting, and holds that “Budgeting precedes planning. Budgets can be made without long-range plans, but those plans cannot work without budgets. . . . Budget makers can live without controlling the plan, the planners cannot be effective without a say in the budget. When multi-sectoral, comprehensive plans are unrealistic, a few planners are unhappy; when budgets lose touch with reality, a nation is in deep trouble” (p. 290).

From this position the Caiden-Wildavsky book proceeds to argue that one key to enlarged control over the terms of development in poor countries is budgetary reform. National planning in a poor country should, in this view, be merged into the ministry of finance. But “What if people do not feel right without seeing the word ‘planning’ somewhere on the governmental table of organization? Change the name of the finance ministry by adding ‘and planning’ to its title. . . . What would become of the planning commission? It would go out of business. Who then would do long-range planning? No one, effectively, as is indeed the case today” (pp. 296–297).

As for budgetary reform, for most poor countries its essence is indicated by the label, “continuous budgeting” (pp. 315–321). A distinguishing feature of budgeting in poor countries is the constant reconsideration of budgetary allocations. Budgets are not really comprehensive statements of governmental intentions for the coming year (p. 316). Uncertainty and the other contextualities of financial management in poor countries allow no way to lick this reality. So let us make the best of it: “let the budget process be explicitly geared to making ad hoc decisions on resource allocation against a background of what is known about revenue and expenditure at the time” (p. 316). Departments would be certain of their established budgetary bases, but variances—additions, new money—would be dealt with continuously, case by case, at any time in the year. The aim would be a better fusion of the opportune and the feasible.

These statements merely sum the conclusions of an argument. The authors begin that argument with a destructive distillation of the rationale of comprehensive planning. They proceed to stipulate the distinguishing properties of poor countries—properties central to any concern with development. If these properties—poverty and uncertainty—are obvious, the authors make clear that their implications for planning and budgeting are not. Three trenchant chapters, reminiscent of Wildavsky's classic work on *The Politics of the Budgetary Process*, portray the realities of budgeting in poor countries. Then the primordial intentionalities of planning are examined—and juxtaposed against the operant realities that Caiden, Wildavsky and their colleagues perceived in field work in fourteen countries and a comprehensive study of the literature. (The bibliography includes about 600 cited items.)

The realities of efforts at comprehensive planning are found to be irreconcilable with the intentionalities. There is no way by which poor countries can really do comprehensive multi-sector planning. Moreover, trying to do it has more often than not had perverse effects. In the language of the title of Chapter Nine: “Planning is Not the Solution: It's Part of the Problem.” “Significant control of the future demands mobilizing knowledge, power, and resources throughout a society. It does no good to propose measures that require nonexistent information, missing resources, and unobtainable consent” (p. 288). Formal national planning is, in essence, not rational.

The evidence incorporated in this study is interesting. This is not a “systematic” study in the sense that some social scientists would insist upon in using the phrase. The method and its defense are presented in the introduction. In its broadest ambit the work covers “over 80 nations and three-quarters of the world's people”—the countries with GNPs of less than \$800 per year (p. xii). A dozen of them were chosen as sites for intensive interviewing, and “validation interviews” were conducted by Wildavsky in two others. These data, plus information from reports and publications, provided the basis, not “for a record of contemporary events but to describe how a process works” (pp. xiv–xv). The result is a composite description of the planning and budgeting processes, or the central tendencies in those processes among a large class of countries.

The categories about which information was collected were determined in a straightforward fashion by the concerns of the authors. Those dedicated to comprehensive quantitative planning exercises will challenge the scope and ade-

quacy of the evidence. But the focus here is not upon intrinsic properties of planning technology, but upon "how things work" across the interfaces between planning and its environment, and budgeting and its setting.

The analysis has three elements. One is a description of central tendencies in the processes under scrutiny. This emphasizes the empirical at the expense of pretension. The second is a set of judgments of *why* things tend to work as they appear to. For me, intuition and experience validate the authors' interpretations; others may disagree. Third, the analysis proceeds to praxiology. The proposals derive from a combination of the facts, the explanations, and the wit and expertise of the writers. These include a monumental grasp of the budgetary process, a cogent appreciation of the properties of a certain approach to planning, and a keen political perspective.

It might be argued that to attack comprehensive multisector planning is to flay a dead horse; but there are today a lot of national planning agencies among the poor countries of the world. It can be said that the work lacks a hopeful appreciation of the full potentials of established planning and budgeting technology-cum-ideology. It does indeed. Others may argue whether this is good or bad.

All in all, this book is an epitome of what can be produced when intelligent social scientists address problems—and possible solutions—in instances where important phenomena cannot be reduced to neat determinacy, or subjected to reassuringly computational treatment.

WILLIAM W. SIFFIN

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**The Bulgarian Jews and the Final Solution, 1940-1944.** By Frederick B. Chary. (Pittsburgh, Pa.: The University of Pittsburgh Press, 1972. Pp. 246. \$9.95.)

This useful monograph, the first of its kind in English, is an elaborated and somewhat abbreviated version of a doctoral dissertation (University of Pittsburgh in 1968). The problems involving the survival of the Jewish community in Bulgaria proper and the destruction of the Jews of Macedonia and Thrace under Bulgarian dominion during the Second World War, have been the subject of intensive research for a number of years. The more notable published books on this theme include the works by Benjamin Arditti (1962), Haim Kishales (1970), Michael Molho and Joseph Nehama (1965), Natan Grinberg (1961) and Buko Piti (1969), all five of which have been published in Israel, the first three in Hebrew and the latter two in Bulgarian.

Together with the Jews of Denmark and Finland, the Jews of Bulgaria escaped with their lives in the midst of the wholesale catastrophe in Europe. While the Danes were overrun by the Germans and the Finns actively fought on Hitler's side, however, the Bulgarians took an intermediate stand. Led by their late king, they were Germany's willing junior ally. Bulgaria did not fight the Soviet Union and throughout the war preserved a measurable degree of autonomy vis à vis the German overlords.

The Bulgarians undertook to demonstrate their faithfulness to the maxims of the New Order by harassing, molesting and persecuting the relatively small Jewish community numbering just under fifty thousand. Comprehensive anti-Semitic legislation patterned closely after the German model was duly introduced and enforced. Yet when the time came for the Jews to be handed over to the Germans and deported in the early spring of 1943, Germany's fortunes of war had shifted. Organized opposition to the deportation schemes developed within the government's own majority in parliament. The antigovernment criticism sprang from amongst nominally right-wing and reactionary deputies who thought the deportation plans ill-advised in view of Bulgaria's ultimate national interests. Their outcry, together with protestations from liberal and church elements, blunted the government's will and created a delay which ultimately became irrevocable. While successful in its main design, the spur of protest did not suffice to prevent the deportation and the ultimate extermination of the Jews from those areas in Greece and Yugoslavia given to Bulgaria by Germany as a prize for Bulgaria's wartime collaboration.

The above sequences are retraced by Dr. Chary in some detail. Because of the narrow contours of the monograph, delineated by the subject matter itself, the author should have seen it as his responsibility to supply the reader with a broad analysis of the socio-political dimensions which permeate the story itself. With a few exceptions no such analysis is in evidence.

The relative smallness of the Jewish community and its overwhelmingly urban setting do not suffice to explain why anti-Semitism in Bulgaria did not strike deep roots. The facts are that the Bulgarian Jews did not reach outwards and did not show propensities for assimilation within the majority not because they not permitted to do so, but because they were determined to preserve and fortify their own insular way of life. While participating actively in the economic life of the country and claim-

ing a proportionately larger share of material wealth than their numbers would have justified, ever since the liberation of Bulgaria from the Turks the Jews endeavored to emulate their Christian compatriots in fostering the sentiments of Jewish nationalism. A keen sense of modern Jewish nationalism (primarily of Zionist coloration), augmented by rapid cultural secularization provided the Bulgarian Jewish community with a fully blown autonomous political life of its own. This more than anything else spared the Jews from colliding with the national sentiments of the Bulgarian majority. Except for the cursory outline of the Jewish consistory administrative system Chary makes no effort to depict the inner-political life of the community.

The real workings of a primitive institutional political pluralism preserved by the Bulgarian state during the war and throughout Boris's dictatorship are left amorphous and flat. What were the true motives, or at least the uttered arguments, of those political figures from the right and the left of the parliamentary spectrum who defended the Jews and castigated the official policies of the government? The full texts of their speeches (primarily those of Mushanov, Stainov and Peshev), are readily available, as are the speeches of Professor Tsankov, the principal ideologue of a particular brand of Bulgarian fascism. Yet we are not given even a taste of their political flavor.

Like many others, the author is misled by the demagogue leftism of the Zveno people (p. 4); essentially Zveno was and remained a crypto-fascist group with some enlightened ideas in the economic and foreign policy domains. The influence of Jews in the Bulgarian Communist party was greater than the author believes (p. 32). The most outstanding Marxist economic historian in Bulgaria was a Jew, as was the head of the radio broadcasts from Moscow during the war being beamed to Bulgaria itself. Unfortunately, the author does not make use of the stenographic texts of these same broadcasts (available in seven published volumes) which, indirectly at least, reflect Moscow's attitudes on the problem.

Chary rightfully indicates that "No other institution with comparable influence so consistently opposed the government's anti-Semitic policy as did the Holy Synod of the Bulgarian Orthodox Church" (p. 188). Yet, why did the Bulgarian Church, historically always compliant with the will of the state, behave as it did? The admirable intervention by Angelo Roncalli, the future Pope John the XXIII (apostolic delegate to Turkey and Greece dur-

ing the war) on behalf of the Bulgarian Jews in the crises of 1943, and the advice given to him by the Vatican not to concern himself with such political matters (p. 188) should have been amplified rather than buried in a footnote.

Chary has done adequate work with the German archives. Yet the author has not tried to master the extraordinary efforts undertaken by the Jewish Agency of Palestine by means of its representatives in Istanbul on behalf of the Bulgarian Jews, even though the archive materials on the subject are available and accessible. It is not for lack of trying by the author that the role of Boris and the events surrounding his death remain unclear. Balan, the king's advisor was not executed by the Communists in 1945 as the author maintains (p. 179). The author is right in pointing out the cruel anomaly created in late 1943 and early 1944 when Bulgarian Jews could obtain exit visas from the Bulgarian authorities while "the major problem for the prospective emigré was obtaining a British entry visa to Palestine" (p. 157); still, the escapees never became "a strong and steady flow" as Chary indicates. Finally, one would have wished that the text of The Law for the Defense of the National Security of 1941, the principal legal instrument of the Bulgarian government's anti-Semitic design, would have been retained in the appendix section of the book the way it is reproduced in the doctoral dissertation version.

NISSAN OREN

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**Israel: Social Structure and Change.** Edited by Michael Curtis and Mordecai S. Chertoff. (New Brunswick, N.J.: Transaction Books, 1973. Pp. 443. \$4.95.)

This is a valuable book, one of the few recently published which deals seriously, objectively, and even controversially with some of Israel's complex internal economic, social, and to a lesser extent political problems. The quality of its 27 selections is generally high, though the scope of its coverage is uneven.

The articles deal substantively and extensively with a few key issues, such as Western versus Eastern Jews and the kibbutz. Other issues, however, such as the role of the military, the question of the Arab population, educational reform, the economy generally and the Histadrut in particular, receive less extensive treatment.

The complete exclusion of macropolitics, however, in a highly politicized and centralized state, where economic and social policy decisions are preeminently political in nature and



national in scope, is questionable. Admittedly, micropolitics in Israel have received less attention in the past.

In this connection, Myron J. Aronoff's article on development towns bridges the center-periphery gap by identifying the key local centers of power and discerning their ties to national political parties and bureaucracies. Judah Matras explores some of the problems of inducing immigrants to go to and to stay in these far-flung development towns. He cites some of the successful experiments and some of the failures, but leaves unanswered the intriguing question of why some succeed and others don't.

The most important and controversial of the fundamental issues dealt with in this volume is the interethnic question of Ashkenazim (Western Jews) versus Sephardim (Oriental or Middle Eastern Jews), with their concomitant differentials in income, housing, education, occupation, social mobility and status, and political participation, access, and influence.

Two schools of thought on this issue implicitly emerge from among the book's contributors. The first, including Shlomo Avineri, Oded Remba, Yoram Ben-Porath, Moshe Lissak, and Daniel Elazar, may be dubbed the optimists. They tend to view the problem of the Sephardim largely as a consequence of "imported" factors: lower educational levels, limited occupational skills, lack of capital or sources of income from abroad, and a general absence of leadership, compounded by the initial manipulative approach of the government and the political parties.

These authors see a subsequent change in government policy and in the attitudes of the (largely Ashkenazi) political elite, whether out of altruistic, Zionist sentiments or for more pragmatic political reasons. Avineri and Elazar point optimistically to the rise of the Sephardim as a power in local government and party structure. This process is supposed to produce a home-grown political leadership and to lead to the ultimate political and social integration of the Sephardim.

The economic gains made by the Sephardim cited by this school are impressive. Indeed, Ben-Porath (p. 233) finds that the rate of improvement for Sephardim is higher than for Westerners. Given the progress to date, the prognosis is for a further narrowing of the income gap which now puts a Sephardi family's income at about 70 per cent of the Ashkenazi's (Remba, p. 203). Avineri also cites the increased rates of intermarriage (p. 294). Thus, time, elite compassion born of enlightened political self-interest, and the normal political

and social processes will eventually lead to Ashkenazi-Sephardi integration.

At the other end of the spectrum stands a smaller, more militant group of writers, including Seymour Martin Lipset, Celia S. Heller, and Henry Toledano. The gist of their argument is that the depressed social and economic situation of the Sephardim, caused by a combination of imported and domestic factors, is now stagnant, if not deteriorating. The Ashkenazi political elite is indifferent at best, hostile at worst. Whatever policies the government has reluctantly adopted, under the pressure of anomic violence, have been cosmetic.

Given the increasing polarization along class and ethnic lines, the only way out for the Sephardim seems to be political, i.e., the organization of a Sephardi political party. Lipset estimates (p. 351) that such a party can "easily secure 20 to 30 per cent of the Kneset seats," and can use the coalition mechanism to achieve Sephardi equality. Why such a Sephardi political party, which has been tried and has failed several times before, would succeed now is left unexplained. And why the cross-cutting cleavages which predispose the Sephardi voter to choose one of the larger, aggregative parties—Labor, Likud, or National Religious—is also unclear.

Neither side in the debate is totally convincing. The real Israel is not a blissfully harmonious social paradise, nor is it a seething class and ethnically split pre-revolutionary state. Moreover, the debate itself is not susceptible to black and white answers. Nevertheless, its relevance, and that of the book, has only been heightened by the 1973 war. On the one hand, the postwar economic squeeze hit hardest those on the lowest economic rungs (mostly Sephardim), possibly increasing their resentment. On the other hand, the end of the post-'67 boom may also put the lid on the conspicuous consumption which, as Howard Pack asserts (p. 189), greatly exacerbated inter-communal tensions. Similarly, while the war may result in a nationalist closing of ranks, the costs thereof may decrease the resources available for the improvement of the Sephardi's condition.

Another chief focus of the book is the kibbutz. With the exception of Bruno Bettelheim's rather naive, idealistic view, the other authors shed some interesting, if not ground-breaking, light on the kibbutz. Editor Curtis's own perceptive, sympathetic, yet realistic article focuses on some of the discrepancies between ideology and reality in today's kibbutz: the necessity of hiring outside labor; the question of privacy and individual freedom versus the interests of

the collective; the problems of industrialization—hierarchy, specialization, higher education; the 20 per cent dropout rate and the more general problem of attracting and retaining the youth; while at the same time finding meaningful roles for the aged in a work-oriented community. The decline of the kibbutz in the political system is only briefly touched upon.

Suzanne Keller's analysis of the family in the kibbutz—particularly her critical remarks on the as yet unliberated kibbutz woman—contrast sharply with the rather positive views of Nancy Datan (p. 387) on "the viability of the traditional female role" in Israeli society generally.

On the whole, Curtis and Chertoff have assembled an interesting and variegated selection of articles on some of Israel's chief domestic issues. The book is a useful tool as an introduction to contemporary Israeli society, as background for its internal political problems, and as a reader to accompany a more systematic text in a course on Israel.

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**Les Forces Politiques en Thaïlande.** By Jean Duffar. (Paris: Presses Universitaires de France, 1972. Pp. 176. F16.)

It is curious that in spite of substantial French influence on law and politics in Thailand, a tradition of archeological and historical research by French scholars on ancient and pre-modern Siam, and continued intellectual and practical interest among the French of Southeast Asia, French scholarly writing about contemporary Thailand is almost nonexistent. M. Jean Duffar of the faculty of Paris University seeks in this volume, *Les Forces Politiques en Thaïlande*, to close the gap.

The short book (163 pages of text) is divided into two principal parts entitled "The Established Power" and "The Contesting Forces." Within this broad framework M. Duffar attempts to penetrate the dialectic of Thailand's political life and to forecast in a general way the trends of change. Although the book was published in 1972, the material from which the author derived his presentation appeared largely in publications of the mid-'sixties and earlier. He draws entirely on French and English sources, both scholarly and journalistic, and there is no evidence of any work on the ground.

Part One, concerned with the structure of political power in Thailand and its social context, is a sound and comprehensive summary. M. Duffar identifies as central the consolidation of the base of political power in the military

and its business based allies. He notes also the fundamental cohesion of the national elite which rules the country within a broadly based attitude that supports its political autonomy and which operates through a comparatively effective administrative apparatus. He also discusses at some length the expanding and increasingly ambivalent role of the United States in relationship to the politics of the kingdom.

In the second part, M. Duffar examines three categories of political forces that he perceives as possibly undermining the establishment. Of these three, the most important, he feels, is the divisiveness and dissidence of minority groups. He concentrates on the insurrectionary groups on the periphery but does not neglect the urban-based Chinese.

The other two categories are examined in the book's most novel chapters—one on "The Economic Menace" and one on "Social Unrest." The author identifies the increasing tension between rapidly increasing population and not so rapidly expanding economy as a trend that clearly threatens the stability of the established powers. The threat has two prongs. One is the possibility that economic hardship will catalyze a reaction between the peripheral insurrection and the bulk of the rural population. The other is the enormous pressure generated on the coherence and respectability of the political class by economic change, extraordinary urban growth (with its attendant corruption, crime and speculation), and, particularly, the appearance of large numbers of discontented and troubled youths (students and otherwise).

Although M. Duffar does not predict the current disarray and perhaps dissolution of the long-standing military core of Thailand's politics precipitated by student demonstrations in 1973, he was able to perceive the possibility some years before the event. Considering the present popularity of the mistaken notion that this student political activity was a complete novelty, M. Duffar's foresight and its evidentiary basis is notable. His more complex analysis—while too brief and not completely free of defects arising from a poor data base—is suggestive and important. His image of a confused force of social unrest, "still groping, consisting of an aggregation of elements, not yet formed and lacking self-consciousness" but with the potential for crystallization, indicates the real complexity of Thailand's politics.

This book is clearly badly needed for informing French readers. For those familiar with the English literature, its merit lies in the thoughtful, although sketchy, analysis.

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**Israel's Political-Military Doctrine: Occasional Papers in International Affairs Number 30.**

By Michael I. Handel. (Cambridge, Mass.: Harvard University—Center for International Affairs, July 1973, Pp. x, 101. \$3.00.)

"War is the national industry of Prussia," Mirabeau once said. According to graduate student associate at Harvard's Center for International Affairs Michael Handel, "this aphorism aptly describes Israel's situation; the questions of war and survival are the main preoccupations of the nation" (p. 69). After setting out the limitations of geography, demography, economic infrastructure, and political environment, the author traces Israel's military development from infantry based strategy to reliance on the most modern tanks, armor, and air force.

Published only three months before Arab-Israel war number 4 in October 1973, the monograph makes possible some timely insights into limitations of the country's political-military doctrine. Until the Yom Kippur War Israel's greatest strategic asset seemed to have been a capacity quickly to innovate and adapt to requirements of rapidly altering military situations. Israel's strategists, however, seemed to have lost some of the flexibility and adaptability with which Handel credits them. He states that IDF practice had been to assume "that the next war will and must be fought very differently from the last one," (p. 70), paying attention to negative lessons such as "what was not relevant for the next war." The political crisis that followed the October War indicated that the public was dissatisfied with performance of their military leaders and that lessons supposedly learned from the 1967 war were not adequately applied by 1973.

Although Handel writes respectfully of former Defense Minister Moshe Dayan's contributions, he is critical of the ex-general's "little understanding of armored warfare" (p. 26) during the 1950s and his lack of creativity and imagination in the conduct of reprisal raids (p. 35). Perhaps a harbinger of Dayan's recent political difficulties was the debate within the Israel Defense Forces (IDF) after 1967 about whether to erect defensive lines against Egypt along the eastern bank of the Suez Canal. The resulting "Bar Lev Line" was supposed to be a forward position manned by few soldiers with the main defense assigned to mobile armored units. "Many senior officers argued that it made little military sense to put the first line under the superior artillery firepower of the Egyptians, and that a better solution would be to build the same first line of defense along the mountain ridge some fifteen miles east of the canal" (p. 51).

Analysis of the country's military doctrine is compact, intelligible and comprehensive. But the account of Israel's political perspectives indicates that the country's leaders have been so preoccupied with immediate security requirements that they have failed to realize the long term implications of war as the national industry. The "basic assumption" of the whole political-military doctrine is that "the central aim of Arab countries is to destroy the state of Israel whenever they feel able to do so" (p. 64). While peace is desired, security is more important, the author writes.

It must be obvious, even if these basic assumptions are correct, that an efficient military machine, if necessary, is far from sufficient to assure the country's longevity. None of the other components of the doctrine presented including alliance with a strong power, developing capacity to produce its own weapons and to become self sufficient, large scale immigration of and support from world Jewry, or continued preemptive strikes against potential Arab attacks, can guarantee the country's security indefinitely. Although the gap in economic and social development with its Arab neighbors may continue to widen, thus assuring the Jewish State another few decades of military supremacy, one wonders whether a nation in Israel's perilous circumstances can afford to place such confidence in the perpetual weakness of hostile neighbors. The present political-military doctrine is shortsighted in giving so little thought to the volatile shifting of international alliances, the decline and fall of empires, the sudden ascent and descent of superpowers. In a world of such rapidly changing power factors no small nation can rely on its own power to survive in isolation. Even development of nuclear arms is not reassuring, for as Handel states, "There is little comfort in retaliating after one's country is already destroyed by one or two nuclear bombs" (pp. 48-49). Many Israeli leaders insist that they have no alternative, (*ain breira*) to the doctrine so well described by Handel. Others insist that if it is worth surviving at all, alternatives must be found which diminish dependence on military and accentuate the development of political components in national security.

DON PERETZ

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**The Communist Tide in Latin America: A Selected Treatment.** Edited by Donald L. Herman. (Austin: The University of Texas Press, 1973. Pp. 215. \$7.50.)

This collection of four essays and an introduction is mistitled. "Communist Outposts and Sallies in Latin America" would be a better

description of its contents. Robert J. Alexander's "Impact of the Sino-Soviet Split on Latin American Communism" offers the reader more than the title promises. Alexander draws on an extensive and intimate knowledge of leftist politics in Latin America to provide a cogent explanation of developments which would have eluded him if he had confined himself to communist doctrinal formulations. For example, he makes much of the factor of generational political attitudes in explaining Venezuelan events. He sees the major split in the Latin American left as between the Jacobin Left exemplified by Castro and the other forces on the left including the Communists. Alexander, who first applied the term Jacobin to this movement, understands it as a nationalistic, xenophobic, and above all activist movement drawing its shifting membership from high-school and university students and from young professionals. In Latin American social conditions such a group is inevitably drawn from the middle and upper classes. In Venezuela these young people loosely organized in the MIR (The Left Revolutionary Movement) exercised a fateful and fatal attraction on the Communist Party. The Communist Youth, who had more in common with their contemporaries in the MIR than with the aged leadership of their own party, foisted a policy of armed resistance on the party. Armed resistance consisted mainly of urban terrorism whose chief victims were the working classes—theoretically the mass base for the Communist party. By 1963, Alexander tells us, revolutionary activity had aroused so much distaste in the population that Betancourt was able to deprive the revolutionary members of the Congress of their parliamentary immunity and go on to win a smashing victory at the polls in which almost 95 per cent of the electorate voted. The Jacobin Left was the victim of its own misreading of the Cuban revolution. Violence had succeeded in Cuba, as Guevara pointed out in his early writings on the guerrilla movement, because the Batista government had bankrupted itself, and the Cuban middle classes and to a lesser extent the Cuban workers were sympathetic to Castro. This single episode exemplifies the cogency of Alexander's analyses, based as they are on an intimate understanding of the forces of Latin American politics rather than on a stereotype of how Communist parties behave.

J. Gregory Oswald's "Introduction to USSR Relations with Mexico, Uruguay, and Cuba" is descriptive and traverses familiar territory, but Donald L. Herman's "The Left Wing and the Communists in Mexico" deserves more extended comment: it deals with the influence of

the Communists in the Cardenas administration of 1934-40. Herman's purpose is to examine the proposition that "dynamic liberal movements, or native social revolutionary movements, under the guidance of democratic left wing political parties, can offer the strongest resistance to the influence of Communism." He finds that theory inapplicable in this case study because the Mexican Communist party grew from 17,000 in 1935 to 33,000 in 1939, according to Communist statistics. Communists were able to occupy the highest positions in the Mexican Confederation of Workers, a popular-front organization which the Communists were instrumental in creating. The Communists in the Secretariat of Education were able to produce and distribute popular textbooks and pamphlets which explained the class struggle in Marxist terms. But the reader wonders if all these unquestionable facts do not prove the opposite of what Herman intends, suggesting instead that Cardenas exploited the Communists for his own purposes, using them to bring the labor movement firmly under government control. It was an unequal struggle. As the extensive literature on the subject makes clear, for all its disappointments, the Mexican Revolution had earned the respect and affection of large elements of the population. Its army was under the control of revolutionary generals; it presided over the industrialization and the urbanization of the country and during Cardenas's administration it established the precedent for all Latin America of the successful nationalization of foreign owned property. Against these solid achievements the Marxist pamphlets of the Ministry of Education weighed only slightly in the balance. Latin American political elites have always been hospitable to foreign ideologies, but the promiscuity of the hospitality suggests an underlying confidence that verbal formulas are only the outer garments of the real body of power. To change the metaphor, communism, rather than being a tide which engulfs the landscape, seems to be a rivulet which runs dry in the sands of Latin American politics.

The obvious exception to this generalization is the adoption of communism by the Castro regime, which is probably more readily explained by an examination of Cuban, Soviet, and U.S. politics in 1961 and 1962 rather than by the strength of the Communist tide.

John W. F. Dulles's "The Brazilian Left: Efforts at Recovery, 1964-1970" is of particular interest because Dulles is able to document from captured diaries and notes the irresponsibility and stupidity of the Brazilian communists and the fecklessness of Chinese and

Cuban agents. One often suspects that Communist revolutionary leaders are endowed with neither common sense nor a firm grasp of reality, but extensive documentation for that suspicion is rare. Perhaps, however, one should not judge the Brazilian communist leadership too harshly because their situation was basically unimprovable. A Brazilian poll showed that the highest percentage of those who feared communism (81 per cent) were illiterates, and the lowest (41 per cent) were university graduates. The lower classes of society feared communism, and the educated who feared it less were more susceptible to the appeals of the right wing in Brazil. In our distaste for repressive regimes we often forget that they can promote dramatic economic expansion and concomitant social mobility; the chief beneficiaries of this process are the educated, who in conditions of stagnation find radical solutions and conspiratorial life attractive. When careers are open to talents and savage repression is visited on those who spurn the offer, Communist parties fare badly.

*The Communist Tide in Latin America* demonstrates once more that to understand Communist movements in Latin America it is more useful to look through the end of the telescope marked "the political process in Latin America" than the end marked "what Moscow wants."

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**From Village to State in Tanzania: The Politics of Rural Development.** By Clyde R. Ingle. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 279. \$12.50.)

Ingle's book is a good analysis of the politics of rural development in Tanzania, an African country many observers feel is one of the continent's most intriguing political experiments. The Tanzanian emphasis on the development of the rural sector is unique in Africa. Other countries pay lip service to the importance of the rural sector, but only Tanzania—in a continent where eighty per cent of the population are incredibly poor subsistence farmers—appears to place development dollars alongside rhetoric. Professor Ingle is interested in the relationship between the ideals of the Arusha Declaration, which set forth Tanzania's goals as well as the methods of implementing them, and the behavior of peasants in rural areas. Like others trained at Syracuse, he is concerned with linkage. What makes his treatment of the Tanzanian material different is that he does not stop with juxtaposing ideology at the top and practice at the bottom.

Ingle's theoretical approach is based on systems analysis. But rather than take the more common assumption of those concerned with the state and ask how the state maintains itself under stress, Ingle suggests that in developing countries "systems lower than the national may also develop to counter stresses and demands upon them, the principal source of which may be the national system itself. In so doing they may jeopardize the development of the longer systems of which they are a part" (p. 19–20). Ingle puts forward three propositions which elaborate the foregoing position and guide his inquiry. He suggests that "within any national society the political functions of that society are shared by a series of political systems," that "political actors occupy political roles in the several political systems in the same fashion as they simultaneously occupy several roles within social systems," and finally that "an input-output exchange occurs between the several political systems in a national setting" (p. 23).

Ingle takes us to the local level of Tanzania politics—to the level where both the Tanzanophiles (to use Ali Mazrui's word) and the regime's sharpest critics say the action is, but where previous authors have not taken us. In *From Village to State in Tanzania* the author is able to show the interface between village and state in a way that other commentators on Tanzania have not done. Liebenow's *Colonial Rule and Political Development in Tanzania* (Evanston: Northwestern University Press, 1970) for example is limited to a much earlier time period. Bienen's *Tanzania* (Princeton: Princeton University Press, 1970) focuses on the national level. Hyden's *TANU Yajenga Nchi* (Lund: Uniskol, 1968) lacks theoretical underpinnings and a synthesis of observations.

There is a good chapter on rural development during the German and British colonial periods, followed by a very insightful chapter on the environment of development in its physical and cultural dimensions. Ingle is cognizant of how political culture conditions development efforts. He discusses witchcraft, paternalism, and compulsion as they relate to rural development. He does not take the "oh-aren't-they-unusual" tone but instead offers realistic appraisal of how cultural milieu constrains efforts at change.

Ingle's analysis of district-level politics and their relationships up to the national level and down to the village level provide a penetrating look at the dynamics of rural politics. The author shows how individuals and institutions react when intermediate political systems attempt to implement policies which require

changes in behavior that people are neither enthusiastic about nor willing to impose openly. He suggests that accommodation to demands is the most frequent and minimal response. When greater coercion or will are present, acquiescence is the response. Withdrawal is yet a third possible response to attempts to change behavior. Ingle concludes his assessment of rural politics by recognizing the paradox facing Tanzania's leadership. They took the revolutionary step of decentralizing political authority in order to assess national-local conflicts while at the same time tightening the national control over the extraction of resources. His prediction for Tanzania is cautiously optimistic.

This book should help to satisfy the continuing need of many who teach courses in African politics, in comparative politics in developing areas, and in ideology, for a good middle-range theoretical treatment of the issues raised by experience in a single country.

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**Political Development: A General Theory and a Latin American Case Study.** By Helio Jaguaribe. (New York: Harper & Row, Publishers, 1973. Pp. 603. \$11.95.)

This book is fundamentally a product of Professor Jaguaribe's long-time interest in the drama of development, and of his concern for the special role of intellectuals in it. It proceeds from the belief that political science now possesses the tools to fashion an "objectively optimal, or at least particularly fitting, recipe for the promotion . . . of the political and overall development" of a national society (p. 2). This is a book meant to be "policy relevant" at the grandest level.

Its design reflects the scope of that ambition. The first two "books" of the massive tome are essentially critical surveys of existing theory in political science, out of which Jaguaribe delineates his own views. The first is intended to establish the distinctive functions of the political in human society, as the foundation for discussing, in the second, what would constitute political "development." The third and concluding "book" is a case study of Latin America, which is to demonstrate the utility of the broader scheme.

Thus conceived, the work claims originality—but very self-consciously from within a tradition. Jaguaribe shows an Encyclopedist's enthusiasm for the advances of political science in the last decades. He marshals 2500 years of theorists (40 pages of bibliography, in seven languages) to be codified, with some casuistry, within systems-functional categories. In the

manner of comparative politics in the '60s, classification abounds. There are literally 100 pages of charts, tables, typologies, and schemata—laid out with a sense of excitement at the accumulation of human knowledge. The result, at this level, is at the least an analytical and often provocative overview of theorists who have profoundly shaped the discipline as practiced—from Parsons, Easton, and Apter, to Huntington, Eisenstadt, and Almond.

Beyond this labor of conglomeration and clarification, Jaguaribe has enriched the concept of "political development" with several distinctive elements of his own. Like earlier theorists, he strongly believes that development is an enterprise of the nation-state. Unlike them, he does not believe it can be understood (or fostered) with the implicit assumption of national autonomy. (That, in some form, is the goal, not the starting point.) He attempts systematically to include international factors, ranging from geopolitical considerations (e.g. "national viability") to economic and cultural influences (from varied kinds of "imperialism" and "dependency"). He quite defensibly devotes a large portion of his study of Latin America to an analysis of the United States, both in its internal development and its external behavior (with George Liska given special prominence). His analytical tools are vague about mechanisms and processes, and are certainly less powerful than he believes (he is one among many who failed to anticipate the "structural" shift of worldwide primary-product inflation). But he does persuade us of his central point, that comparative politics must include study of external influences and interactions.

Another distinctive emphasis, very much in keeping with his larger "policy" purposes, is his insistence on the inevitable intermingling in development of the economic and the political. (Here and more generally Jaguaribe's concerns echo the earlier work of such different scholars as Barrington Moore and Charles W. Anderson, neither of whom, curiously, he seems to have read.) "Political development," if it occurs at all, is inescapably shaped both by economic demands and interests and by the strategies and goals adopted by elites to achieve economic development. It is for the latter that Jaguaribe, building on his own earlier work, elaborates three basic "models" of political and economic development—national capitalism, state capitalism, and developmental socialism. Each of them constitutes a path consciously to be chosen by national elites—considering their own case within the scheme of conditions, categories,

processes, and parameters that Jaguaribe has laid down.

He cannot be said really to have succeeded in this task, his aspiration for policy relevance. The reasons for his failure, however, owe not so much to his own shortcomings as to those of his school. The implicit determinism built into doggedly "structural" thinking leaves no room for politics. Here Jaguaribe's key section on elites, which potentially could link structural and choice perspectives, is a great disappointment. It is not very helpful to invoke "political genius" for developmental success; nor to explain failure in terms of a lack of "men of high quality," or of "sufficient support" or of "appropriate leadership." Jaguaribe does not much tell us how leadership which is "semi-functional," "non-functional," or "dysfunctional" can become "functional" to his "objective" model-recipes (pp. 264ff). (A more systematic development of his many interesting asides about intellectuals would be a good start.) In short, he gives us little insight into what leaders do with what they have, after they know what they face.

But even to know that—the resources and constraints for development in a given case—is ultimately nothing for which Jaguaribe's elaborate machinery is of much use. His case studies of Latin America are thin and descriptive, lacking in comparative rigor and persuasive detail. Where they are illuminating, their insights often have little to do with the larger conceptual scheme (as, for example, on the dynamics of military regimes). Jaguaribe suffers here from the same problem as earlier scholars (e.g., Black and Chalmers Johnson): however useful their classificatory grids to frame paradigms for display, they don't tell us much about *particular cases*. However subtle and sophisticated their conceptual distinctions, we never seem to be able to operationalize key terms or specify parameters well enough to make them relevant to ongoing policy or politics. Historically, the greatest impulses to development seem to have come not from cross-sectional comparison but from longitudinal reflection—from the effort, often idiosyncratic and parochial, to grapple with a nation's own past.

Jaguaribe's *Political Development* is rather like Neuschwanstein, the grand, anachronistic castle of Ludwig II of Bavaria—a monument to the style, strengths, weaknesses, and excesses of an idea and its times. It is also a "transitional" work (to borrow a phrase Huntington applied to himself some years ago). Scattered through its pages are concepts

such as "the state," "corporatism," "regime," "authoritarian," "dependency," "intelligentsia"—none of which were prominent in the comparative theory of the recent past. They seem to arise from politics as it is experienced and understood in the area Jaguaribe knows best (one which had little impact on comparativists earlier). Ironically, they appear now among the fruits of that unprecedented outpouring of research abroad which was so much stimulated by the older grand theories. As such they can perhaps be taken as signs of a movement already underway to work our way back from our greater knowledge toward new theory, perhaps more modest, hopefully more useful, certainly more solidly based.

ALEXANDER W. WILDE

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**The Transformation of Communist Ideology: The Yugoslav Case, 1945–1953.** By A. Ross Johnson. (Cambridge, Mass.: The MIT Press, 1972. Pp. viii, 269. \$15.00.)

In addition to having written a useful book on Yugoslav Communist ideology, Ross Johnson is to be congratulated for paying attention to the importance of Communist ideology generally.

Ideology is seldom a blueprint for actions of Communist Party states, but it is an indispensable background for understanding what they do and do not do. Failure to realize this is one reason why Western commentators on communism are so often wrong. The American reaction to Yugoslavia's dispute with Moscow—although Professor Johnson does not refer to it—is a good illustration. Once the rift was taken seriously here, there was a tendency on the one hand to expect Yugoslavia to join the United States in its Cold War stance against the U.S.S.R.—e.g., by affiliating with NATO—and on the other hand to fret about Tito "going back" to the Soviet Union. There was no likelihood whatsoever of either of these courses of action, as any competent student of Yugoslav ideology could have testified (and some did).

The emergence of a peculiar Yugoslav version of Communist ideology has been one of the most significant developments in Marxism since the Bolshevik Revolution. This book reviews the Yugoslav ideological positions as they existed before the 1948 break with Moscow, traces their evolution in following years and analyzes the factors, domestic and foreign, which led to the dramatic changes. In so doing, the author makes clear the role of ideology in Communist party states generally and in Yugoslavia in particular.

It is no criticism of the author to point out that he has encountered the problem which has confronted virtually every scholarly writer on Yugoslavia in the past 20 years: the constantly changing political and economic kaleidoscope of the country tends to make all publications somewhat dated. The history of Yugoslavia in this period has been like a pendulum, especially—though not only—as concerns the role of the Communist Party. Calls for democratization were followed by demands for tighter controls, which in turn were superseded by more decentralization. Dr. Johnson quite rightly indicates that only after the purge of Ranković in 1966 was there really an effective departure from the idea of democratic centralism, which the Sixth Party Congress in 1952 seemed to herald. But today, 22 years after the Sixth Congress and eight years after the Ranković affair, this is still—along with the nationalities problem—the hottest issue in Yugoslavia. Tito has called for a revision of the 1952 Party Congress and has purged the “best and the brightest” of the younger Party leaders in the republics, who resist it. Whether he can succeed, and if he does whether success can be compatible with “Titoism” as it has developed up to now, are questions which involve the future not only of the system but, perhaps, of the country itself.

In addition, there is now still another constitution—the fourth since 1946—with further new concepts, especially the “delegate system” of representation which, it is now claimed, is “basic” to the Yugoslav idea of “withering away” of the state. None of these developments, of course, gainsay the essence of what Dr. Johnson has written, although they place certain aspects in a somewhat different perspective.

The lacuna of discussion about the relationship between “withering away” of the Party and “withering away” of the state is the Yugoslavs', not the author's. Some might feel, however, that the Yugoslav doctrine of “withering away of the state” deserves more elaborate treatment than given in the book, especially since it involves one of the central themes of Marxism, surplus value. The Yugoslav position, first advanced publicly by Djilas before his fall from grace, is that surplus value inheres in socialism as well as in capitalism. The essential question is not who owns the means of production but who controls the surplus profit. The Yugoslav answer to this question was worker management—later expanded to “social self-management” generally—and this in turn required dismantling of the state apparatus. Thus, as Edvard Kardelj

formulated the concept, a state is not really socialist unless it is “in the process of withering away.”

If Dr. Johnson, whose use of original sources is prodigious, had referred to what I, at least, regard as the most authoritative Yugoslav ideological work in the period preceding the Sixth Congress—Kardelj's *Socijalistička Demokracija*—these matters might have been covered.

One of Johnson's most innovative points is an emphasis on differences between the Soviet and the Yugoslav conceptions of “people's democracy” prior to 1948. In my opinion, however, the situation was more complex than he indicates. There certainly were differences, but the Yugoslavs did not then recognize them. The differences were objective, stemming from the nature of the Yugoslav revolution. Prior to the 1950s, there was no formal Yugoslav party doctrine, *qua* doctrine, on this subject. The initial Yugoslav approach to socialism was marked more by enthusiasm and confusion than doctrinal logic. Yugoslavia was then predominantly a peasant country, and the Yugoslav Communists—backed by the broadly based Partisan movement—perforce had to rely heavily on peasant support. This accounted in part for the early nonrevolutionary treatment of the peasants. On the other hand, the Yugoslav five-year plan was based on the idea they could build socialism immediately. From the viewpoint of Soviet Communism, Stalin was quite correct in looking askance at this approach. Wholly aside from the ideological questions it raised, practically it had no chance of success—at least without vast aid from the USSR, which was unavailable even if Moscow had desired to grant it. Then, in late 1947, the Soviet conception changed, calling for a more doctrinaire approach toward the countryside, i.e., rapid forced collectivization. This was exactly what the Yugoslav Communists could not do, because it would have undercut much of their support.

Alienation of the peasants was not a major issue for the other Eastern European regimes; however, because, unlike Yugoslavia, the Red Army was there to keep things in hand. How confused the Yugoslav Communists were about all this is seen by the fact that *after* the break with the Cominform they belatedly tried to enforce Soviet-style collectivization, with near-disastrous results. Not all Soviet criticism of Yugoslavia was without ideological foundation.

Over-all, the Johnson book is a first-rate scholarly contribution, both objective and



knowledgeable, with many original insights. It throws light not only on Yugoslav developments and Communist ideology but also on the whole relationship of theory and political action.

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**The Politics of Ceylon (Sri Lanka).** By Robert N. Kearney. (Ithaca, N.Y.: Cornell University Press, 1973. Pp. 249. \$13.50.)

Robert Kearney's *The Politics of Ceylon (Sri Lanka)* is a welcome addition to the growing bibliography on Ceylonese government and politics since Independence. In the series on South Asian Political Systems edited by Richard Park, it mercifully spares us the neologisms and esoteric conceptualizations that were fashionable a few years ago. Professor Kearney makes an orderly argument, his language is straightforward, and he brings to his study detailed knowledge from a number of research stays on the island. In compact compass, the book represents a first-rate introduction to the political system as we conventionally define it.

The author's implicit political model stresses the articulation of demands and their aggregation through political parties which compete in elections to place their principals into highest office. For most of the book, as for Ceylonese politics itself, short-run political power is the name of the game. The last chapter, however, does consider "Challenges to the Political System" generated within the system itself and by the stagnating economy.

A brief discussion of Ceylon's society and history is followed by a description of the structure and process of government, focusing on the legislature, the Cabinet, the public service and administration, and on local government. The contrasting and parallel orientations, structures and memberships of the political parties seeking to survive and win in a competitive pluralistic setting are analyzed with sensitivity. Kearney's descriptions of elections, voting, and campaigning evoke the atmosphere and process of Ceylon's elections which have so well institutionalized government changes that no government since 1956 has yet succeeded itself. Each of these parts of the political system is neatly characterized, its evolution traced and some of its typical processes identified.

Chapter 5 lays greater stress on the more uniquely Ceylonese characteristics of the system. Devoted to "Communal Loyalties and Groups in Politics," it illuminates the underlying religious, linguistic, and caste identifica-

tions which still form the substructure of all political behavior. It is from these political elements that leaders put together their winning (or losing) coalitions to contest elections. For readers new to South Asian politics, this chapter is a good introduction to the social springs of political action. One wishes the fascinating materials in the chapter had been used from the outset to show the reader the very foundations on which the system thus far has rested, rather than appearing only after elections have been discussed. But the discussion itself is systematic, balanced and illuminating.

Thus far, the study is generally up-beat. Kearney shows (a) how the system induces the politically ambitious to respond to mass aspirations, at least in the Sinhalese community; (b) how it promotes vertical integration as the rural mass Sinhalese voter affects the way the Sinhalese political elite lives, acts and allocates; and (c) how it regularly brings down incumbents, to give others a chance to rule. He believes that voting by interest is increasing while more traditional voting by deference and patron-client relationships is declining. The changing composition of Parliament and the public services shows that even though the two clan-like families, the Senanayakes and Bandaranaiques, have monopolized the prime ministerial position since Independence, substantial social changes within the Sinhalese community are reflected in changes in the personnel of successive cabinets and other positions in the political and governmental system. He also notes the increase of direct political influence on the bureaucracy and the decreasing opportunities open to the hitherto much more prominent Tamil community.

Chapter 6, by contrast, elaborates the challenges besetting the system, including the problem of coercive protest, the political violence of the youth rebellion in 1971 and the near-stagnation of the economy. Alas, the latter problem has grown increasingly serious since terms of trade began to slip in the mid-1950s. Terms of trade have deteriorated more regularly than for most plantation economies, and the economy has shown little capacity to diversify or to concentrate on new productive activities. These handicaps make it increasingly difficult for the government to meet the demands that are intensified by the competitive political system itself. Each successive government's efforts to do so has reduced the resources available for innovative investment designed to diversify the economy and to lay the base for new sectors of economic growth.

While it is gratuitous for a reviewer to

sketch the book he wishes the author had written, this study illustrates a wider problem in our comparative politics literature. Too often fascination with competitive group interests and the growth of political ideas and institutions leads us to ignore underlying economic trends. In Sri Lanka, growing economic cramp has been quite as crucial a limiting factor in the political process as the important forces described in Chapter 1 on Ceylon's history and Chapter 6 on its communal and ethnic structure. Significantly, the economic conditions are both "input" to and "output" from the political system as it has been functioning, representing an instance of cumulative circular causation in Myrdal's phrase. Competitive politics have virtually forced extensive distributive policies, which have clearly impeded either private or public productive investment. With a rapidly growing population this in turn perpetuates and intensifies economic cramp. Accordingly, the nexus between political activity and economic performance in countries like Ceylon would seem to justify more than the ten pages that Kearney devotes to it. Perhaps future studies in this useful series will pay more attention than their predecessors to the economic setting of politics, the economic consequences of political processes and the long-run political effects of persisting economic stagnation in the face of growing population and rising expectations.

But Kearney's volume is a valuable and shapely introduction to the politics of Sri Lanka. It should be particularly useful in introductory courses in South Asian or Comparative Politics.

W. HOWARD WRIGGINS

*Columbia University*

**Modernization, Dislocation, and Aprismo: Origins of the Peruvian Aprista Party, 1870-1932.** By Peter F. Klaren. (Austin: The University of Texas Press, 1973. Pp. 189. \$8.95.)

This compact monograph by a historian makes interesting reading for political scientists. Not only is it an impressively researched, well-written interpretation of the origins of Latin America's most durable indigenous reform party, but the author frames his analysis in terms of the dual processes of socio-economic modernization and political development. In doing so he helps clear up certain anomalies of Peruvian politics while at the same time deepening our understanding of the political implications of modernization. Why did the Aprista party emerge in Peru and how does it manage to persist? In dealing with this question, Professor Klaren gives us new perspective on the Peruvian military, which took

a reformist tack in 1968. While his use of social science techniques is quite limited, Klaren explicitly approaches the specifics of the Peruvian case in such a manner as to give the reader new insight into the dynamics and causal interplay of modernization and development.

The Peruvian Aprista party, or Apra, founded and led to this day by Victor Raul Haya de la Torre, has been a major power contender in Peru and inspiration for the generation of moderate reform parties that sprang up throughout Latin America in the 1930s and 1940s. (In fact they are sometimes referred to as the Aprista parties.) Explanations for the rise and political survival of Apra have generally focused on the party's dynamic, charismatic leader. While Klaren recognizes Haya's considerable talents and magnetic personality, he searches deeper for reasons behind Peruvian susceptibility to Apra's reformist appeal.

The party arose and has always found its primary base of support on Peru's north coast. When we look at the north we find that it differs economically from the rest of the country. Since the latter part of the last century, it has been under the domination of large, highly efficient plantations that produce cane sugar for export. Klaren traces the origins of Apra to this geoeconomic situation. "Haya, along with other fellow northerners, became the political spokesman for various segments of the population in that area who in one way or another were dislocated and frustrated by the rapid break-up of the traditional society" (p. xv). Generally speaking, the process of modernization during this period was confined to the northern sugar area, but in the other parts of Peru where it also occurred, Apra likewise built a base of support according to the book.

To test this geoeconomic hypothesis, Klaren carefully reconstructs the socioeconomic and political histories of two important sugar-producing valleys near the city of Trujillo. With an impressive array of data, the study documents the fascinating process which ultimately thrust the Aprista party on to the national scene. Beginning in the 1870s outside interests supported by foreign capital started to buy up land for the cultivation of sugar. By 1927 there were three huge, modern plantations where once there had been 60 traditional haciendas. This modernization dislocated several groups that eventually formed the core of Apra. First, the labor requirements of the sugar plantations created a rural proletariat composed of Indians from the highlands who quickly fell victim to a vicious debt-peonage

system. Labor unrest became commonplace in the valleys early in this century. Second, the modernization of sugar production wiped out small farmers and sharecroppers, forcing them to work on the plantations. Third, the sugar boom, which first stimulated local commerce, subsequently led to its demise as the large estates vertically integrated. Finally, the local intelligentsia, led by Haya de la Torre—whom the author pictures as something of a clever opportunist—joined the cause.

In 1931 Haya returned from exile in Europe (the exile period is not particularly well integrated into the author's analysis) to assume leadership of the diverse coalition of disaffected northerners and to contest a hastily scheduled presidential election. The Apristas sought to carry their message, radical for its day, to all Peru, but their support, which came primarily from the north, was not enough to win the presidency for Haya. Frustrated by electoral failure, the Apristas turned to violence which brought them into conflict with the armed forces in the Trujillo revolt of 1932. This confrontation poisoned relations between the Apristas and the military, although in 1968 the armed forces would appear to have adopted the party's appeal to those dislocated by modernization without having accepted Apra itself.

Peter Klaren's study of the origins of the Aprista party joins a growing body of literature on Peru that focuses on the impact of economic change at the local level. The author provides only a partial test of his geoeconomic thesis since he examined only one area in Peru. He does, however, mention similar developments in other Latin American countries in the conclusion. In fact the book cries out for cross-sectional, cross-national, and longitudinal analysis. This observation should not be taken as a criticism, but on the contrary, as an enthusiastic response to Klaren's effort to refine our understanding of modernization and development.

TERRY L. MCCOY

*The Ohio State University*

**Legislatures in Comparative Perspective.** Edited by Allan Kornberg. (New York: David McKay Co., Inc., 1973. Pp. 457. \$6.95, paper.)

**Comparative Legislative Behavior: Frontiers of Research.** Edited by Samuel C. Patterson and John C. Wahlke. (New York: John Wiley & Sons, Inc., 1972. Pp. 311. \$14.95.)

A thorough reading of the two volumes under review should make it quite clear to

even the most sympathetic reviewer that the comparative study of legislative behavior and institutions is still in its prehistory, still a collection of interesting but unrelated research efforts in desperate need of an integrative theory. Veteran observers of the comparative art will note more than a touch of irony here. The movement of recent years away from whole-system comparative studies was fueled by a widespread consensus that theory was inadequately connected to empirical research, and that smaller-scale efforts would be more effective in eventually building up a theoretical and empirical base upon which grand theory could rest. It now seems apparent that the same problems have been transported to the subfield of comparative legislative behavior. In fact, the disparity between theory and empirical research seems more glaring than ever, because the concentration on specific institutions has facilitated narrow-gauge empirical studies without corresponding theoretical integration, to the point where the subfield seems in danger of splitting into a series of related sub-subfields. The twenty-seven essays in these two volumes could be scaled along three axes: *scope* (from internal structure of a single legislature to external interaction of several legislatures), *method* (from qualitative descriptive to quantitative data-oriented), and *area* (from developed, highly institutionalized to underdeveloped, weakly institutionalized). The interests of the authors range from the quaint practices of the nascent "legislatures" of the South Pacific to the career patterns of past justices of the Iowa Supreme Court, but the reader is left with the impossible task of assembling the pieces of this puzzle into some coherent pattern. I think many readers will feel (as I did) both admiration for some of the individual contributions and distress at the overall lack of an integrating conceptual analysis which would have given these efforts added meaning. Although a respectable number of the articles are interesting and perhaps even stimulating in themselves, they have simply no overall cumulative effect.

While the inherent and as yet insurmountable difficulties of the subject are responsible for much of this lack of focus, part of the problem seems to lie in the editors' failure to provide an integrating theme around which the contributions could be organized. Allan Kornberg attempted to organize his volume around the concept of "institutionalization" of legislatures (the theme of a conference from which these papers were drawn), but he ruefully notes that "only four of the [seventeen] papers can be said to focus explicitly on institutionalization although another half-dozen do give more than

passing consideration to the concept" (p. 1). Undoubtedly Professor Kornberg faced insuperable difficulties in trying to force his individualistic colleagues to lie down together on the procrustean bed of "institutionalization," but the result of this failure (for which no one who has ever tried to organize a panel could blame him!) is that the papers in his collection, by his own admission, "have not been given sufficient conceptual unity by a common focus" (p. 16). They represent dots on the widest possible landscape.

Perhaps most distressing, the more broadly analytical pieces seem to me the least successful contributions in the volume. Richard Sisson's "theoretical exploration" of legislative institutionalization is largely devoted to substituting idiosyncratic jargon for common used (and generally clearer) terminology. Is it of any possible help to anyone interested in legislative autonomy to dream up a phrase like "institutional/generic means of environmental control" to mean nothing more than the constitutional rights, either traditional or legal, of the "relevant unit" to manage its affairs and to control the allocation of values? Undoubtedly, further clarification and definition of terms is vitally necessary to this field—for example, the very term "legislature" seems inappropriate to many of the institutions discussed in this collection—but Sisson does not provide any conceptual clarification or operationally useful propositions through his manipulation of words. Similarly, Fred Riggs's loosely organized paper, aptly titled "Some Thoughts on Elected National Assemblies" becomes largely an exercise in word-play and ratiocination. For example, from the simple notion that legislatures can be described according to the degree of cohesion of their parties, Riggs first defines the two extremes of "disciplined" and "fluid" assemblies, then adds an intermediate category of "restrained" assemblies, and then follows this with an absurd ninefold permutation of these categories (i.e., fluid, fluid-fluid-restrained, fluid-restrained, etc.). Furthermore, Riggs's attempt to use precise language (to "avoid using neologisms") leads to some incredibly complicated statements that conceal rather commonplace thoughts, such as: "The balanced elected assemblies to be found in assembly-oriented (parliamentary) constitutive systems in open balanced polities are likely to be more disciplined than those found in not-assembly-oriented (presidential) constitutive systems in open balanced polities" (p. 68). I remain unconvinced that this is a genuine advance in theoretical clarity or precision. The logically distinct categories which Riggs de-

vises seem virtually impossible to apply to empirical research, and he devotes too much time to inventing new nomenclature (such as, for example, "monotonic" and "isotonic" systems) which can be more of a hindrance than a help to conceptualization of the subject.

A good and healthy contrast to these forays into "pure" theory is the modestly conceived paper by Peter Gerlich, in which he gathers and analyzes historical data in order to infer some interesting tentative explanations for the differing rates of institutionalization of European parliaments. Of the remaining broadly comparative and conceptual pieces in the volume, the discussion by Malcolm E. Jewell ("Linkages Between Legislative Parties and External Parties") is informed and judicious, but fails to organize the materials and observations into some reasonably systematic conclusions. The most satisfactory analytical paper in this collection is provided by Andrew J. Milnor and Mark N. Franklin, who provide an interesting taxonomy of forms and contexts of opposition, enlarging the old and limited constitutional definitions of oppositional activity into the wider arena of modern politics, and relating oppositional activity to the type of issue involved.

The most impressive and stimulating articles in the volume, however, are those that build (or test) theoretical propositions on a solid data base. In this category, the essay by Samuel C. Patterson, John C. Wahlke, and G. Robert Boynton represents a most admirable achievement. Starting from an exceptionally clear and thoughtful discussion of the various functions of representative bodies, the authors decide to examine the function of generating "diffuse" and "specific" support for legislative institutions. They utilize data from a survey of the public, party leaders, lobbyists, and legislators in Iowa and expand this with less complete data from thirteen other American states, and finally with some sketchy but suggestive survey data from Britain, Germany, and France. The result is a highly successful blending of theory and empirical methodology. Also of interest in this category are the preliminary report by Richard I. Hofferbert and Ira Sharkansky of an ambitious project to relate socioeconomic factors and political attributes of subnational political units at very different stages of modernization; and the paper by Allan Kornberg et al. concerning parliamentary recruitment in Canada.

Several quantitative papers in the collection neatly demonstrate conceptual pitfalls in the organization of the research design and thus essentially fail to show relationships at the

usually acceptable levels of significance. John G. Grumm's attempt to operationalize Easton's input-output model at the simplest level is innovative and potentially useful, but the simplifying assumptions result in the use of indicators that are not sufficiently refined to tap the complexity of the input structure of these systems. The article by William Mishler et al. attempts to determine the extent of freshman cue-taking from older members of the House of Representatives through multiple regression of voting by the two groups—a weak, indirect, and ultimately inconclusive (but undoubtedly more convenient) method when compared with the alternative of collecting interview data. It should be pointed out that stronger correlations in this case would not have demonstrated that cue-taking is the correct causal inference. Another innovative paper by Chong Lim Kim on "Consensus on Legislative Roles Among Japanese Prefectural Assemblymen" points the way toward possibly productive future research but uses indicators that are far too crude to elucidate the questions that the author asks.

The volume edited by Samuel C. Patterson and John C. Wahlke is apparently intended to represent the "frontiers of research," and perhaps for that reason the authors are somewhat more self-conscious about methodological questions. Gerhard Loewenberg provides a very thoughtful and readable critical overview of the field. Unfortunately, the defects that he notes in his essay are demonstrated in the remainder of the book. Professor Loewenberg points to the "inadequacy of theory to guide legislative behavior research" (p. 16), but the book does not contain a single attempt to develop such theory; he warns that the choice of problem has too often been determined by the "desire to employ particular analytical techniques rather than to solve problems of theoretical relevance" (p. 19), a phenomenon which seems to me well demonstrated in the article by Hans Daalder and Jerrold G. Rusk on "Perceptions of Party in the Dutch Parliament." This article, especially in the beginning portions, does provide some interesting information and observations on the perceptions of Dutch parliamentarians, but as the article progresses, the authors become increasingly preoccupied with measurement techniques, devoting the last half of their paper to a prolonged discussion of the various attributes of the multidimensional scaling techniques developed by Kruskal and Coombes. While the authors' adaptation of the data to their statistical methods seems to me highly questionable, the main point is that the primary focus of the study has by the end

shifted from comparative legislative behavior to comparative statistical methods.

Indeed, the entire notion of comparative research is weakly represented in this volume. Of the eight research papers (excluding overviews) in the collection, only one is cross-national in scope, and even that one (by Mogens N. Pederson, "Lawyers in Politics: The Danish Folketing and United States Legislatures") is unbalanced and unsystematic in presenting comparisons. What we have in this volume is a set of interesting, even exceptionally well-done studies of individual national legislatures or sets of subnational legislatures, asking different questions, using different methods, and finding different answers. A search for common themes or plausible interconnections among these studies proves almost totally frustrating. For example, the article by Heinz Eulau, based on a survey of 82 city councils in the San Francisco area is a superb piece of research and analysis. But what are the conclusions that one can legitimately draw from this study? Eulau convincingly demonstrates certain patterns of behavior and their relation to structural and situational factors, but have we learned something merely about San Francisco city councilmen, about small (mostly five-member) "legislative bodies," or about interaction in small decision-making groups in general? Is Eulau's analysis relevant to the study of large national legislatures? Of course, Professor Eulau can not be held responsible for this difficulty; but the point remains, in the words of the editors, that "in the United States experience, we simply have not dealt with the question of the idiosyncrasy of legislative institutions operating for different territorial units" (p. 293). Similarly, the study by John A. Brand of the attitudes of voters and councillors in three Scottish cities is an exceptionally well-conceived piece of survey research and analysis, but it may very well be irrelevant to other localities, especially in view of the author's conclusion that "each city appears to have its own cultural and institutional patterns that impose themselves on politicians and laymen in all parties . . . [and] which seem to stamp the relationships in every issue with a distinctive local character" (p. 265). Of the remaining articles in this volume, the one by Peter Gerlich on attitudes of Vienna city councilmen is informative and provides some rough comparisons with earlier American findings; the study by John E. Schwarz and Geoffrey Lambert of the voting behavior of British Conservative backbenchers attempts, through a questionable set of assumptions, to relate voting behavior to

the backbenchers' aspirations for higher appointment and to the marginality of their constituencies; and John G. Grumm's essay is an attempt to test the notion of feedback by using aggregate data (over a 15-year timespan) from the American state level—a serious conceptual mistake, since feedback, at least in Deutsch's and Easton's conceptions, was the resultant of specific outputs and could not be measured by the overall amount of governmental activity (in welfare expenditures or any other gross measure). Also included in the volume is a description of the parties in the Finnish parliament by Pertti Pesonen, and a survey of recent research by the editors.

JEROME M. GILSON

*The Johns Hopkins University*

**Dianying, Electric Shadows: An Account of Films and the Film Audience in China.** By Jay Leyda. (Cambridge, Mass.: The MIT Press, 1972. p. 515. \$12.50.)

In the midst of a spate of recent books and articles on China produced by "post-ping pong thirty-day wonders," one welcomes a book by one of the few Americans to have had the opportunity to live in China for an extended period during the years of Sino-American hostility. Jay Leyda lived in Peking from 1959 to 1964 and worked in the Film Archive of China (established in 1957), primarily helping to catalogue their collection of foreign films, but also providing his hosts with comments and advice on the many Chinese films, new and old, he had the opportunity to see over this period. These viewings of Chinese films, supplemented by Mr. Leyda's consultation of film archives in several other countries, form the primary basis of this book.

As a history this book provides the most comprehensive treatment available in English of the development of the Chinese film industry from the early years of the 20th century to 1967. Mr. Leyda's personal contacts and years in Peking also provide a fair amount of new biographical detail about the lives of Chinese film-makers and artists, and their struggles to produce effective films in the midst of turbulent political upheavals and pressures. Finally, the author offers useful critical commentary on films covering this entire two thirds of a century.

The result is often valuable as a general sourcebook on Chinese films, but a number of shortcomings lessen its usefulness as either a critical study or a treatment of how artistic and political institutions interact. Mr. Leyda has crammed his book full of all kinds of in-

formation, much of it scarcely related to his central themes. This is particularly annoying in the chapters dealing with the years he worked in China, onto which he has tacked personal memoirs, including diary notes, poems he scribbled to himself, and incomprehensible jottings about people he knew but we don't. Furthermore, his goal of producing a comprehensive history of Chinese films often gets in the way of his critical analysis. In many places we are given listings or short plot summaries of films which, unfortunately, Mr. Leyda didn't get a chance to see. Thus the reader interested in his critical evaluations of Chinese films has to wade through a good deal of chaff to find the films which are treated in enough detail to be very meaningful.

While my primary complaint is that this book is cluttered with too many divergent types of information—and here the lack of a forceful editorial hand must take much of the blame—there are some curious gaps. The book's subtitle tells us it is partly about the film audience in China, but in fact very little is said on this topic in the text (understandably, since Mr. Leyda saw most of his films in private screenings within the Film Archive of China offices). Also, Mr. Leyda has written a number of books about Soviet films and speaks Russian (while he doesn't speak Chinese, and apparently relied on a constant interpreter to make sense of the Chinese films he was viewing), but when he tries to deal with the conflict between artistic expression and Party controls in China he makes almost no reference to parallel or divergent Soviet experience in this area, which would have formed a useful comparison.

Despite these defects, Mr. Leyda's critical conclusions are worth summarizing. He feels on balance that the most creative and dynamic period in Chinese films was in the 1930s and 1940s, when film-makers struggled against Kuomintang and Japanese oppression to make films of social value and human meaning. While fine films are not lacking in the post-1949 period, the transformation of film-makers to employees of the official propaganda organs has often resulted in the stultification and abuse of some of the finest talents in Chinese films, a process affecting other arts as well. This process reached a climax in the Cultural Revolution (1966-69), when many of the artists Mr. Leyda knew were attacked and their pictures withdrawn. As Chinese film studios have reopened in the last few years, they have been more sharply bound by orthodox film formulas than before, and one can

only wait and hope with Mr. Leyda that the creativity he reports for earlier periods will some day emerge again.

MARTIN KING WHYTE

*University of Michigan*

**Honecker and the New Politics of Europe.** By Heinz Lippmann. Translated from the German by Helen Sebba. (New York: The Macmillan Company, 1972. Pp. 272. \$7.95.)

Erich Honecker succeeded Walter Ulbricht in May 1971 as First Secretary of the East German Socialist Unity Party (SED) in a relatively smooth transfer of power. This biography, which covers Honecker's career until he became party chief, is a welcome addition to the limited English-language literature on the political leadership of the German Democratic Republic (GDR). Although not conceptually comparable, it provides a companion piece to the recent studies of Peter C. Ludz.

Heinz Lippmann's credentials are interesting because of his long personal acquaintance with his subject. Presently living as a journalist in West Germany, Lippmann was for many years an official of the SED, and from 1946 to 1953 he served as Honecker's deputy. Detailed first-hand accounts of Honecker's style of leadership, his personality and character, and his close relationship with Ulbricht make the book valuable to observers of contemporary European affairs even though the author makes no claim to offer a definitive work. The background and accomplishments of the future SED leader are traced chronologically, and little effort is made to place the subject in his historical or political context other than in the East German one. Thus as a biography the book is somewhat narrow, its focus on Honecker depriving the reader of a sense of the great dramatic developments of postwar Europe surrounding the GDR.

Erich Honecker's childhood, we are told, was characterized economically by material deprivation and politically by the militancy of his family environment. At the age of six, he entered politics, working for his father as a distributor of leaflets demanding better conditions for the mineworkers of the Saar. At ten, he joined the Communist youth organization, the Young Pioneers. He joined the German Communist Party at seventeen in 1929, spent the next years in anti-Nazi political activities, and in 1935 was interned for what became a decade of imprisonment. After the war, Lippmann tells us, Honecker ran the Free German Youth (FDJ) organization, and during this period developed what Lippmann

describes as keen tactical skill as a Party fighter. Honecker's ambitions coincided with his ideological tenets, and he became a close collaborator of Ulbricht, whose guiding principle was that East Germany and the Soviet Union should remain closely allied. On occasions when Honecker's judgment faltered, he was rescued by Ulbricht—incurring debts which he later repaid at times of intra-SED challenges to Ulbricht's leadership by his strict loyalty. Like his patron, Honecker was always aware of the indispensability of Moscow's support. Perhaps more than Ulbricht, he felt a deep emotion toward the Soviet Union; on the occasion of Stalin's death, says Lippmann, "his voice faltered, and tears ran down his cheeks" (p. 152).

Honecker emerged as heir-apparent in 1958, when with Ulbricht's support he became a full member of the Politburo, in charge of armed forces with responsibility for the security of the GDR. In 1961, in Berlin, he functioned as chief supervisor and builder of the Anti-Fascist Protective Wall, an accomplishment for which Lippmann almost praises his former colleague. The ultimate responsibility lay with Ulbricht and Khrushchev, the author notes,

But it was Erich Honecker who actually built the Wall. It was he who took charge of all the organizational and military preliminaries, supervised security while it was being built, and planned and directed the political and propaganda campaigns connected with it. The coordination of the vast military, organizational, and political plans that culminated in the utterly unexpected erection of the first barricades was entirely in his hands (p. 186).

This operation strengthened Honecker's position in the Party even further, and during the 1960s he continued to take hard-line stands on both domestic and international questions. When Warsaw Pact forces occupied Czechoslovakia in 1968, he welcomed the use of military measures to counter antisocialist developments. As *Ostpolitik* began to unfold in Bonn, Honecker was skeptical of Brandt and hesitant to destroy the carefully shaped image of the Federal Republic as an enemy. The latter is one of the few known examples of Honecker's disagreement with the preferences of the Soviet leadership.

Lippmann's picture of the SED chief is one of a conservative figure, pragmatic and thorough, neither mechanical nor flamboyant. When Honecker initially assumed power, reactions in the Federal Republic were pessimistic. The *Frankfurter Allgemeine Zeitung*, a leading West German newspaper, predicted editorially that "the moment will soon come

when Ulbricht will be remembered with a kind of dialectic regret" (May 4, 1971). Since then, however, there has been a steady, although uneven, trend away from earlier cold war rigidities. But here Lippmann's account ends, and we must search elsewhere for the record of Honecker's performance as party leader and for the broader international context within which he operates. Much of the information which would have been necessary for a more complete account is simply unavailable, of course, but Lippmann might have enriched his sketch of Honecker by the addition of social, political and economic material.

Prospective readers should hence be advised that the book's title is somewhat misleading in the English-language edition: this is *not* a book about the new politics of Europe. The original German title, *Honecker: Porträt eines Nachfolgers* (Honecker: Portrait of a Successor), should have been retained. Helen Sebba's translation is quite satisfactory, and there is no sense of awkwardness in the text.

ROBERT MCGEEHAN

*The City College of New York*

**The French Budgetary Process.** By Guy Lord. (Berkeley: The University of California Press, 1973. Pp. 217. \$12.50.)

Guy Lord's short book is the first full-length study of non-American budgeting from a political (largely Wildavskian) perspective to see print. His questions are: "What are the rules which govern the process? Who are the main actors? What are their functions? When and how do they intervene in budgetary decisions? What are their goals, and how do they seek to achieve them? In short, how are decisions made when the budget is prepared, discussed, and voted?" (p. 188). The book is based on some 60 interviews with participants, along with newspaper accounts and secondary works (mainly on public finance and bureaucracy).

The first three chapters are straightforward accounts of "The Development of Financial Procedure" from the Revolution to the late 1960s, "The Constitutional and Legal Framework," and "Stages and Actors" in the process. Budgeting was of course greatly affected by the Gaullist reforms of 1959, which reduced Parliament to a marginal role and gave the President a significant coordinating and even initiating function. More subtly, the influence of the Ministry of Finance within the administrative process increased as economic aspects of budgeting were given greater weight, and as growing complexity produced tighter

deadlines and a tendency to base decisions on administrative or financial criteria rather than political considerations. As Lord points out, the details of organization and procedure outlined here in some detail not only provide the framework for budgetary politics, but themselves become important strategic resources for the participants who understand them best.

The heart of the book is Chapter 4, "Attitudes and Strategies." Few students of budgeting will be surprised to learn that the French process is highly incremental, or that spending ministries emphasize the needs of their own clienteles, while Ministry of Finance officials see themselves as rising above special interests to decide unselfishly and objectively what is best for the nation. Still, it is startling that the repertory of budgeting behaviors appears to be so narrow: Lord's list of participants' strategies closely resembles those in independent accounts of other budget systems. (Very probably budgetary calculations are similarly universal, although Lord does not treat this aspect in detail). On the other hand, Frenchmen are not Americans, or Japanese. The budget subsystem lies too close to "the authoritative allocation of values for a society" not to reflect the style of the broader political system. The most interesting peculiarities in the French case are the historic and continuing tension between government and legislature, which continues to dominate much writing about budgeting; the tendency to meet political problems with legalistic responses; and the pattern of extreme fragmentation and institutional autonomy called *cloisonnement*. Professor Lord, following Michel Crozier, particularly stresses this last factor as producing many conflicts—the field vs. Paris or division vs. division within each ministry, spending ministry vs. Ministry of Finance—which cannot be resolved through hierarchical administrative channels. Rather unfriendly horizontal negotiations are the norm, and they often end in arbitration at political levels. Many overlapping structures, liaison conferences, and other bridging devices seem to be needed for the system to work.

Lord deals with his intended subject so competently the reader might wish for a touch of speculation in a broader context. Even though the data available at time of writing were probably insufficient for specific cross-national comparisons, a more comparative perspective might have allowed preliminary guesses about a few general questions. For example, what proportion of total domestic policy output is handled through the budgetary system in France? How incremental, rela-



tively speaking, is French budgeting? What is the balance between top-down, policy-oriented "macro" decision making and bottom-up "micro" decision making? Somewhat similarly, even if rigorous quantitative analysis is unfeasible, one still might try to ask "what difference does it make?" from time to time—do variables of process actually influence budgetary outputs? For example, does the loss of parliamentary influence mean that a smaller share of governmental resources will go to vote-based interest groups, like veterans? Does increasing complexity and more Ministry of Finance power mean more or less fluctuation in budget shares? How can variations in policy expenditures cross-nationally or over time be explained? Books such as *The French Budgetary Process*, with its detailed description and sensitive analysis of relationships among budgetary actors in one interesting country, provide the foundation for asking questions like these in the future, as well as expanding our understanding of contemporary governmental politics.

JOHN CREIGHTON CAMPBELL

*The University of Michigan*

**Buganda in Modern History.** By D. A. Low. (Berkeley: University of California Press, 1971. Pp. 277. \$8.75.)

The erstwhile kingdom of Buganda has long fascinated social scientists. Until its destruction in 1966, Buganda represented a unique amalgam of apparently contradictory elements. All observers noted, on the one hand, the pomp, hierarchy, symbolism, and power of the Kabaka (the monarch). On the other hand, however, they discovered the adaptability, dynamism, and political awareness of the Baganda. (Following Bantu spelling, in which prefixes are used extensively, let the reader be alerted that *Buganda* refers to the kingdom, *Baganda* to the people, with *Luganda* the language spoken by an individual *Muganda*.)

Professor Low makes clear that this scholarly fascination is amply justified. *Buganda in Modern History* is a set of essays—some brilliant—that collectively and episodically portray the kingdom from precolonial times to the 1966 sacking of the Kabaka's palace. He describes two "revolutions"—oligarchical and Christian—that accompanied and amplified the external pressures on the state in the late 19th century. These "revolutions" were linked to a distinctive tension in Baganda society between two norms of social organization. Each *Muganda* belonged to a clan (*bataka*); the clans were egalitarian, paradoxically, through

their ascriptive bases. At the same time, a strong hierarchical system existed, its significance enhanced by the oligarchical "revolution." Each *Muganda* was linked to a chief in a patron-client relationship (the *bakungu* nexus, based thus on subordinate-superior ties). Atop both the clan structure and the pyramid of commoners and chiefs sat the Kabaka. He united the two means of social organization in Buganda, each of which, in yin-yang complementarity, permitted adaptation under diverse circumstances.

None of what I have noted thus far will surprise those familiar with David Apter's *The Political Kingdom in Uganda* (1961). Yet Apter, in his concern for "bureaucratic nationalism," concentrated more on the oligarchical transformation than on the populist awakening. Apter tended thus to focus more on the *bakungu* pattern than the *bataka* pattern, as suggested in the previous paragraphs. In one of the curious ironies of history with which Buganda abounds, the populism that arose did not take the form common elsewhere in Africa, that is to say, the dominant, mass-based political party. Political movements that were founded by young, Western-educated Baganda foundered on the skepticism of farmers, who saw these upstarts as likely to become "only one more generation of dominating chiefs" (p. 154). Populism followed the *bataka* pattern (egalitarian), and became focused on the person of the Kabaka, in his guise of head of all clans.

How did the British view this bundle of enigmas? Low examines the parentheses of the colonial period, fittingly the symbolic beginning and end of imperial rule in East Africa. Buganda helped significantly change British public opinion in 1892. Numerous petitions from diocesan conferences, chambers of commerce, and public meetings, Low suggests, "heralded the beginning of the end of that mid-Victorian anti-imperialism which had on most occasions fought against the creation of 'formal' empire" (p. 82). The parentheses of colonial rule started to close in 1954, when a constitutional conference was held in which "the notion of a primary African state [in East Africa] had for the first time been given constitutional specificity" (p. 130). With self-government assured, little urgency impelled nationalistic parties to press their claims, accounting for the otherwise surprisingly low level of political commotion (apart from the temporary exiling of the Kabaka) in the 1950s.

*Buganda in Modern History* offers only partial glimpses into the rest of Uganda: for ex-

ample, into the replication of the chief-subordinate relationship outside Buganda, into the slow awakening of political movements, or into the dominance of the Baganda in the civil service. Elsewhere in Uganda, envy grew against the riches, exclusivity, and adaptability of the Baganda, leading toward the 1966 bloody battle between supporters of the Kabaka and troops of the central government. Despite the formal abolition of the kingdom and the 1971 installation of the idiosyncratic Amin, government (effected after completion of the manuscript), the energy and educational accomplishments of the Baganda continue to make them indispensable to Uganda's functioning.

Low's thematic book does not resemble the ordinary study of African politics or history. *Buganda in Modern History* is not a smooth, chronological recounting, nor is it focused upon an *idée fixe* to the exclusion of other arguments. Each chapter is self-contained, five of the seven chapters having been published earlier as articles. Overlap and disjunctures remain, as the author recognizes. Should the book be purchased? Yes, I suggest, for *Buganda in Modern History* is a useful, more readable counterpart to, and extension of Apter's 1961 book, and is a coherent compilation and re-editing of already published significant material. Few African societies have enjoyed such substantial analyses—yet perhaps no African society has been as important to social scientists as the Baganda.

CLAUDE E. WELCH, JR.

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**Daily Life in Revolutionary China.** By Maria Antonietta Macciocchi. (New York: Monthly Review Press, 1972. Pp. 506. \$15.00.)

One imagines the tour guide, his Mad-Hatter headpiece advertising destinations throughout People's China, standing near Tien An Men in Peking, hustling tourists into his bus. And then we're off, on a tour ever less magical and mysterious, with an itinerary at least as formal as the one that takes the provincials from Times Square to Greenwich Village. First it's off to the summer palace, where the splendor of feudalism has been proletarianized as a reminder of the oppression that was its obverse, now to the university with its scholars chastened by their remolding and speaking humbly, on to the transistor factory set up and run by proud former housewives, now to the school where deaf-mutes are treated successfully by dedicated soldiers using acupuncture techniques

they mastered through dangerous experiments on themselves, next on to the May 7 School for the Peking district where contrite cadres have been taught to respect common people by laboring among them. And so it goes, a series of set-pieces, Macciocchi's little different from those of dozens of other China-trippers, beneficiaries of improving relations between China and the West since 1970.

And yet for all their familiarity, many of these vignettes remain both poignant and interesting. It is through the presentation of these scenes that the Chinese portray themselves as they wish to be seen, as a people who have "stood up," in Mao's memorable metaphor, and as a nation-state of immense proportions whose members nevertheless feel and act out daily a sense of mutual involvement and collective purpose. "Confronted by such a collective presence," Macciocchi quotes another writer as observing, "Westerners (except peasants, the poor, and some rare exceptions) tend to feel awkward, stingy, and 'slobbish' . . ." (p. 107).

This is no less true of Macciocchi herself, a Communist member of the Italian Parliament, journalist-editor, and specialist on Asia and Africa, who was made reluctant to complain about a sudden and excruciating pain in her ear by the accounts of revolutionary self-sacrifice and courage she was listening to at the time of its onset. Perhaps this accounts for a kind of studied ingenuousness that appears from time to time in her comments. [The Chinese "don't ever seem to get mixed up when they talk. . . . They all seem to be born speakers and lecturers," the author says (p. 62), as if they had not said the same things in the same way to other foreign visitors and to each other in political sessions many times before. As a result of the Great Leap Forward, she observes farther on, "the peasants became adapted to the industrial process and established a closer relationship with the working class" (p. 122).] And too much of the book consists of verbatim transcriptions of stilted talks by her hosts, without sufficient analysis to help the texts overcome a triteness that is not mitigated by the sincerity with which they are delivered.

Nevertheless the book as a whole transcends both the limits of the experiences the author was permitted and the familiarity of the form (she was in China in late 1970, well before the numerous Americans whose writings, made available more quickly in the United States, account for this familiarity), on the one hand, and the Westerner's tendency to make of China what he needs, on the other.

There are a number of reasons for this, not the least of which is that the immensity of change and the new possibility of human dignity in China cannot be trivialized to the level of a tourist attraction, no matter how pat the presentation. The author sees these somewhat abstract experiences of "liberation" as essential to the release of energy we associate with development. When she does mercifully abandon (p. 135) her injunction to herself to "Be modest," and simply transmit without interpretation an account of her experience, the author describes China as the enormous experiment that it is, the laboratory for an attempt to carry out a unique and human-centered program for development. Particularly in her chapters on industrialization, medicine, and women in China, Macciocchi portrays effectively the interplay of efforts to expand China's resources and the determination to ensure egalitarianism in their distribution. She seems to appreciate the Chinese ambivalence to technology, which is essential to great achievement, for example, in space and weaponry, and at the same time has the potential to become "a new kind of barbarism," (p. 129), creating hierarchy and contributing to a politics the Chinese reject. And as a Marxist, the author appreciates the vital role the Chinese assign to ideology, a subject not always given its due in Western books on China, as a bulwark against the revival of the rejected tendencies. This understanding is woven into her interesting discussions of schools, cultural life, the use of Mao's writings, and the pervasive continuing consciousness she found throughout China of the critical significance of the Cultural Revolution.

Nevertheless, with all the author's advantages in her quest, there remains a feeling that something is missing, as with virtually all accounts of foreigners visiting China. Are we wrong to wonder where is the ripple of anger disturbing the placid surface of collective life, where the cadre who resents being "remolded," the peasant who is offended by the pervasiveness of politics? Can we do anything more than admit that in this sense the Chinese will remain (one winces to say it) inscrutable?

PHILIP E. GINSBURG

*University of New Hampshire*

**The Bolshevik Seizure of Power.** By S. P. Melgunov. Edited by Sergei G. Pushkarev. (Santa Barbara, Calif.: ABC-CLIO Press, 1972. Pp. 260. \$15.00, cloth; \$5.50, paper.)

This is a superb book, published originally in Russian in Paris in 1953, and now available in good English translation by James

Beaver with excellent editing, bibliography and indexes. The abridgment is limited and carefully done to preserve essentials. The original Russian title, "How the Bolsheviks Seized Power," describes the precise theme—the circumstances and the actions which enabled the Bolsheviks to seize and maintain power in October/November 1917. The author, S. P. Melgunov, was a distinguished historian, and an important political leader of the revolutionary era—the head of the People's Socialist Party.

Melgunov emphatically rejected a determinist interpretation of the Bolsheviks' victory; "October" was not the realization of 'February.' Only mistakes of those who were able to prevent the seizure of power by the Bolsheviks made it 'inevitable' (p. 3). He shared Kerensky's view of the importance of the Kornilov Affair in undermining the provisional government and strengthening Bolshevism, but credited the style of Kerensky's response to Kornilov with giving much unnecessary support to the Bolshevik cause. His book illustrates clearly the lack of defensive preparation by the moderate socialists as the Bolsheviks prepared their coup, and the strange role of Kerensky in preventing the sending of troops to Petrograd to regain control from the Bolsheviks once the coup had occurred. Moreover, Kerensky's government was so severely discredited as to render vain all hope of rallying anti-Bolshevik opposition around a scheme for its restoration.

Melgunov evidently believed that only the leaders of "revolutionary democracy"—the non-Bolshevik socialists—could have put an early end to Bolshevism. Their failure to do so is explained by a combination of their own errors of judgment and the aggressiveness and determination of Lenin. The chief error was a tacit assumption, widely shared (even by many Bolsheviks), that Bolshevik power would not last, an assumption buttressed by reluctance to initiate armed action against a fraternal socialist group. Hence the vain pursuit of a coalition government in the first days after the Bolshevik coup, and, once that hope was shattered, the placing of all faith in the Constituent Assembly. The strong showing of the Bolsheviks in the mid-November Constituent Assembly elections merely strengthened the reluctance to act against them, and the non-Bolshevik socialists drifted helplessly to the denouement in January 1918 when the Bolsheviks forcibly dispersed the Constituent Assembly.

There are many remarkable features of the book, not least of them the presentation of

Lenin, whose ruthlessness and revolutionary fanaticism, and whose occasionally irrational behavior, are unusually well depicted—often in the words of fellow Bolsheviks. Other interesting points are the degree of spontaneous popular opposition to the Bolshevik coup, and the amount of simple thuggery used by the Bolsheviks against socialist opponents, as in vandalizing the offices and equipment of the opposition socialist press in the early days following the coup. Perhaps the most striking point made about Lenin is his utter lack of inhibition about the use of verbal and physical violence, and all manner of deceit, against opposition within and outside the party, to establish and maintain his monopoly of political power and his own conception of the revolution. It is scarcely surprising that he won against an opposition which “insisted on liquidating the October revolt without repression and was only willing to allow ‘noncooperation’ as a means of obtaining this objective” (p. 189).

Melgunov's presentation is eloquent testimony to his scholarly training and commitment. He is a reliable and perceptive guide to the extensive memoir literature on the events he describes, and is ever careful to indicate the limits of available factual information. He is a sharp critic of Lenin, but he is no less critical of Lenin's erstwhile colleagues. In his presentation, as in the events, the real heroes are largely obscure figures—such as the Bolshevik Lozovsky, a man of decency and principle who fought Lenin's exclusion from power of the non-Bolshevik socialists. But even this remarkable man clung to the premier political fallacy of Russian revolutionary democracy—that democracy was the concern and the right only of socialists. As Melgunov notes, whatever role the Constituent Assembly might have played in the struggle with the Bolsheviks, even the non-Bolshevik socialists had largely abandoned any commitment of principle to that institution. In an important sense, therefore, they were without an effective rallying principle as they faced Lenin; they were ideologically disarmed before the struggle began. The skillful development of this theme is perhaps the main contribution of the book.

HERBERT J. ELLISON

*University of Washington*

**Twilight or Dawn: The Political Change in India.** By Iqbal Narain. (Agra: Shiva Lal Agarwala & Company, 1972. Pp. 244. Rs. 20.)

Professor Iqbal Narain perhaps ranks among the top five or so political scientists

in India. In this book, much of which has already appeared as articles in various journals, he analyzes the impact of the fourth general elections of 1967 on the Indian political structure and examines the tensions and problems of transition from one-party dominance to multiparty competitive politics. Toward this end, he discusses, relying basically on newspaper materials, a series of issues bearing on state politics, center-state relations, coalition formation, and certain key political roles.

His work demonstrates a remarkable ability to see the essence of a problem amid a heap of detail, to articulate it expressively and precisely, and to generalize from the immediate event or case to a wider universe, accompanied by a penchant for, and great skill at, categorization and classification. The discussion is always balanced and level-headed, bringing clarity and sanity to the issues of great debate in Indian politics. Although the book lacks thematic unity, tends to jump from one subject to another, and does not deal with any issue in depth, it is essential reading for the delineation of the institutional issues in Indian politics in the late 1960s.

The major theme of the book is that the fourth general elections constitute a political watershed, marking the end of one-party dominance and generating another phase which the author calls the politics of polarization. In the development of this theme, Narain adopts a measured and cautious stance but nonetheless one that is characterized by a robust though unsupported optimism and insistent approval. He urges that the new phase of the politics of polarization should be treated “as a sort of protracted middle range phenomenon on the continuum of Indian politics which opens with one-party dominance, and whose ‘middle’ may itself have to undergo several political transformations by way of its stages of growth towards the final state of political equilibrium at which the Indian polity may ultimately settle down as a democratic system” (pp. 73–74).

As a perspective this may be useful, but Narain constantly shifts the grounds for optimism and approval. If one-party dominance comes to an end, it is considered a desirable development for competitive politics; if competitive politics assumes unsavory forms, it may be considered disturbing but is necessary for the transition to a new political equilibrium; if the Congress party splits, it is seen as a necessary step toward polarization in Indian politics; and if one-party dominance is restored that too is hailed as a well-deserved end to the unhappy experience of competitive

politics. It would seem that almost anything that happens within the Indian political system can be seen as either desirable or necessary for desirable ends.

The key weakness of the analysis, however, is its wholesale concentration on political epiphenomena. The focus of attention is entirely political structure, with no reference to the underlying economic forces and social classes. More important, there is little concern for the ends for which political structure exists or ought to exist—social justice, national integration and political independence. In the evaluation of political structure, Narain curiously apotheosizes differentiation and, flowing from it, the liberal-democratic framework, especially of the British variety, while he denigrates—through neglect—system performance in relation to development. Fundamental to this orientation, I believe, is the author's failure to appreciate the essential difference between the political features of *modernity* and the political requirements for *modernization*. This is unfortunate for Indian political studies, because they would benefit tremendously if Narain's great analytical skills and incisive intellect were also turned toward these issues of crucial concern.

BALDEV RAJ NAYAR

McGill University, Canada

**Area Handbook for Ceylon.** By Richard F. Nyrop, Beryl Lieff Benderly, Ann S. Cort, Newton B. Parker, James L. Perlmutter, Rinn-Sup Shinn, and Mary Shivanandan. (Washington, D.C.: Foreign Area Studies, 1971. Pp. 525. \$3.50.)

The intent of this volume, one of a series of area handbooks prepared by Foreign Area Studies of American University, is to assemble and organize a large quantity of information on a wide range of subjects relating to modern Ceylon. In this, the volume seems successful. Although it contains little that is new to the social scientist familiar with the nation, it can provide a convenient starting point for the scholar (or other person) who is commencing study of Ceylon. The volume is divided into four major sections: "Social," "Political," "Economic," and "National Security." The longest, by a wide margin, is that titled "Social," occupying 224 of the 472 pages of text. The section on "National Security" is only 27 pages in length, but is probably as full an examination of the police, the military, internal security, and crime as exists. The emphasis is on description, giving to the narrative a rather flat and uninspiring quality. Nonetheless, the volume does, overall,

seem to capture some of the complexities and incongruities of a nation involved in rapid social, political, and ideological change. That there are some factual errors, misinterpretations, and oversimplifications seems less surprising than that there are so few, given the wide range of information crammed between its covers.

A major limitation on the value of the volume results from the particular time it was written. The research and writing was completed in 1970. Although events stand still in no nation, the past four years have witnessed particularly breathtaking change in Ceylon. The adoption of a new constitution in 1972 rendered obsolete the description of governmental organization, and indeed outdated the title of the volume, for the nation is now officially the Republic of Sri Lanka. Other subsequent developments have had far-reaching political consequences. The statement that "students and youth in 1970 posed no immediate threat to the incumbent party or to the maintenance of public order" (p. 258) seemed a reasonable observation in 1970, but early the following year an armed insurrection of students and youths left at least 1,200 persons dead and some 14,000 in custody. The handbook, in short, can be useful as an introduction to Ceylonese society, but those using it should be alert to many changes, some profound, in the intervening years.

ROBERT N. KEARNEY

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**Toward Central Government Computer Policies.** By the Organization for Economic Co-Operation and Development. (Paris: Organization for Economic Co-Operation and Development, 1973. Pp. 215. \$4.75.)

This monograph is the fifth in a series on government computer problems issued by the O.E.C.D.'s Committee for Science Policy. Prior volumes have focused on the implications of computer technology for public administration, communication, and the privacy problem. These issues are necessarily raised in this study as well, for it focuses upon governmental decision making about the structure, content, and utilization of public and large-scale private data bases. It surveys the state of the art and short-term plans of O.E.C.D. member-nations, and analyzes their common and divergent approaches to data-base development.

Data banks store a vast array of information—the budgets of governmental agencies, trade-flow statistics, and weather reports. This volume focuses upon the most nettlesome form of data, that organized around individual

citizens or private organizations. Government file clerks at various levels long have squirreled away much of this information. The computer, a technological development, simply has encouraged them to convert their files to machine-compatible formats. It is an *administrative* development, the integration key, which makes it feasible to merge these files. Driver's license, land and vehicle ownership, tax liability, church membership, military service, arrest, and voting participation information can be merged practically into super files only when each citizen or business is assigned a unique identification number. This practice is currently most advanced in Germany, Sweden, and the Netherlands, the committee reports. In the United States, the social security number has enormous potential in this regard. Congress, however, has been vigilant in resisting the Office of Management and Budget's plan to establish a National Statistical Data Center along these lines. With the exception of England, where the Opposition (whichever party it happens to be) also opposes executive data-bank proposals, most O.E.C.D. governments have enjoyed less resistance to the development of administrative information systems.

With some simplification, these systems can be classified into two categories: statistical systems, which rely upon integration keys to associate data but produce only grouped or numerical output; and intelligence systems, which store and regurgitate dossiers on specific individuals. But without elaborate and costly safeguards the former can usually be turned into the latter, and both contain information that can be reused by persons and for purposes far removed from those ostensibly involved in its collection. In the United States, the left has been concerned about "overt" files on public-assistance recipients, those who collect unemployment insurance, credit applicants and arrestees, as well as "covert" data banks on black leaders, political activists, and those who have come to the attention of the F.B.I. and the I.R.S. The right has aimed its guns at meddling by the Bureau of Census, which they argue has gone far beyond its constitutional mandate to count heads. What both groups are calling for is what this book is about—policy-making concerning the collection, integration, use and reuse of data on citizens.

The report concludes that there are in practice three solutions to the problem. Some member governments monopolize the rights to these data bases and maintain their own systems at the highest level to process them (Finland,

Denmark); some attempt to coordinate and contain data base construction and use at the ministerial level (France, Holland, Sweden); and several pursue anarchistic nonpolicies that the committee euphemistically dubbed "partial co-ordination" (the United States, Canada). The latter approach consists of "organizing interaction among ADP users rather than imposing decisions on how their systems should be developed or operated" (p. 39). While this does not mean that anything goes, a lot does; and it is in the partially coordinated countries that disputes over who has access to public and private files, to what uses they may be put, and what power people have to review the accuracy and relevance of their files' contents, have been most prominent.

WESLEY G. SKOGAN

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#### **International Population Census Bibliography:**

**Europe.** By the Population Research Center, Department of Sociology, The University of Texas. (Austin: Bureau of Business Research, University of Texas, 1967. Pp. 472. \$6.00.)

Until the recent attention given to environmental variables by state and local policy researchers, the importance of demographic variables for the political system and public policies has not been generally recognized. For this reason, census data, in spite of their utility and potentially quite low cost to the researcher, have not been used. The *International Population Census Bibliographies* should further decrease user costs, and this volume on Europe should have exceptional utility for persons interested in both cross-sectional and longitudinal analyses of comparative policy research.

The volume presents an annotated bibliography by country and in chronological order of all available published censuses of the countries of Europe. This means that knowledge concerning data on not only demographic characteristics but also housing patterns, income, land ownership, and other politically relevant variables is available to researchers. Census data are available in many countries for well over 100 years and often on a state, city, and country basis. For the researcher who needs to include such variables in his analyses, the *Bibliographies* provide the best available source to indicate what can be found and where.

R. KENNETH GODWIN

*Oregon State University*

**The Civil-Military Fabric of Weimar Foreign Policy.** By Gaines Post, Jr. (Princeton, N.J.: Princeton University Press, 1973. Pp. x, 398. \$16.00.)

This excellent study will challenge the notion that the change from Weimar to Hitler marked anything quite so neat as a democracy retreating before the mob in accordance with constitutional processes. Professor Post makes clear that the essential characteristics of the late Republic (1930–1933) were those of temporizing, of attempting to compromise with disaster, and, finally, of immobility. As a consequence, the surrender of an ineffectual republic was merely one more in a series of surrenders. One may wonder whether anything as neat even as surrender ever occurred. Post does not force upon his narrative a predilection for the sharp break, for what Meinecke called “the caesura,” between two distinct eras in the winter of 1932–1933. It is part of Germany’s tragedy that contradictory policies and opposing personalities simply faded into one another. Nothing demonstrates this better than the civil-military relations of the Weimar system: a “paradigm . . . [which] concentrated power in the executive-departmental sphere and stressed interdepartmental planning” (p. 189).

Dealing with functional relationships more than with actors, the book does not concern itself directly with personal vignettes. Personalities, to the extent that they are described, emerge from the political maelstrom in which they (often, of course, because they are who they are) are caught. It is only occasionally that one senses that policies (or relationships) might arise from character or lack of it. This is not necessarily a virtue, but the book thereby avoids one of the risks of historical writing: limning individuals in greater scale and sharper outlines than the evidence warrants, or else blurring an individual’s distinctive qualities by suggesting that these were all of an age or of a condition. Professor Post avoids the now fashionable psychohistory; for this we are in his debt.

This judicious study provides a picture of cautious foreign and military policies dictated more by necessity than by good faith. The characteristic policy approach, as outlined by Post, was one of indirection. Germany embedded her international objectives in the policies of others; and civilians and military interchanged their domestic goals under the rubrics of revision, security and stability.

One learns that despite some attachment to Clausewitz during the Republic, underlying the

relations between the military and the civilian was a sense that for Germany war was not seen as a “continuation” but as a reversal of policy, as an instrument of *another* policy. The “fabric” in the title involves the shared interest of the Wilhelmstrasse and the Bendlerstrasse that war not be excluded as a means of undoing political settlements. It remained for the political side to placate the West while both sides planned for revision of the frontiers in the East. Revision was a strategic objective, linked with security in the East; it was not, according to Post, basically a humanitarian goal.

Civil-military relations were not simply in a state of equilibrium, but in *the* state of heterogeneous equilibrium which is at the root of post-1919 Clausewitz. Each “weight” had its own constituencies, guiding principles, counsels, and powers. This system remained in a state of conditional armistice among its subsystems until its legacy of suspicious and tainted activities served to embolden all manner of laughing heirs. As Post demonstrates in an exceptionally lucid discussion, the fragility of the balance was shown not only when the civilians and military participated in joint planning: the tensions between them carried over into and exploited service rivalries. Post provides the ingredients of this balance—a “conditional definition of ‘political leadership’ and the military’s selective obedience to that leadership” (pp. 351–352). Party politics and a bureaucracy politicized at the top were phenomena not anticipated by Clausewitz. Thus Weimar made explicit a flaw in Clausewitz: the assumption of a responsible civilian or political component to give direction to an inherently responsive military whose views were complementary to the political.

Military aloofness from politics, to the extent it existed, can be explained, Post correctly notes, as well by suspicion of the parliamentary system as by fidelity to Clausewitz. The attitude disappears with the ascendancy of von Schleicher, whose approach to the parliamentary system was unabashedly and quintessentially political. Versailles had succeeded in making an inner emigrant of the military. The military’s hostility to parliamentarism, a sentiment it shared with the executive and the bureaucrats, then made it a readily available inner balancer between the executive and the legislature.

Basic to Post’s essay is the realistic Foreign Office and Defense Ministry view that, given the behavior of the Reichstag, the priority of the political over the military should be nar-

rowly interpreted as being within the exclusive purview of the executive and bureaucracy. Especially instructive as to the depths to which the Republic had militarily and politically sunk is Post's treatment of the drama surrounding Schleicher's selection as chancellor.

Gaines Post has laid before us the irony that the highpoint of the *Reichswehr's* power was the selection of the "field-grey eminence," Schleicher, at the very moment he was forced to admit the failure of his long dabbling in politics. Ultimately all—civilian and military—succumbed to the chronic debility which characterized the Republic. Only the paramilitary, it seems, was spared.

The Weimar system represented a restless and laborious balance between a military weight which sought to maintain and increase Germany's physical force and regain its own *éclat*, and a political weight which sought to increase Germany's moral force and freedom of action. As the two were inextricably fused and reciprocally dependent, the failure of the one would require increased energies and genius of the other. The last man to attempt to maintain this balance was both a politician and a soldier.

Professor Post has written a book whose very excellence will rescue it from being *the* definitive work on the topic of civil-military relations and German foreign policy between the two world wars. He raises too many attractive questions and opens too many avenues for additional research for this to be the proverbial last work. And, with the modesty characteristic of the good historian, he shies from such conceits.

PETER K. BREIT

*University of Hartford*

**Patterns of Australian Federalism.** By J. E. Richardson. (Australia: Australian National University Press, 1973. Pp. 142. \$6.00.)

The decision to establish a Centre for Research on Federal Financial Relations at the Australian National University in 1970 was unquestionably wise. One would be hard put to find a more critical and permanent area of federal study than the way monies are raised and allocated between the federal and state governments. However, the decision by the Centre to publish Professor Richardson's monograph and especially to make it the first of the proposed series of research studies to come from the Centre was most unwise. Unhappily, it is not his best work. First, the title is misleading. The 104 pages of this mimeographed text largely consist of a fragmentary commentary on such items as—the legislative machinery of the Com-

monwealth Parliament, Commonwealth legislative powers and their interpretation, Commonwealth-State finances, types of Commonwealth-State cooperation, the separation of powers in the Commonwealth Constitution, and the various attempts to amend the Constitution. Neither the title of these sections, nor the content suggests a "pattern," let alone "patterns" of Australian federalism. Indeed the evocative use of the title "Patterns of . . ." is too close to the box-office eyes of the film distributor who gives the title "Sabbatical Lust" to the simple story of two small children exchanging a kiss at Sunday school. Again, it is surprising to find such an outmoded arrangement and treatment of the material. One would have hoped that by now constitutional lawyers, even if they scorned pop-jurisprudence, would have left the style of the 20s and conveyed some sense of the close politico-legal mix of their subject.

The teasing question throughout, however, is, "Who does the writer intend to instruct or enlighten?" It cannot be the constitutional historian, for the historical content is incidental and far too slight. Nor can this monograph serve any student of politics, for there is little of politics, and the very little there is, when it is not transparently tendentious (e.g., p. 63) hardly takes cognizance of the distinction between constitutional law and *realpolitik*. Were Professor Richardson aware of the distinction he would see no paradox between the readiness of the Australian states to complain of the federal invasion of their "sovereignty" and their continuing tolerance of the power of early imperial legislation to nullify their laws. The first belongs to the pressing and permanent reality of the struggle for funds; the second is an anachronism that rarely discomforts them. Again this monograph can hardly be of any special interest to students of constitutional law. For while there are synopsisized bits and pieces that would not normally appear in constitutional law texts (for example, the three brief sections baedekering instances of Commonwealth State cooperation) the rest is an analytically thin goulash of what is available in more complete and sophisticated studies of Australian constitutional law. Possibly, the novice federal politician who is totally unread in the powers of the Commonwealth may find some value in this work. For in some respects it is a bit like a handy *vade mecum* to the constitution. Possibly too, those who are drawn to constitutional revision may find some interest in the writer's proposals to amend the constitution. For example, proposals for the removal of the right of ultimate veto from the Senate, ways of extending the federal power over economic affairs, su



gestions for the revival of the Commonwealth Inter-State Commission, changes to improve the process of judicial review, proposals to facilitate federal taxation on behalf of the States, and so on. There is some value in these ideas even if they are almost entirely without novelty. But to graft these recommendations on to a hotchpotch of law, microtonic slivers of history, and subliminal political messages is not what a research monograph should be about.

RUFUS DAVIS

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**Atencingo: The Politics of Agrarian Struggle in a Mexican Ejido.** By David Ronfeldt. (Stanford, Calif.: Stanford University Press, 1973. Pp. viii, 283. \$10.00.)

As I was reading David Ronfeldt's excellent study, I thought of Regis Debray's thesis that a successful revolutionary process requires the establishment and development of a hard-core guerrilla force. But this did not take place in the Mexican agrarian struggle. As Professor Ronfeldt indicates, revolutionary conditions have not existed in Mexico for decades. The *ejidatarios* might have been critical of certain officials and policies, but most of them had a deep respect for the government's authority and firmly believed in the benevolence of the president. There was no legitimacy vacuum in Mexico for the *ejidatarios* of Atencingo. They chose to struggle for reform within the established institutional system, and if I might modify Debray's terminology, they offered an example of a struggle for Reform in the Revolution.

In his well-documented study, Ronfeldt chooses the period from the 1920s to 1969 to analyze the causes and consequences of political and economic struggle in Atencingo, a complex which included a town of 7,500 people, one of the principal sugar mills of the country, the ejido of Atencingo, the ejido's fertile cane lands and credit society, and eight villages in the ejidal system. The author focuses on a long series of struggles—often marked by violence—among the peasants, powerful economic interests, and state and federal government officials for control of the ejido's valuable lands and production. But rather than a description, Ronfeldt offers a comprehensive analysis of agrarian struggle in Atencingo, crystallized in an excellent final chapter, by focusing on the effects of the strategic context in which the struggle had taken place; the strategies and tactics employed by the participants; and the consequences of the struggle.

The strategic context for the *ejidatarios* was in part determined by the allies and opposition they had at a given time. Ronfeldt discusses

the role of William Jenkins, a former American consul who remained in Mexico for more than 40 years to amass a fortune. At the time of his death in 1963, it was estimated that he was worth between 200 and 300 million dollars. It was his land that was expropriated to form the Atencingo ejidal complex. Jenkins remained a powerful figure through his control of the sugar mill and the administration of the ejido. He was a formidable force in his opposition to any meaningful structural reform; exploitation, murders and violations of the law were commonplace under his regime.

Ronfeldt points out that strategically the *ejidatarios* had very little political or economic power. On major and many minor issues, what happened in Atencingo was determined by developments in Mexico City and Puebla (the state capital). Such external realities often blunted the struggle for agrarian reform. For example, the attitude of a new state governor or president of the country was of crucial importance. Presidents Ruíz Cortines and López Mateos were more sympathetic to the aims of the *ejidatarios* than was President Díaz Ordaz, who was more interested in order than reform. The same reality held true for the attitude of bureaucrats in governmental agencies, particularly the all-important National Peasants' Confederation (CNC). Ronfeldt also feels the different perceptions of the government and the *ejidatarios* affected the effort to realize structural changes. The former wanted political stability and economic development, concentrating on sugar cane production; the latter wanted local institutional power and agrarian reform. They demanded a radical change in the ejido structure: diversification of crops, division of the ejido, parcellation of ejido land. The government did allow local institutional power, but basic structural reform threatened key political and economic interests. This was a major issue in the struggle. Ronfeldt explains how the *ejidatarios* utilized a variety of strategies and tactics in their effort to demonstrate a combination of popular support and government support. They had to cultivate governmental officials; popular support alone was not sufficient to win concessions. They did succeed at times in electing their own leaders to run the ejidal complex, but even they broke many promises and the *ejidatarios* were forced to resort to bureaucratic lobbying, propaganda, direct action (violent confrontations, work stoppages, building seizures, a march on Mexico City). As Ronfeldt indicates, however, the government effectively employed cooptation and control. It utilized the patronage system and benefitted those who cooperated. Throughout

the years the government might react to local pressure by conceding local power, but it denied reform by immersing the *ejidatarios* in such technical tasks as census, survey and mapping.

Ronfeldt concludes that the consequences of the struggle were a victory for the government, to some degree the mill administration, and a defeat for the *ejidatarios*. Ultimate structural reform did not take place, although the *ejidatarios* did receive some general socioeconomic progress in their standard of living. The government retained its power and assured stability under its control.

David Ronfeldt's work provides an excellent analysis of the interrelationship of politics and economics in the struggle for agrarian reform. It also raises serious questions concerning the promises and performance of the Mexican Revolution. The book should be of considerable value to those interested in comparative institutions, the problems of developing societies and the process or revolutionary change.

DONALD L. HERMAN

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**Education and Politics in India: Studies in Organization, Society and Policy.** Edited by Susanne Hoerber Rudolph and Lloyd I. Rudolph. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 470. \$20.00.)

The title of this edited volume may raise hopes that it will focus attention on one of the many important aspects of the relationship between politics and education in India. As the subtitle suggests, however, the contributions are concerned with organization, society and policy; topics that seem almost to expand, rather than delimit, the book's parameters. Though the subject matter of the contributions is wide-ranging and diffuse and their quality somewhat uneven, the serious reader should not be deterred from carefully exploring this book for the significant insights and information it contains. Some mapping of its scattered resources, however, may be useful.

The book is concerned for the most part—though by no means exclusively—with Indian higher education. Its most consistent substantive focus is the organizational character and quality of Indian educational institutions. The general theme that engages the attention of most of its authors is the permeability of those institutions to external political influence. Forty per cent of the volume is devoted to studies of the political structures and environments of three colleges—Presidency in Calcutta, Muir in Allahabad, and Mohammedan Anglo-Oriental in Aligarh—and two universi-

ties—the Maharaja Sayajirao University of Baroda and Osmania University in Hyderabad. Three other chapters explore the influence of communal interests and party politics on higher secondary schools and intercolleges in Faizabad District of Uttar Pradesh, on privately-managed schools and colleges in three districts of Mysore State, and on primary schools managed by *panchayati raj* institutions in Rajasthan.

The book's most penetrating and significant theme concerns what, in other hands, might be referred to as elements in the political culture of education in India. Eschewing social scientific terminology in favor of an analogy deriving from recent developments in biology, however, the Rudolphs suggest that the historical legacy of educational institutions in India "impresses" upon the contemporary educational system "certain forms and traditions that function rather like a *genetic imprint*, which dictates the further evolution of their institutional arrangement, not with the exactitude it exercises in a biological organism, but with a certain dependability" (p. 13, emphasis added).

A series of historical themes is developed that focuses primarily on university education. The reader becomes familiar with the early pattern of government subsidization of privately-sponsored colleges; the development of affiliating universities which functioned not as centers of advanced education but rather as bureaucratic control mechanisms designed to set examinations and maintain standards in the colleges; the bureaucratization of professors in an Indian Education Service (IES) cadre; and the growth of university governing bodies that were at once early training grounds for Indian competitive politics and heavily infiltrated by governmental representation.

In a revealing chapter, for example, Irene A. Gilbert describes both the conditions and traditions that led to the creation of the IES and the resulting patterns that shaped subsequent Indian education. The need for career incentives to attract qualified applicants to teach at struggling Indian universities was met by the establishment of a cadre that supplied not only professors but also educational administrators and inspectors. The mixing of teaching and administrative roles, the emphasis on building civil servants for the Raj and gentlemen of character, and the relatively late development of professionalization among academic disciplines in English universities all contributed, Gilbert argues, to the failure to develop traditions of academic professionalization that might have withstood the pressures

toward bureaucratization to which Indian academics have largely succumbed.

A second chapter by Professor Gilbert on three of India's early privately-managed colleges describes patterns of internal conflict that developed in the early twentieth century as the nationalist movement politicized students and Indian members of the governing bodies of the colleges. Carolyn M. Elliott's chapter on Osmania details the progressive evolution of that institution from an urban and Urdu-oriented university to one more responsive to public pressures of rural Hyderabadis, and subsequently to a major university in the expanded state of Andhra Pradesh. The theme of her study is the university's efforts to maintain key elements of its autonomy in the face of increasing challenges from the state government.

The University of Baroda is better prepared to resist the threats to the autonomy of its academic interests than most Indian institutions. A unitary, rather than affiliating, university, it has traditions of disciplinary autonomy and excellence. Its early links with the elites of Baroda were maintained as that group established itself in prominent positions in both state and municipal post-independence politics. The Rudolphs, in a lengthy chapter on this university, document the evolution of its management institutions in response to the increasing politicization of its environment. They argue, on the basis of extensive data, that while the university has resisted pressures for the "localization" of its student body, it has accommodated demands for "easy access professional and occupational training at more modest standards" (p. 237).

Related features of the genetic imprint are drawn on by Harold Gould to explain the accelerated exploitation of higher secondary schools and intercolleges in Uttar Pradesh by local notables as institutional bases for their political ambitions. Traditions of the nationalist movement leadership in sponsoring educational development and government grant-in-aid programs to support private institutions, Gould argues, combined in the post-independence period to make such institutions both accessible and rewarding bases for partisan organizations. This theme is effectively illustrated by case studies drawn from the author's field research in Faizabad District.

In these detailed descriptive studies, and in the first three of the Rudolphs' six introductory chapters, the reader gains a number of important insights into the political and bureaucratic roots of contemporary higher education in India. The authors write from the perspec-

tive and with the insights of political scientists. At the same time, their concern for the intellectual growth and integrity of Indian universities is apparent. Their consequent sensitivity to the often conflicting values of the university as an autonomous center of learning and the university as an instrument of democratic change is one of the appealing features of this book.

The quality of the other studies varies greatly and often, it seems, inversely with their relevance to the central themes of the book. Paul Brass has contributed a very interesting chapter on the highly effective political lobbying efforts of the revivalist Ayurvedic medicine movement. Brass describes how this group pressured the central government to grant it a large measure of professional legitimation and support for a massive Ayurvedic educational establishment. Analyzing the internal conflicts that have plagued the movement in its efforts to develop coherent medical practices, Brass concludes that, "The failure to develop adequate educational and professional standards in the Ayurvedic colleges has meant the production of thousands of new, poorly qualified practitioners, if not quacks, providing a low quality of medical relief to both urban and rural residents" (p. 371). The study might more appropriately have been included in a volume on the politics of medicine in India, but it makes rewarding reading nevertheless.

The chapter by T. N. Madan and B. G. Halbar uses caste and religious data from 156 privately-managed schools in Mysore to show that the students and staff in these institutions are disproportionately grouped in schools managed by members of their own communities. Many readers may be less surprised by this finding than by the diversity of students in these schools: fully 60% of the students served by these sectarian institutions are from communities other than that of the management of the school they attend. In any case, one must share the Rudolphs' skepticism (p. 88) of the authors' contention that the social composition of these schools is convincing evidence that they "fail in a crucial manner to advance the modernization of the educational system and society" (p. 146).

Iqbal Narain, in a chapter on primary schools in Rajasthan, argues that the transfer of control over those schools from the state government to local political institutions of *panchayati raj* has "infused politics into the management of primary schools" (p. 162). He cites Legislative Assembly debates as evidence for partisan political activities of teachers,

political motivations in personnel and distributive decision-making, and deterioration of standards under *panchayati raj*. His contentions are marginally strengthened by questionnaire data drawn from a sample of 85 respondents in 2 of the 232 *panchayat samitis*. The Rudolphs, in establishing a comparative context for the study, suggest the contrast with decentralization in New York City: "teachers in Rajasthan are subject to being used or appropriated by community or partisan interests, while teachers in New York . . . seem to have appropriated the educational system for their own benefit" (p. 90). In attributing this difference to weaker teacher professionalization and bargaining capacity in Rajasthan, they miss the key structural distinction: functionally specific political bodies such as school boards are much more subject to capture by professional interest groups than are general purpose bodies such as panchayats.

Despite the weaknesses and idiosyncratic subject matter of some chapters, this book does, then, have much to offer. Its first three chapters, especially, provide a lucid and insightful analytic introduction to the study of education and politics in India. These pages, and selected portions of the rest of the volume, should be read by anyone concerned with this subject, and particularly by the scholar interested in beginning to fill the many gaps in our knowledge and understanding of the complex relationship between the political and educational spheres in India.

STANLEY J. HEGINBOTHAM

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**The Heroic Image in Chile: Arturo Prat, Secular Saint.** By William F. Sater. (Berkeley: University of California Press, 1973. Pp. 252. \$10.50.)

Here is a haunting book and, sadly enough, a most timely one for students of Latin America and for Chileans alike. This work of an historian is an outstanding contribution to the understanding and appreciation of the taproots of Chilean political culture and the impact of historical idiosyncracies on present-day politics.

All societies develop specific needs and desires to create heroes in their own image. A time of societal crisis, the uncertainties of change processes, and the ambivalence of individual or group response to the challenges of the day all tend to focus the societal spotlight on the hero. The hero as the martyr, the hero as the avenging angel of real or imaginary injustices, the hero as the "political saint." On May 21, 1879 a Chilean naval officer, Arturo

Prat became a symbol of national commitment, rejuvenation, and dedication to act and to sacrifice. Alas, his sacrificial act was *per se* a meaningless one, the culmination of a meaningless battle in a meaningless war. Or so would claim the cynics.

Indeed, Prat's simple act of heroism had no pragmatic basis, nor did it significantly alter the course of Chilean history. What it did, though, was to provide a touchstone for judging the attitudes and behavior of Chileans of later generations. The eminently pragmatic, and for a typical *latino* much too unself-conscious, Chileans became both haunted and fascinated by the existential figure of an authentic if not also enigmatic hero of their historical consciousness. But just what exactly did the image of this naval officer represent for Chileans of the past and the present?

As Professor Sater most convincingly argues, the constant resurgence of Prat's popularity and his adulation is clearly related to the cycles of internal political, economic and societal crises Chilean society experienced. The anxiety and the uncertainties of the processes of change compelled Chileans constantly to reinterpret the specifics of Prat's behavioral relevance to their age and their circumstances. The hero singlehandedly captures the images of nationalism and readiness to act instead of debating, and compromising—qualities so much cherished by the urban *chileno*.

These qualities of course directly relate to the questioning of one of the most revered institutions of political bargaining in Chile, the parliamentary system! Time and again Chileans under the stress of change wondered if the endless bickering and the impotence to act in the Chilean congress is something that a country can really afford.

Similarly, Prat's image tended to remind Chileans that their country in many respects is merely nominally independent as long as the dominant economic interests in society tend not only to link themselves to the outside world but also to ape its social mores and consumption habit. This clearly carries an eerie connotation of memories of things recent past.

The two-year lifespan of the socialist experiment and the bloody intervention of the military in 1973 all find some explanation in this brilliant exposé by Professor Sater. And explanations also give rise to some nagging doubts about decades and volumes of historical and political analysis that have dissected Chilean political behavior and institutions. Were they all to the point in stressing the inherent stability and strength of institutions,

the "correctness" of the military's behavior and the relaxed and sophisticated tone of political bargaining? I believe that it is in exactly this realm that the lasting value of Sater's work lies. In light of the implications of his exposé, students of Chilean politics will have to reexamine some of the basic assumptions we now hold about Chile's political culture. Bless the strength of the persistent social historian who can place the unpleasant reminders of the past into the proper conceptual perspective for us political scientists!

For those of us who will have to return to our "homework" on analyzing the Chilean political system, this volume offers an excellent documentary and historical backdrop. The use of new materials is extensive and the bibliographic material included is a most comprehensive one. Professor Sater clearly offers us an opening salvo for a "revisionist" reevaluation of many things we assumed as given. For that alone his contribution is more than appreciated.

ARPAD VON LAZAR

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#### **Burma: A Selected and Annotated Bibliography.**

By Frank N. Trager. (New Haven, Conn.: Human Relations Area Files Press, 1973. Pp. xii, 356. \$18.00.)

The explosion of publications, documents, articles, monographs, and dissertations—has confused as much as clarified our knowledge of non-Western countries. That Burma, one of the world's least visited states, should yield 2100 entries in a substantial bibliography is a comment on the inflation in quantity of publications. Still, the task is incomplete, for although the volume includes some 96 dissertations, it omits all but 50 Burmese language references including the most important journals; furthermore all non-Western language sources are excluded. For the researcher who uses German, French, and Japanese there are other English bibliographic sources noted, in addition to a list of nearly 200 Russian and other Slavic entries.

Although there are brief annotations which move the novice a step past the titles, there is no critical comment to suggest the relative importance of material; however, cross-referencing will aid those who proceed by discipline. For example, "politics" and "administration" are separate categories in the topical index, and authors are indexed both alphabetically and topically.

The value of *Burma* for the individual scholar may be questioned if a university is

nearby, for the price is \$18. Most of this material is listed in the Association of Asian Studies annual bibliography (which also includes Burmese journals), the Cornell and Ohio University Southeast Asia (and Burma) bibliographies, and the standard *Readers' Guide* and *International Index*. The *Guide* and *Index* will be a means of updating Trager's entries which end in mid-1970. While the book is handy, it may be too expensive a luxury for any but the most avid researcher, therefore it should be purchased by any library with a pretense of Asian coverage to assist the individual scholar.

While scholars have complained about the paucity of social science work on Burma, this collection reveals a mass of material that elevates Burmese studies well beyond the "exotic." Burma escaped the deluge of third-rate social science financed by the Department of Defense for Indochina and Thailand. Many of Trager's references are of the highest quality, in particular the historical research of J. S. Furnivall and Gordon Luce which ranks with the world's best, while the anthropological research of Manning Nash and Melford Spiro is superior to most U.S. scholarship elsewhere in Southeast Asia. Unfortunately such critical commentary is not part of this bibliography. Another gap in our knowledge of Burma is revealed in the lack of humanistic sources. While Burmese Buddhism has long fascinated the West, other literature has yet to be seriously analyzed. Since neither Professor Trager nor any other American-born scholar can really use Burmese easily, it remains for the next generation of students to fulfill the promise of "understanding Burma" which can only come through a command of humanistic studies.

JOHN BADGLEY

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#### **Abuse of Psychiatry for Political Repression in the Soviet Union.** By United States Senate, Committee on the Judiciary. (New York: Arno Press, 1973. Pp. 257. \$10.00.)

This volume of personal testimony and documentary materials is a reprint edition of the record of a hearing held before the U.S. Senate Judiciary Committee's Internal Security Subcommittee on September 26, 1972. At the hearing, testimony was offered by the logician, poet, and mathematician Alexander S. Yessenin-Volpin, of the faculty of Boston University, who has been incarcerated in Soviet psychiatric facilities on five occasions as a result of his activities as a political dissenter. He was permitted to emigrate in the summer

of 1972. His final incarceration was prompted by his application for a U.S. visa when he was invited to an American university to attend a scientific meeting; his release was effected as a result of a protest by ninety-five Soviet scientists, most of whom were from the University of Moscow and subsequently suffered reprisals.

The bulk of the hearing materials consists of documents most of which (eight cases) were gathered by Vladimir Bukovsky, himself a prisoner, who appealed to Western psychiatrists in 1971. There are additional materials from *samizdat* sources, including a report by Zhores Medvedev and an open letter by Aleksandr Solzhenitsyn.

One quarter of the volume deals with Major General P. G. Grigorenko who taught at the Frunze Military Academy and who is one of the more prominent Soviet political dissenters. The documents include the text of the speech he was to deliver before a Tashkent court in defense of the deported and persecuted Crimean Tatars—one of the activities that led to his incarceration. Judged on the basis of his own statements and writings, Grigorenko hardly appears to be a mentally ill person. Two commissions of psychiatrists disagreed in their findings regarding Grigorenko. The Tashkent Commission, which examined him for three hours on August 18, 1969, found him to be in good mental health; the other commission, at the pliable Serbsky Institute of Forensic Psychiatry in Moscow, met with him for twenty minutes on November 19, 1969 and advised that he be incarcerated. Included in the volume are open letters of Mrs. Zinaida Grigorenko describing the abuse and mistreatment that he is said to have experienced at the Serbsky Institute and in a military prison in East Prussia—characterized by her as “slow murder.”

The volume contains personal accounts by a number of detainees as well as the texts of reports of psychiatric commissions, letters, protests, and appeals. The official reports and other materials provide evidence regarding the kinds of “symptoms” of “illness” relied upon by some Soviet psychiatrists. These include: “behaved with an over-high opinion of himself, without any self-criticism,” “obsessive fears and persecution mania,” and “reform delirium.” Two detainees were asked why they had beards. Some of the alleged acts of deviant behavior that led to detainment and incarceration include the Red Square demonstration of August 25, 1968 (Nataliya Gorbanevskaya and Viktor Feinberg), active propagation of religious belief (Gennady Shimanov), record-

ing one's philosophical views regarding alternatives to Soviet rule (Vassily Chernishov), distributing one's own satirical verses (Anatoly Ponomarev), attempting to leave the country by illegal border crossing (Vladimir Shleshev and Anatoly Chinnov), defending General Grigorenko (Vladimir Gershuni), “anti-state utterances” (Vladimir Borisov), publishing abroad (Boris Yevdokimov), and signing protests (Leonid Pliushch).

Soviet psychiatry emerges from these documents with a tarnished and compromised reputation. Hasty examinations and superficial “diagnoses” result in excessive use of “schizophrenia” or “chronic schizophrenia of a paranoid type” to describe the condition of detainees. Able to identify “psychopathic personalities” by fiat, certain Soviet psychiatrists have become auxiliaries of the security police, willingly identifying new “syndromes” that require hospitalization. Political “patients” and detainees have no means of appealing the decisions of medical commissions. Injections of drugs given to prisoners are often detrimental to their health; prisoners are also threatened on occasion with being placed among severely disturbed patients. The prisoner becomes a “cured patient” if he renounces his views. The sculptor Mikhail A. Naritsa has summarized Soviet political psychotherapy very succinctly: “The main method of treatment is intimidation. The main symptom of healing is repentance. The treatment may last as long as they wish. One can always say, ‘he is not well yet.’” (p. 189).

The Soviet leaders can undoubtedly provide a rationale for these practices. With the connivance of unethical psychiatrists, political dissidents can be incarcerated without a public trial or judicial proceeding of any kind and cannot challenge the finding of mental illness. Dissidents can thus be deprived of the opportunity to speak critically using the political trial as a tribune. Dissidents can be given debilitating drugs under the guise of “therapy,” and it can also be reasoned that the dissent movement will be discredited more readily if enough of its advocates are certified as “mentally ill” after being silenced by essentially administrative action.

The materials in this volume make for depressing reading. Apart from the human suffering conveyed in the personal accounts and the abuse of governmental authority, there is the abdication of professional responsibility and abandonment of medical ethics by certain Soviet psychiatrists and other medical personnel. The personal accounts on psychiatric practice and on conditions in Soviet mental

hospitals—especially these by Grigorenko, Shimanov, and Gorbanevskaya—indicate low professional standards. It is a dubious achievement to equate political dissent with mental disorder. One is prompted to ask: Is this creative development of a new tenet of Soviet Marxism-Leninism?

JOHN S. RESHETAR, JR.

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**Laos: War and Revolution.** Edited by Nina S. Adams and Alfred W. McCoy. (New York: Harper & Row, 1971. Pp. 482. \$15.00.)

This volume, published under the auspices of the Committee of Concerned Asian Scholars, offers varied fare in its 30 chapters and nearly 500 pages, ranging from thoughtful, well-researched essays to trivia. Written while the U.S. was deeply involved in the raging war in Laos, the book was politically aimed at condemning U.S. intervention. This theme pervades most of the articles, giving consistency to the collected contributions, and marks the book as an example of the prodigious efforts mustered by many young Asian scholars to alter U.S. policy in Indochina. Despite this admirable effort, it must be noted that a number of the essays were perishable when first written (for example, "A Liberated View of the War: American Businessmen, Laos . . . and Beyond"), and even the most thoughtfully researched have a measure of contentiousness which reduces their value for those who are searching, as the passions of war diminish, for more sober evaluations.

Several background articles dealing with the land, the people, and the historical development of Laos are very good. Two French anthropologists, Condominas and Moréchand, offer useful brief ethnographic profiles. John K. Whitmore's thoughtful chapter on the Thai-Vietnamese struggle for Laos in the 19th century illuminates both the historic Vietnamese concern over hostile forces manipulating the upland populations of Laos who spill over into the hills of North Vietnam, and the Thai concern that the Mekong Valley territories bordering Thailand remain in friendly hands. These historic observations remind us of the importance of a Thai-North Vietnamese accommodation on Laos for attaining regional stability. A well-written essay by Alfred McCoy on French colonialism in Laos from 1893 to 1945, drawing abundantly upon French sources, makes the point that the French viewed Laos as a pleasant backwater, and shows how little effort they devoted to the development of the country. This light French intervention, and the resultant slow pace of

modernization in Laos, compared to its Vietnamese neighbor, help to explain the slower growth of nationalism in Laos. An interesting essay by Nina Adams which explores patron-client relations reveals how the Lao elite, recognizing their weakness compared to more powerful external powers, have linked themselves to outside patrons, the French, Japanese, Thai, Vietnamese, and Americans. As this article reaches the contemporary scene, however, objectivity fades and the central theme obtrudes. Adams suggests that the Vietnamese have been good patrons while the Americans have been bad for their Lao clients. Although it is true, in this reviewer's opinion, that the tragic U.S. intervention in Indochina has had disastrous effects upon Laos, the North Vietnamese intervention has not proved an unmixed blessing. Unfortunately, the North Vietnamese role is not adequately analyzed anywhere in this volume.

More than half of the book is devoted to an evaluation of U.S. intervention in Laos. Two seriously researched chapters in this section, one by Jonathan Mirsky and Stephen Stonefield and the other by D. Gareth Porter, persuasively document and condemn the escalating, if clandestine, U.S. role in the conflict. Fred Branfman, in a forceful article aimed at exposing U.S. mischief in Laos from 1964 to 1970, diminishes the thrust of his arguments by trying so hard to minimize the North Vietnamese presence in Laos during this period. Most of the authors of this book see little distinction between U.S. policy from 1954 to 1960, when the U.S. manipulated internal Laotian politics in favor of right-wing elements, and the post-Geneva 1962 period, when the centrist tendency of Souvanna Phouma was supported. The authors show little sympathy for Souvanna whom they portray as becoming a bogus neutralist by 1964, joining with his former right-wing antagonists under powerful U.S. pressure.

In view of the sympathy of this book for the Pathet Lao—Noam Chomsky characterizes them in the introduction as the only viable nationalist political force in Laos—the section on the Pathet Lao is the most disappointing. Of a total of six chapters on this subject, two are verbatim interviews of a Lao refugee and a PL Director of Information in Hanoi, conducted by Chomsky on flash visits to Vientiane and Hanoi, with little background on the interviewees and no analysis of their observations. Another verbatim interview of a North Vietnamese party official, reported by an Australian anthropologist, could, like Chomsky's interview of the PL official, be matched

by a random selection from translations of Hanoi radio at the time. The section is rounded out by a translation of a PL peace plan, a journalistic account of life in the PL zone, and—the most interesting—a short memoir of a Vietnamese adviser to Prince Souphanouvong in 1945. This thin assortment hardly justifies the promise of the subtitle that one of the themes of this book will be "revolution."

Despite its shortcomings, and particularly in view of the paucity of literature on Laos, this book will reward the careful reader with interesting information and opinions.

JOSEPH J. ZASLOFF

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**The Nutrition Factor: Its Role in National Development.** By Alan Berg, portions with Robert J. Muscat. (Washington, D.C.: The Brookings Institution, 1973. Pp. 290, \$8.95, cloth; \$3.50, paper.)

As political scientists become increasingly interested in policy issues arising out of development, works such as Alan Berg's *The Nutrition Factor: Its Role in National Development*, become increasingly important. The discussion and analysis of public policy cannot take place in a vacuum. Whether we are analyzing the politics of health programs in the United States or issues of development internationally, we need to have some insight into the relationship between the underlying technological and biological factors and the administrative programs that emerge to deal with grave social and economic problems.

The world problem of food and malnutrition is becoming much more pressing. Malnutrition is widely conceived as an obstacle to national development and, especially among children, is a cause of impaired physical and mental development, with adverse effects on education, productivity, and the working life span. Moreover, the complexity of the factors involved is such that in the short run, improvements in nutrition may cause an increase in birth rates, but in the long run improved nutrition may very well turn out to be a precondition for effective population control.

Berg, who prepared this study under the auspices of the Brookings Institution and the Foundation for Child Development, is a nutritionist who worked in India on nutrition problems and is now associated with the World Bank. The discussion, which reflects Berg's experience and concern, covers issues ranging widely from technology to the politics of nutrition. There are interesting chapters on malnutrition and development, nutrition and the

population dilemma, agricultural advances, problems in education for better nutrition, the crisis in infant feeding practices resulting from the decline in breast feeding, fortification of foods as a solution, the potential contributions of private industry and public programs, lessons from the Indian experiment, and a broad discussion of policy directions and program needs.

Although only two subsections of chapters are specifically labelled as dealing with politics—The Politics of Feeding Programs (pp. 176–179) and Nutrition and Politics (pp. 192–194)—much of the book deals with issues relevant to public policy analysis. The concluding chapter on policy direction and program needs is particularly provocative.

The link between American domestic needs and foreign policy approaches to food distribution is very briefly but incisively explored. Child feeding programs, for example, are described as "politically attractive, for they are direct, highly visible, and generally well received" (p. 176). These programs have had their origin in "relief efforts motivated by compassion, political attraction, and the need of affluent countries—primarily the United States—to dispose of surpluses" (p. 177). Our preoccupation with assuring that the donor of food is identified is shown to have caused antagonism and to reflect in part a lack of understanding of how nations are perceived in underdeveloped countries. One authority is quoted as saying "Some children who have learned that their daily school milk comes from America have told us that America is the name of a cow" (p. 177).

Berg appropriately notes that in the development of nutrition programs "at a certain stage, nutrition problems move away from the domain of technology toward the domain of administration" (p. 191). Among the problems of administration are issues of budgeting, management, logistics, and marketing but also political concerns such as resolving conflicting interests and promoting support. Another political issue is the widespread failure to build in adequate evaluation processes in a wide variety of public welfare programs. In part, reluctance stems from a fear that a concession of shortcomings in a program would jeopardize future funding.

Implementation of successful nutrition efforts is particularly troublesome. Many elements are involved including agriculture, health, education, and industry. Berg explores whether to fix administrative responsibility for nutrition programs on an interministerial council, on an existing ministry, or on a separate



agency and tends to favor a separate agency. Councils, he says, have proven to be ineffective; and an existing ministry, where nutrition programs may be under the control of vested interests "is unlikely either to give support or to have the breadth necessary for an effective nutrition program (p. 204).

Berg's book could not be more timely. With a new situation emerging out of recent changes in patterns of international food sales and problems of new areas of drought in such places as Africa, international food and nutrition policies require reexamination. Alan Berg's book is highly recommended as a fascinating study of the relationship of technical, organizational, and political problems in developing world nutrition policies. It is a major contribution to the necessary redirection of these policies and programs.

MARVIN LIEBERMAN

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**Czechoslovakia Before Munich: The German Minority Problem and British Appeasement Policy.** By J. W. Bruegel. (New York: Cambridge University Press, 1973. Pp. xiv, 334. \$14.95.)

**The Slovak Dilemma.** By Eugen Steiner. (New York: Cambridge University Press, 1973. Pp. ix, 229. \$13.95.)

The common theme of both works is not just Czechoslovakia, but more specifically the nationalities problem of that country. Professor Bruegel's book, first published in Germany in 1967 under the title *Tschechen und Deutsche* in a somewhat differing version, is a logical continuation of Elizabeth Wiske-mann's classical treatise of the same title, published in 1938. It not only goes beyond that date chronologically, but represents a distinct improvement because the author was able to draw on diplomatic documents published recently, including many Czech sources.

Again, the book is useful in presenting an abbreviated but cogent history of Czech political aspirations under Austria-Hungary and a lucid exposé of the first Republic's political and economic development. On the whole it gives a very favorable picture of that experiment and of President Masaryk's efforts to build a state that would be democratic, multinational, progressive, and humane. It points out that the German minority was gradually being won to constructive cooperation with the result that "until 1935 no parliamentary elections in Czechoslovakia ever showed less than two-thirds and sometimes as much as four-

fifths of the German voters adhering to parties with a positive approach to the State" (p. 33).

In addition to Czechoslovakia's internal development, the author discusses its external relations with Germany. Documents subsequently available from German archives reveal some interesting facts, such as the continued efforts of German envoys to Czechoslovakia to improve relations and to play down German nationalist ambitions. Thus, even Eisenlohr, appointed in 1936 by the Nazis, "confronted with the realities, . . . became a kind of advocate of the Czechs in Berlin" (p. 151).

British appeasement policy has been described by others, but it becomes even more misguided when read in the framework of Czech efforts to compromise and "appease" the Sudeten Germans. Perhaps one weakness of the book is that it makes no effort to analyze the sources of British behavior at that time. Surely the blame could not have been attributable solely to Chamberlain and his closest collaborators. There are, however, interesting sidelights: Vansittart who later embodied anti-Nazism is shown to have been on the side of appeasers of Henlein and Hitler before Munich.

The book ends, as indicated by its title, at Munich. It does not deal with the final chapter of Czech-German internal relations—the expulsion of three and a half million Germans from Czechoslovakia in 1945 after some seven hundred years of cohabitation and, only occasionally, confrontation.

Steiner's work deals with the other half of the country and specifically with Slovak nationalism. It has the advantage of carrying the analysis up to the present, and much of the book is devoted to what may be called nationalism under communism. It is indeed, as Professor Steiner tells us, "a paradox that nationalism should have played so significant a part in the development of a socialist society" (p. 2). Literature on Slovaks and Slovakia is rather meager in the West, and Steiner's brief outline of the position of Slovaks under Hungary which lasted for almost one thousand years, as well as its history from 1918 when they united with the Czechs in a common state (except for the brief existence of an independent, or at least separate, Slovak state between 1939 and 1945) is very readable. His most original contribution to our knowledge, however, is his description of the role of Slovaks since the Communist takeover in 1948. The author writes from experience as an active party member until 1968 when he left for England.

People may differ about the real significance of the Slovak national uprising in 1944 and some aspects of it are still a bit cloudy. The book does present an interesting account of the interplay between the government-in-exile in London, the Communist exiles in Moscow, certain elements of the Slovak state government in Bratislava and the grass-roots local underground.

Perhaps the most suggestive chapters deal with the Slovak roots of the Czechoslovak Spring of 1968. It may be true that "the Czechoslovak Spring of 1968 was preceded by the Slovak Spring of 1963" (p. 112), when a cultural protest movement erupted in earnest in Bratislava. From then on the several streams united: in the author's terms: "Slovak liberal nationalism and Czech national liberalism" (p. 120). Gradually the two took more distinct shapes: the Czechs wanted more democratization, the Slovaks more self-government. The story of an obscure Slovak *aparatchik* who more or less involuntarily became the symbol of liberalization in the person of Alexander Dubček, and the transmutations of a prisoner of the Stalinists into a tool of Brezhnev in the person of Dr. Husák is well told. It is ironic that the only remaining feature of the 1968 Spring is the federalization of Czechoslovakia and the creation of two republics, Czech and Slovak. Even that achievement is blurred by the fact that although the government has been federalized, the Communist party is as centralized as ever, and perhaps more-so, because the Secretary General, Husák, is a Slovak himself.

Underlying both books is the universal problem of integration and separation, especially on the ethnic level. Of the two contemporary tendencies, that of global fusion and of national fission, the latter seems the more prevalent. If nations and nation states have trouble sticking together, the cause of global (or regional) unification does not seem to have much chance.

FRANK MUNK

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**Patrick J. Hurley and American Foreign Policy.**

By Russell D. Buhite. (Ithaca, N.Y.: Cornell University Press, 1973. Pp. xiv, 342. \$14.50.)

With the proliferation of historical and biographical studies of American diplomacy during the Second World War and early Cold War it was only a matter of time before historians turned their attention to Patrick J. Hurley. As Franklin D. Roosevelt's personal representative to Chiang Kai-shek and subsequently

as ambassador to China, Hurley played a prominent role in the evolution of American policy toward China in the critical years 1944 and 1945 and was a central figure in the postwar recriminations over the failure of that policy. In this biography Professor Buhite analyzes Hurley's role in the "China Tangle" while illuminating the less-known aspects of a career as soldier, politician, and diplomat.

Professor Buhite traces Hurley's life from his "frontier origins" in the Oklahoma Territory through a prosperous law practice and service with the American Expeditionary Force in World War I, to prominence in local politics. His early support for Herbert Hoover led to a position as Secretary of War in which role he secured national attention for his suppression of the Bonus Army March in Washington in 1932. Exceeding his instructions to protect the business district, he directed the Chief of Staff, General Douglas MacArthur, to drive the protestors from the city and destroy their encampments. Criticizing Hurley's actions, the author observes that he misunderstood the plight of the veterans, overreacted in his use of force, and misled the public by emphasizing the radicalism of the demonstrators. The author is equally critical of Hurley's role in the controversy over independence for the Philippines. As the chief spokesman against independence, the Secretary of War reflected a paternalism and nationalism reminiscent of nineteenth century imperialism, leading Professor Buhite to conclude that the Oklahoman was insensitive to the aspirations of Filipinos and the political undercurrents running through Asia.

In the 'thirties, Hurley returned to his law practice, representing the Sinclair Oil Company in important negotiations with the Mexican government. Although a Republican, he maintained political and personal ties with the Roosevelt Administration and, after Pearl Harbor, served as the President's personal representative in the Southwest Pacific, Middle East, and Russia. His most important assignment was China where he pursued a policy of compromise and accommodation between the Kuomintang and the Communists. Historians have characterized Hurley's approach as inflexibly anti-Communist. Professor Buhite, in a lengthy analysis of the China mission, argues that the envoy accepted the need for military unity in China and conscientiously pursued accommodation. He demonstrates that, at least in the early months of his tenure, Hurley was neither irrevocably committed to the Kuomintang nor particularly anti-Communist and rightly observes that the policy of support for Chiang Kai-shek reflected the exigencies of war and

the personal preferences of both Presidents Roosevelt and Truman.

An envoy, however, is more than a passive instrument of policy; his reports and recommendations often determine policy. This was Hurley's greatest failure and the source of his celebrated feud with the young China specialists on his staff who sought to reverse the open ended commitment to Chiang's regime. In his attempt to be fair the author obscures the fact that Hurley's suppression of his staff was, at best, impolitic and, at worst, a scandalous abuse of his responsibility to analyze objectively the political situation in China. The author's criticism of the foreign service officers for bypassing the ambassador and ignoring established policy is rather gratuitous given the facts that events demonstrated the bankruptcy of the established policy and (as both Barbara Tuchman and John Paton Davies have written) that Hurley abused his staff, refused to consult with his specialists, and rejected contrary advice and information.

In general, Professor Buhite offers an informed and objective analysis of a rather enigmatic figure, and his research reflects extensive use of the Hurley papers and other archival sources. This is a competent biography and a useful addition to the literature of recent American diplomacy.

DAVID J. ALVAREZ

*St. Mary's College of California*

**Russia in the Caribbean, Part One.** By the Center for Strategic and International Studies. (Washington, D.C.: Georgetown University, 1973. Pp. 36. \$1.00, paper.)

This monograph, based on a Caribbean conference held in October, 1971, under the auspices of the Center, is primarily of a prescriptive nature, which consists of a set of policy guidelines. The panel suggests that the postwar era of American predominance came to its end in late 1960s when the Soviet Union acquired an "effective and credible" second-strike capability and began to launch an active diplomatic, military and economic expansionist policy throughout the Third World. In the Caribbean, this policy is spearheaded by a naval expansion, carried through two distinct but interrelated strategies. The Soviets attempt to improve the strategic balance through the deployment of a nuclear submarine force out of Cuba against the soft underbelly of the U.S. strategic defense system. What alarms the panelists more than this, however, is a 20th-century Soviet version of gunboat diplomacy, a policy aimed at influencing the general political environment of the area by diplomatic overtures, naval demon-

stration, anti-American propaganda, and economic assistance.

Against these perceived realities, the panel suggests several policy guidelines, which may be characterized as a revised version of the Roosevelt corollary to the Monroe Doctrine. According to the panel, the U.S. should (1) stiffen her reaction to the Soviet expansion by resisting hostile communist and *other* anti-U.S. regimes; (2) seek an "understanding" with the Soviets for the mutual scaling down of naval activities in the Caribbean and the Black Sea; (3) normalize the U.S.-Cuban relations through a negotiated settlement of outstanding problems and reintegration of Cuba into the Hemisphere system; and (4) observe a conditional non-interventionist policy provided that a Caribbean regime does not receive a direct support from the hostile military power or attempt to subvert a pro-American regime. Such a noninterventionist policy, it is proposed, should apply to a regime which may adopt a different political system or ideological orientation "as long as they play a constructive role" (p. 22).

The gist of the policy recommendations is the proposal that the U.S. maintain a policy of nonintervention against any indigenous political movement which does not solicit Soviet politico-military support. This approach, it is said, is necessary to accommodate, rather than resist, rising nationalism. Yet it must be understood that the new nationalism is essentially an economic movement directed against nonindigenous economic forces, i.e., the United States private investors and traders as well as their local economic allies. Favorable responses to Soviet overtures by the new nationalists are natural consequences of the détente and their desire for economic sovereignty, a condition which would easily render the panel's theoretical differentiation either impractical or incredible.

HOON M. CHUNG

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**The South West Africa/Namibia Dispute: Documents and Scholarly Writings on the Controversy Between South Africa and the United Nations.** By John Dugard. (Berkeley: University of California Press, 1973. Pp. 584. \$28.50.)

John Dugard, advocate of the Supreme Court of South Africa and Professor of Law at the University of Witwatersrand, has put together a work of nearly 600 pages dealing with the South West Africa/Namibia dispute. Although he provides a capsule description and a brief history of this rich, sparsely populated territory, the central thrust of the book is an

extensive analysis of its history since South West Africa became a mandate under the League of Nations.

Carefully weaving official documents and scholarly writings, Dugard covers the political and legal developments from that time to the present, ending with his personal suggestion for a resolution of the dispute. His conclusion is straightforward enough: "If the parties involved are genuinely concerned about self-determination and the best interests of the people of South West Africa, or Namibia, rather than the promotion of their own ideologies, this [a joint UN/South African sponsored plebiscite] surely is the best course" (p. 542). Dugard is most eager that this plebiscite be "free," and he calls for a series of stipulations, including the freeing of all Namibian political prisoners and their forthright participation in the plebiscite (which would be held one to three years in the future in order to ensure maximum understanding of the choices by the electorate).

*The South West Africa/Namibia Dispute* is comprehensive but not complete and a South African cast inevitably creeps into some of the analysis. Is the dispute really "the international *cause célèbre* of the century" (p. xi)? Do the Khoi Khoi and San people have to be referred to as the Hottentots and the Bushmen? Does not the author's reliance on scholarly and official documents automatically undercut the positions of the Ovambo People's Organization and the later South West Africa People's Organization? Would the South African government really accept the prerequisites of a "free" plebiscite?

Still, when all is said and done, there is a great deal of material here, most of it well presented. Subsequent scholarly work in this area will have to wrestle with its conclusions and analysis, even though other authors may want to challenge many of its conclusions and its overemphasis on official sources.

CHRISTIAN P. POTHOLM

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### Religion and Soviet Foreign Policy 1945-1970.

By William C. Fletcher. (New York: Oxford University Press, 1973. Pp. 179. \$11.25.)

Professor Fletcher seeks to survey an aspect of Soviet foreign affairs previously neglected, "the role of religious organizations and individuals in Soviet foreign affairs" (p. 1). Religion's primary role, he avers, has been in molding public opinion abroad, image-building, and the "development and use of a vast array of non-propaganda activities designed to elicit a favorable reaction to the USSR, whether gen-

erally or in specific matters" (p. 2). Such a role has been essentially marginal in terms of the entire array of Soviet foreign policy activities.

The main success of religious activity abroad has been in the exploitation of the peace issue, particularly during the Korean and Vietnamese conflicts when Western opinion was receptive to such appeals. Fletcher describes this process in his chapter on the Prague Christian Peace Conference, a meeting in which a combination of an issue with broad appeal to many circles of society, a parallel Soviet policy goal, and an absence of direct Soviet involvement or dictation redounded to the Soviet Union's benefit. *How much* the CPC strengthened Soviet influence on the peace issue is left vague.

Fletcher is most satisfactory in describing philosophical antinomies between Soviet politico-religious mechanisms and religious doctrines abroad and analogous dilemmas at home. Islam, he contends, reduces distinctions between the sacred and profane, and even a Communist atheist from Soviet Central Asia may enjoy advantages in the Muslim world of the Middle East because in the Muslim tradition, the political-religious linkage is an advantage. The Muslim emphasis on social justice and the "righteous society and state" offers advantages to a power emphasizing these themes. Buddhism, on the other hand, is pessimistic about temporal existence and lacks an ethic of social improvement. This makes difficult any application of "broad theories of social aspirations" (pp. 74, 82). (Adda Bozeman's pioneering work on multi-cultural approaches to international law also exemplifies this type of analysis.)

Fletcher's discussion of the philosophical-political dilemmas of church and regime is also cogent. He asks how the Soviet regime can use religious themes and benefit from religious instruments abroad without strengthening religion and religious-based nationalism at home, and points out that the Islamic emphasis is particularly pertinent in this connection. How can the Russian church assist the regime's foreign policy purposes abroad, ostensibly to assist in the preservation of its slim institutional structure at home without undermining any remaining vestiges of independence or weakening its domestic base of support? How can it desire simultaneously to promote certain values and support regime foreign policy goals and survive a crisis like Czechoslovakia, particularly where brethren in Eastern European churches are involved? The dilemmas of "concordat" politics are not unique, of course, to the Soviet system, but the "contradictions become more acute" in a regime that officially proscribes religion.

Fletcher's command of his subject is less apparent in his discussion of specific questions of foreign policy and international relations, partly because of the inevitable difficulty in defining "influence" abroad. Thus, "influence" is at times reduced to the negative goal of using religious themes and organizations abroad without suffering undue disabilities from simultaneous religious persecution at home. There is also the ubiquitous "image building." Fletcher's description of the "continuing penetration of the Middle East" as one of the "most spectacular successes of the post-Stalin era" (p. 68) can hardly be accepted without qualification. The Soviets seem to be as much "the hunted" as "the hunter," alternately beguiled and bedeviled by the Middle East "sub-system." Nor can one accept his sharp distinction between "the actual situation" and "the current ideological assessment" in describing Soviet studies on Muslim areas before the middle 1950s (p. 73). Is there truly a "value-free," "objective" analysis of the Middle East, or was the Soviet analysis simply marred by ideological distortion? Fletcher offers no guidance on this question. He concludes that the Church deserves a share of the credit for helping to build a more favorable image of the USSR in the West (p. 95) without saying why it deserves such plaudits.

The work will be of value to students of Soviet "church-state" relations who have also benefited from Professor Fletcher's other works. It has something of interest to say on the "clash of cultures" question and contains some interesting observations on Soviet relations in Eastern Europe and the church role there. For those concerned with Soviet policies in the Third World, especially the Middle East, the work will be of less value, and it does not add much, in my opinion, to our existing store of knowledge about Soviet relations with the West.

ROGER HAMBURG

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**Confrontation and Accommodation in Southern Africa: The Limits of Independence.** By Kenneth W. Grundy. (Berkeley: University of California Press, 1973. Pp. xxi, 360. \$12.50.)

If in the aftermath of the Portuguese coup of April 25, 1974, Mozambique becomes independent under a black government, will there be a domino effect in Rhodesia, Angola, Namibia, and eventually South Africa? Could an independent Mozambique afford to stop the export of its labor to the Witwatersrand, deny to Transvaal rail traffic the port of Lourenço Marques, and do without South African tour-

ists? With the fall of white Rhodesia, is it likely that the Western powers will move toward an even more overt support of South Africa than now exists? What are the strengths, the weaknesses, and the prospects of white domination in southern Africa?

Professor Grundy's book contains a good deal of data pertaining to these questions. The author believes there is an urgent need to understand southern Africa's "inherent potential for violence" (p. xi) and the dangerous involvement of states outside the region. The final two pages of his concluding chapter express both "pessimism and optimism" about the future of white domination (pp. 300-301). This discussion begins with the judgment that "the situation, currently, is moving in a direction beneficial to the white governments," but after an appropriate warning that "over-pessimism" may be "as unwarranted as was the euphoria" of the late 1950s and early 1960s, Grundy concludes that "the southern African sub-system seems to be, for many reasons, a fundamentally unstable one."

There is much to praise in Grundy's skillful and comprehensive work, the first book-length survey by a political scientist of the southern African regional subsystem. The book should also be of interest to political scientists who are engaged in developing systemic theory in their study of international relations and "regional sub-systems," a term which Grundy prefers to "subordinate" state systems."

But if it is fair to judge the book as an analysis of the strengths, the weaknesses, and the prospects of white domination in southern Africa, the book is a disappointment. For this I blame the author's preoccupation with systemic theory. In seeking conceptual clarity, he focuses on the region and the discerning of intraregional patterns. In analyzing his data, he asks how a particular "patterned interaction" fits into the currently functioning subsystem, and describes and categorizes it, rather than asking how the data bear upon the survival of white domination. The domino theory, for example, is referred to, but its validity for southern Africa is not directly examined, although Grundy is not averse to futurology, as shown in his *Guerrilla Struggle in Africa* (1971). For the most part, despite identification of trends, the book is curiously static.

Indeed, one wonders how useful is the focus on the region since it excludes the giving of important attention to (among other extra-regional actors) the United States, Britain, and other Western powers that support the white-governed states of southern Africa. Focusing on interstate relations within the subsystem also

underplays the importance of the political and economic structure of power within each state, and the probable effect upon that structure of major changes outside it.

Over half of the concluding chapter (and much of the theoretical appendix) is concerned with problems of defining the southern African "border," "membership" in the subsystem, and criteria for delimiting it. Fortunately, Grundy's realistic good sense enables him to surmount his theoretical preoccupations. For example, he recognizes the sterility of the "boundary" issue, noting that in southern Africa policies and not boundaries lead to conflict. The book itself proceeds from the outset, though somewhat apologetically, to define southern Africa "arbitrarily . . . in broad geographical terms" (p. xiv): South Africa and Namibia (South West Africa); Rhodesia (Zimbabwe); the Portuguese territories of Angola and Mozambique—all ruled by whites—and the black-ruled states of Botswana, Lesotho, and Swaziland, linked to South Africa in a customs union; the Malagasy Republic (Madagascar); and (north of the Zambezi) Malawi, Zambia, Zaïre, and Tanzania.

Grundy is keenly aware of the difficulties facing the researcher whose interest is southern Africa. Although concerned that his study may be "less than methodologically rigorous, at least in terms of modern political science," he points out that "the unavailability of comparable over-time data" makes an eclectic approach necessary (p. 303). One may add that adequate field work is also difficult to accomplish, and access is sometimes impossible (for example, to South Africa, which denies visas to some American academics while the United States grants visas to all South African academics who apply). Grundy gathered data during visits to southern Africa, including South Africa, in the late 1960s. Through careful use of press and documentary sources, his writing is generally up-to-date. One lapse is the lack of recognition of Botswana's mineral wealth and potential for development.

Of particular interest is his description of South Africa's economic hegemony and dynamic thrust into the region. Based upon this are economic, transport, military, and diplomatic networks—and flows of labor—that serve largely to bolster the *status quo* and to place constraints on neighboring black states. The book also describes the international relations and problems of guerrilla activity, the generally futile efforts of a few black states to "build bridges," and South Africa's so-called "outward-looking" policy toward black Africa. This policy, now in decline, is well described as

one that strengthens *apartheid* by sowing confusion among some African states, improving South Africa's image among Western policy-makers, and encouraging wishful thinking among South African whites.

Southern Africa deserves higher priority, including scholarly priority, in the United States. It is hoped that Professor Grundy's book will contribute to redressing the balance.

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**The Dominican Intervention.** By Abraham F. Lowenthal. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 246. \$10.95.)

The international crisis which resulted from the revolution and near civil war in the Dominican Republic in 1965 was quickly overwhelmed in the minds of many scholars and policy makers by the simultaneously escalating developments in Vietnam. Few scholarly book-length treatments have appeared, though articles and chapters of books have been numerous. Now a slender volume has come from Abraham F. Lowenthal, who lived in Santiago, D.R., throughout the entire crisis period and thus developed numerous resources unavailable to others.

Lowenthal has chosen to make a case study of the process of foreign policy making up to May 3, 1965, a date when the intervention in the revolution by the U.S. became a full-scale military as well as political operation. Within its limited scope (a matter to which we shall return), the work is on the surface a balanced and insightful presentation of valuable historical data, and a very fine analysis of the process which produced the U.S. intervention, in which Lowenthal sheds much new light on developments in both Santo Domingo and Washington. Beneath the surface, however, lurks a problem of methodology common to many studies of decision-making in national security affairs: the use of many private and secret documents and interviews without attribution, and in some cases without *any* identification.

Of about 150 persons interviewed, some of them intensively, eleven refused even to be listed in the appendix, and most of the others talked on a "background" basis. What the author refers to as "a great deal of written material" (p. vii) is paraphrased and unidentified as well. Lowenthal comments that this approach was "the price for obtaining a great deal of previously unavailable information" and one the reader will have to share. He is aware of the consequences and issues that this raises, and assures us that he gave much thought to

the problem, especially after reading Theodore Draper's essay on the subject. The author states, however, that he must stand by his original commitments. The problem, therefore, remains fundamentally unresolved.

Lowenthal minimizes its effects by judiciously balancing conflicting points of view and by his use of such terms as "reportedly" and "apparently" as signals to the readers. The author, however, is not primarily a journalistic reporting an "inside" story, but a scholar presenting a book with a thesis which belongs in the realm of science. His colleagues are ultimately unable to determine the validity of that thesis without access to much of the data upon which it was based. Since Lowenthal restricts the thesis to the Dominican situation and makes no claims about its applicability to analogous situations, the problem is further amplified.

Assuming that the data presented are as complete, balanced, and objective as possible (they certainly appear to be, based upon their relationship with information already known and their own consistency and coherence), we may then consider the author's hypotheses concerning the foreign policy-making process. He begins his analysis by pointing out the weaknesses of the three prevailing schools of thought: the "official," the "radical," and the "liberal." Their common and most fundamental failing is reliance upon a "rational policy model" which evaluates actual behavior in terms of clearly stated and understood goals. In summation of his own thesis, Lowenthal states that actions are shaped by "perceptions and information, by priorities and procedures of different actors in the policy-making process, by bargaining and politics, by external pressures and internal strains, by conflict resolution and consensus building. Competition, compromise, coalition, and confusion combine in uncertain ways to produce occurrences we term foreign policy" (p. 149).

Lest we infer that he concludes that no real thesis is possible, i.e., that neither rules nor predictability apply, the author quickly adds that the actions in the Dominican case were the result of a definite pattern of attitudes, processes, and behavior with fundamental and identifiable causes. He points out three: (1) a set of assumptions about the environment, in this case the location of the Dominican Republic, the character of its political and military leaders, the threat of a "second Cuba," and the undesirability of obvious intervention in the internal affairs of a Latin American republic; (2) a "tendency to decide as little as possible at each stage of the crisis and to postpone difficult choices" (p. 157); and (3) a tendency to

"act on the basis of extremely optimistic appraisals and predictions" about the situation (p. 159), meaning the position of our "friends," of course. These factors conspired to result in the massive intervention which came four days after the crisis began.

The thesis and its probable applicability to other crises seem almost beyond question. I can even see its relevance to understanding the behavior of the Nixon administration in the "Watergate" situation. While its very simplicity is a mark of its validity, some will argue that it lacks theoretical substance and rigor, or that it explains nothing because it attempts too much. Certainly the thesis quite properly accounts for much uncertainty and unpredictability about specific actions in this or analogous cases. It would seem, however, that scholars utilizing the thesis will be able to discuss probabilities of certain courses of action, and to understand why some events do or do not occur. The question still open concerns the balance between predictable and unpredictable factors in any theory of political behavior.

A final criticism, however, relates to the problem of the data. The limitation on the period covered leaves the vast number of subsequent developments untreated, and thus unrelated to the author's thesis. This is especially regrettable in the case of this crisis about which much remains unknown, and about which Lowenthal seems to have much new information. Those of us who are Latin Americanists or students of international politics, and who have focused on the Dominican crises and the many actors involved in its eventual settlement, would have to agree that, as good as the data available from documents and other books are, Lowenthal's treatment of the subsequent developments would have been very useful.

N. GARY HOLTEN

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**China and America: A Bibliography of Interactions, Foreign and Domestic.** Compiled by James M. McCutcheon. (Honolulu: University of Hawaii Press, 1972. Pp. 75. \$4:50.)

Professor McCutcheon's bibliography vastly underrates the existing knowledge of Sino-American relations. It is a disorganized mixture of antiquated and recent or still useful works. Frequently an author's major contributions are omitted and some minor article cited. Basic monographs published years ago are listed as "unpublished doctoral dissertations." Topics vital to an understanding of Sino-American relations are ignored, and no effort is made to relate Sino-American relations to the world and domestic events of which they were essentially

an integral part. The most positive feature of the bibliography is its strong emphasis on Chinese communities in the United States. But here it suffers from competition in a field of great current popularity in which many similar bibliographies are being produced.

DOROTHY BORG

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**Conflits et Coopération entre les États, 1971:**

**Prélude à un nouvel ordre international.**

Edited by Jean Meyriat. (Paris: Armand Colin, 1973. Pp. xiv, 285, 63 F.)

In this volume, produced by the highly regarded *Centre d'études des relations internationales* of the *Fondation nationale des sciences politiques*, thirteen French scholars have contributed a wide array of essays that are linked by the common purpose of analyzing the mechanisms of either a major and recent episode in international relations or the behavior and motivation of a leading protagonist. Although the authors largely confine their analysis to events of the year 1971, the editor, Jean Meyriat, justifies this on the grounds that 1971 marked a watershed between the dominant postwar bipolar East-West pattern of international relations and a new international order of multipolarity and North-South conflicts. The claim of special significance for 1971 merely indicates another candidate for the title of "transition" year, a designation awarded and contested annually since at least 1963. Nonetheless, the current configuration of international relations does differ in significant respects from the dominant postwar pattern, and M. Meyriat can point to major developments in 1971 which constituted important shifts; the admission of China to the UN and the beginnings of a thaw in US-Chinese relations, major changes in US foreign policy, Britain's successful negotiations for entry into the European Communities, Brandt's Ostpolitik, and events in South Asia.

Each of the contributions has been prepared specifically for this volume: despite the divergent perspectives of the authors, most of the selections—all in French—are very high in their quality of description, explanation or analysis, and some of them are provocative. Jean Meyriat, who is Director of Research at the *Fondation*, notes in his introductory essay the prevalence of major international conflicts in areas formerly under colonial domination and then concentrates on the increased relevance of the North-South conflict. He justifies his analysis by stressing the growth of US-Soviet solidarity and finds that the two superpowers share a common interest in extinguishing local

wars or at least in preventing their spread. But Meyriat's assumption is incomplete, since it does not take sufficient account of other interests to which the US and USSR accord higher priority (cf. Soviet behavior in the Middle East prior to and during the October 1973 war).

One of the most provocative essays is that of Marcel Merle, whose thesis is that a new world equilibrium is leading to a heightening of internal tensions and a crisis of the nation-state. In particular, Merle sees three sets of international pressures—national rivalries, transnational movements, and global environmental constraints—all tending to paralyze national leaders' power of decision. While Merle gives excessive credence to the "Report from Iron Mountain" on the difficulties of coping with peacetime, he does provide convincing arguments about the dangers of defensive, nationalist, and isolationist reactions as each of the developed states tries to protect itself from international resource, economic, and monetary constraints. In essence, the decreased salience of Cold War tensions has allowed internal problems to become more manifest; whether the consequences may be as grave as Merle fears is another matter, but his is a broad-gauged and provocative essay.

Another contribution, that of Manuela Semidei, provides a useful foreign perspective on the Nixon Administration's foreign policy. Semidei is critical of President Nixon for denouncing isolationism, yet taking unilateral acts reminiscent of 1930s isolationism, and she notes effectively that moral and political irresponsibility can isolate the US as much as any putative unreliability. The virtue of Semidei's analysis lies in her focus upon the US problem of rationalizing its foreign policy with the lessening of its global anti-communism; certainly this helps to explain why bridge-building to the USSR and China has sometimes coincided with a worsening in allied relationships.

Of the remaining essays, several provide incisive and highly detailed analyses of individual conflicts. C. Tirimagni-Hurtig offers a skilled and well-written contribution on "The End of Equilibrium in South Asia: The India-Pakistan War and the Victories of Mrs. Gandhi," in which she rebuts the idea of Indian expansionism and stresses the self-defeating nature of US policy in the region. Pierre Gerbet and Françoise de la Serre contribute detailed, balanced and useful discussions of Britain and the European Community: Gerbet's, an analysis of the negotiations of 1971; de la Serre's, a discussion of the British domestic setting. The book also contains an excellent chapter on



South America (by Alain Rouquié), a discussion of French foreign policy (Roy Macridis), and essays on the admission of China to the UN, the Soviet role in the Third World, tensions in the Arab world, details of the Berlin accords, and a striking set of geopolitical graphs.

Any generalization about this collection must be limited because of the divergent subject matter. The editor's contention that the authors share a common conviction that it is possible scientifically to analyze contemporary social and political phenomena is neither developed nor demonstrated in the individual chapters. Nonetheless, and despite the fact that some of the analyses have been outdistanced by events, the contributions are of generally high quality and basically carry out the authors' stated objective of picking out the present tendencies in IR and recognizing the direction in which these are evolving.

NANCY I. LIEBER

ROBERT J. LIEBER

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**The Next Phase in Foreign Policy.** Edited by Henry Owen. (Washington, D.C.: The Brookings Institution, 1973. Pp. 345. \$8.95, cloth; \$3.00, paper.)

Perhaps if the Department of State had maintained a full-scale policy planning function instead of scaling it down in 1969, the present collection of planning papers would have been produced routinely within the Department, rather than by the shadow government of Brookings. (I am not being entirely fair, of course; some basic conceptual thinking about U.S. foreign policy pre-1969 was done outside the government by one Professor Henry Kissinger.) But it is not surprising that Henry Owen, Chairman of the last full-fledged Policy Planning Council, should have produced this policy-options-and-forecast document outside the Department.

This book addresses questions essential to mapping the U.S. role: How will the world change? Will the U.S. accommodate to that change? How? The book organizes itself rather like the Department itself, discussing seven "regional trends" (Japan, Western Europe, East Asia, Middle East, Latin America, Soviet Union, and China); functional areas of economics, general purpose forces, and strategic arms; domestic opinion, and new forces in world politics.

In his brief curtain-raising essay Mr. Owen, correctly in my opinion, argues that established premises must be questioned. (It will not be unfair to evaluate the book's fidelity to that

injunction.) No consensus has yet been formed on the next phase, except that the United States should do less and rely on others (but not the Soviet Union) to do more. On the one hand the nation-state will function quite differently in the future; on the other hand it is still the best mobilizer of resources, and in the less developed countries represents a driving force.

Thus "new trends generate new needs and problems," but "these must be confronted in the context of ancient institutions and attitudes. An old epoch is dying, but a new one has not yet come of age" (p. 8). Others have also concluded that, in the words of Matthew Arnold, we are suspended between two worlds, one dead, the other powerless to be born. But the needed transformation is elusive to the bureaucrat who mistakenly believes he can control events, and to the theorist who overreacts to transnational phenomena and prematurely writes off governments as the prime actors.

The essays follow the editor's prescribed approach and thus have a kind of coherence not always found in collections. (Most sound as if they were finished in 1971; it is a shame they were delayed in publication.) The advantage they share is in being private-sector counterparts to balanced, prudent in-house analysis. The disadvantage they labor under is that by never thinking the unthinkable, they lose that extra dimension of imagination and boldness of planning that is free to challenge first-order assumptions rather than operate confined within the preconceived framework.

Morton H. Halperin's essay on Japan is thus sound, at least for the time when it was written, but it may not be able to help us very much if circumstances substantially change. The Okinawa Reversion and the decline of the American-Asian presence "makes the [U.S.-Japanese security] treaty seem less necessary" (p. 12). My own 1974 conclusion was that the shifts that have taken place in trade and energy sectors make the stability of the strategic-military environment more essential than ever.

Halperin's review of the main issues is sensible but a little dated; the nuclear option, so widely discussed no more than a half-dozen years ago, is no longer seriously talked about, and NPT ratification may be more likely than before. The issue of American bases is now dormant and, as with Western Europe, the personalized style of Henry Kissinger's diplomacy increasingly decides whether the nagging nonstrategic issues can be contained or will escalate. I agree completely with (and preached all over Japan in 1974) the virtue of multilateral routes to minimize some of the frictions arising in Japan's foreign presence.

John Newhouse produces a sound diagnosis of the weaknesses of NATO and Western Europe. His skepticism regarding French cooperation stands up in the light of French strategy of *sauve qui peut* during the oil crisis of 1973-74. Newhouse is skeptical of a Western European *entente nucléaire* and sees the present European security system as probably more cost-effective than any likely alternative. He may well be right, and my only quarrel is that he confines his vision to the near future, which deprives us of one comparative advantage of independent policy analysis.

Ralph N. Clough in his essay on East Asia seems to me too complacent and accepting of U.S.-China policy for the twenty-year period preceding the Nixon-Kissinger turnaround. He unwittingly confirms one's sense that U.S. policy noticed the Sino-Soviet split a lot later than it should have, and still tends to see Asia as "theirs" or "ours." But he makes an appealing case for a Southeast Asian neutral zone.

In one of the best chapters Robert E. Hunter writes prophetically about a more neutral United States acting as mediator in the Arab-Israeli confrontation. His perceptive analysis of the situation stood up very well in 1973-74. It is also one of the few contributions touching on the sociology of the region. Given the latter's implications for longer range U.S. policy, I wish the editor had included that dimension in his authors' checklist.

The essay on Latin America by Peter T. Knight and John N. Plank is first-class. It realistically recommends "creative pluralism" (p. 107) and sensibly calls for "constructive disengagement" (p. 108). It would have been interesting if the authors had considered a scenario in which the U.S. disengages from the OAS and operates vis-à-vis a less neurotic hemisphere, much as the U.S. is going to have to do in Western Europe.

With his customary gusto, Professor Brzezinski advances the theses of inherent Soviet instability (a "metastable state," p. 114) and intra-bloc contradictions that so endear him to the Kremlin. He also calls for a Western military edge for bargaining purposes. His optimism regarding détente is thus very restrained, but over time he sees it as demanded by the interdependencies of the worldwide "metropolitan political process" (p. 132).

Finally, A. Doak Barnett produces a characteristically thoughtful piece on China in a short-term perspective. Taiwan—the residual thorn in Peking's flesh—is increasingly forgotten as a prime issue, but Barnett usefully reminds us of it and recommends an "opened" U.S. policy (p. 151).

Turning to the functional areas, Edward R. Fried's piece on foreign economic policy is unsurprising. He assigns top billing to U.S. relations with Canada, Japan, and Western Europe, and looks to monetary reform as the key to developmental assistance. East-West trade he regards as nice but not decisive. One wonders if the enormous dislocation in foreign exchange ratios as a result of the fourfold increase in oil prices in 1973-74 will seriously affect his projections.

Leslie H. Gelb and Arnold M. Kuzmack restate the conventional wisdom regarding U.S. forward deployment, but leave unexamined whether the U.S. does in fact have the "responsibilities" they take as given. They usefully confront—and deny—the Fulbright thesis that military assistance invariably leads to unwanted political and strategic involvement, and their conclusion is that the U.S. should not reduce by more than one division.

Jerome H. Kahan likewise advocates a conservative strategy towards nuclear weapons, involving the air-sea-land triad, across-the-board redundancy, and the thinkability of nuclear war-fighting scenarios. With the exception of the latter, these are difficult to refute as practical counsels at any given time, but since they differ in no wise from Pentagon analyses, they are not particularly intellectually challenging.

Only in Seyom Brown's brief chapter are the "new forces at work" in the world actually named. Despite the editor's wise introductory remarks about change, only here is found any significant mention of the vast influence of science and technology on future world politics, including the increasingly politicized role of the biosphere, the transformed meaning of the seas for both economics and strategy, and the phenomena labeled as transnational. In this sense the structure of the book reflects what is wrong with governmental approaches: most officials work on traditional areas, while disconnected special assistants work on the "esoterica" that in fact increasingly influence traditional relationships.

In his introduction the editor anticipated three common conclusions: First, there has been a shift from the East-West focus to advanced nation relationships, with some North-South interaction. Second, economic issues increasingly "transcend" security issues. Third, these changes call for redirection rather than retraction of American efforts, as well as a new multilateralism to supplant the recent American role. In his concluding chapter Mr. Owen repeats that the age of nation-states, at least in the advanced world, is drawing to a close.

I am afraid that the book, with the exception

of Brown's chapter, does not either act on this insight or generate speculation that would illuminate (or refute) it. There is little evidence that policies adequate to the kind of change the editor speaks of are even considered.

Its generally high quality nevertheless makes it useful to the contemporary affairs student, and its consistency with official perspectives makes it a viable substitute for policy planning papers in the tradition of the former Policy Planning Council. The scenarios are surprise-free and, if the Spenglerians among us are right, the book misses some of the main points about the future flagged by its editor. Still, 'it will have to do until the U.S. government finally confronts its policy planning weaknesses, and until both planners and scholars acquire the second sight I unreasonably expect of them.

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**Toward the 21st Century: Education for a Changing World.** By Edwin O. Reischauer. (New York: Alfred A. Knopf, 1973. Pp. 195. \$5.95.)

Professor Reischauer's latest book is a strong, simple, and uncomplicated statement of the need for a global outlook based on the premise that "the world is becoming a single unit for mankind" (p. 75). Not only are parochialism and narrow nationalism obsolete for the times; an education which sustains them from generation to generation is an anachronism. Hence education for world community.

Yet Reischauer has no illusions that universalism will uproot localism in the next decade or two. If change comes it will occur with a new generation now embarking on primary and secondary school education. A restructured educational system must prepare the young to cope with universal problems such as the food-population crisis, worldwide environmental issues and overall demands of a world community, not by adding a course on Asia or Africa to the existing curriculum but by throwing the spotlight on mankind as a whole.

Reischauer knows how difficult this will be. Everywhere he looks communities less than worldwide are in trouble despite being drawn together by the forces of interdependence. Their problem is not their needing one another but breakdowns in mutual understanding. Writing of the new trilateralism, Reischauer warns "the real question is whether Europeans, Americans, and Japanese are ready to live with one another in such a community" (p. 126).

What is it that stands in the way? Military and economic problems as well as growth and change are one aspect of the problem. The

root cause, however, is differences in attitudes, outlooks, and traditions. Doubts, suspicions, and misunderstandings result. The Japanese treat shock diplomacy or new economics from the west as somehow bound up with the West's sense of superiority. "The Japanese are . . . conscious of racism in others because they are so profoundly racist themselves" (p. 127). But Americans are not without guilt. When a balance of payments problem threatened the American economy, Toyota somehow appeared more menacing than Volkswagen, Japanese textiles caused more uproar than Italian shoes, and Japanese economic strength is widely attributed not to efficiency and hard work but to exploitative low wages. For their part, the Japanese are equally obtuse in recognizing the economic problems of the United States. "Nursing their anxieties about the outside world and living in relative cultural isolation, they have proved imperceptive to the viewpoints of other countries . . ." (p. 123). And these deep-running misconceptions between Japan and the United States symbolize the deep gulf between the developing and developed world. They are the foremost obstacle to building the kind of world community in which mankind can resolve its most urgent human problems.

Reischauer's call for world community is welcome therefore, at a time when the number and quality of strong idealistic statements has shriveled. His proclamation is not without difficulties, however, and the analysis hesitates and falters at certain crucial points: First, while he acknowledges his limitations in primary and secondary education, he quickly goes on to propose more far-reaching changes in national educational systems than any country has achieved. One thinks of the slow and painful efforts to introduce more biracial material into American education and asks how far more fundamental changes in education for world citizenship are to be achieved not in one country but throughout the world.

Second, Reischauer has little patience with discussions of the balance of power, which he consigns to the eighteenth and nineteenth century. He shows that bipolarity in great-power relations is past and concludes that talk about a five-power balance today does more harm than good. His mistake is in confusing particular expressions of the balance of power with its necessary and inevitable manifestation in every political system. Checks and balances are a form of balancing power within the American constitutional system; other forms prevail internationally whether on a worldwide or regional basis. It is illusory to suppose that international order can be maintained in the

absence of ever-changing patterns of equilibrium among states. Statesmen must recognize these patterns and strive to formulate policies that channel international cooperation along practical lines given existing power realities.

Third, Reischauer asserts that the doubts and misunderstandings between Japan and the West foreshadow the conflicts likely to arise between the Third World and the West in the future. In one sense, his comparison is valid, but the example of Japan may also be misleading. Japan enjoyed a period of economic and cultural isolation denied to present-day emerging nations. Japan is rich in economic advancement and they are poor. Japan has reconciled a rather eclectic pluralistic system of values drawn together in Buddhist universalism with modernization. It is problematical whether others will be able to do this.

Fourth, Reischauer finds the grounds for world community in new economic and security factors, ignoring the fact that the moral foundations for community no longer exist. Cultures in an earlier era were united by consensus on religious, philosophical, and cosmological beliefs, as when Europeans were drawn together by a universal Catholic Church and the Latin language, and the same was true of coherence in the Muslim and Buddhist worlds. A firm cement of common faith and belief held each of these areas together, but that cement has long since crumbled. It may be that a new cement will take its place at the level of fundamental values. If Professor Reischauer is to make the case for world community, he should address this issue which is less tangible than economics or security but more compelling than new educational and curricular strategies. It is in a word discovering new moral ground for human solidarity assuming the old grounds have been weakened or passed away.

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**U.S. Foreign Policy and Peru.** Edited by Daniel A. Sharp. (Austin: The University of Texas Press, 1972. Pp. xxv, 485. \$10.00.)

The papers in this volume on relations between the United States and Peru were prepared in 1969 and 1970, under the auspices of the Adlai Stevenson Institute of International Affairs, The Chicago Institute of Foreign Relations, the Johnson Foundation, and others. Their presentation before government officials from Peru and the United States, as well as academic and business people from both countries, reflects an attempt by a

"citizens' constituency" to revive the relationship between Peru and the United States. Relations that were already cool deteriorated following the Peruvians' seizure, after a 1968 army coup, of a Standard Oil of New Jersey subsidiary and the sugar holdings of W. R. Grace and Company. Sharp intended his collection of articles to be particularly valuable to policy makers and specialists. Some of the contributors, such as Luigi Einaudi and William Mangin, had previously published on Peru. Even though the papers were prepared during the first months of revolutionary rule in Peru, essential elements in the strained relations—the Standard Oil and W. R. Grace cases—were not among those considered separately in this volume.

In his illuminating article on the armed forces, Einaudi pleads for a greater understanding of the political role of the Peruvian military establishment. He would have us believe that it does not fit any American stereotype of the traditional military. Einaudi states (p. 26) that many of the uniformed officers "may be more favorable toward social innovation" than the Aprista party, long considered the champion of nationalist revolutionary change in Peru. He proposes a "sensible military policy" toward Peru (p. 49), one which would be limited to the maintenance of "decent working relations on a technical basis" with the Peruvian armed forces. His conception of a desirable relationship includes unrestricted—as opposed to selective—sales of military equipment, the virtual elimination of grants, some training in military matters, and a curtailment of the military mission program in Peru.

David C. Loring contributes an indispensable analysis of the fisheries dispute. This outstanding paper is the only one followed by a critique, provided in this case by Luis E. Llosa, a former foreign minister of Peru. It is impossible, says Loring, to consider the fisheries dispute without handling the matter of Peruvian jurisdiction and sovereignty over the two-hundred-mile zone of contiguous waters. The complexities of the problem are presented by Loring: need for a regime regulating both use and exploitation of the seabed, ocean floor, fisheries, surface of the water, and also the air space immediately above. Loring suggests that the United States examine empirically all assumptions governing present U.S. policy on fisheries and maritime limits. None of these assumptions he states (p. 113), have been conclusively demonstrated to be valid. Llosa, on the other hand, recommends a series of very specific concessions by the

United States, in return for some general "compromise solutions" on the part of Peru (p. 124).

According to John Strasma, the United States should reject a "neutral" policy on Peru's agrarian reform, for it would reinforce pre-reform differences in power and income (p. 198). Strasma believes that even under the existing state of relations between Peru and the United States, positive, but "low profile," assistance would be accepted by the land reform program. Aid should be proffered either through multilateral lending agencies or by way of tangential programs, such as marketing facilities, he maintains.

The aforementioned lending agencies, themselves, have become controversial. John P. Powelson ably examines the charge that development assistance agencies were slowing down their aid to Peru because of pressure from the U.S. government. Powelson concludes that the slowdown was a consequence of the separation of powers, with the administration defending the lending agencies from the wrath of the U.S. Congress, which in turn was under pressure from the American business community because of the petroleum confiscations and fishing boat seizures in Peru. Powelson warns that a U.S. policy based on promoting the interests of specific groups of its own nationals would not be likely to encourage Peru to behave according to international standards (with respect to contracts, governmental continuity, and international law).

In a well-reasoned article, Bruce Blomstrom and W. Bowman Cutter urge the Peruvian government to delineate clearly the bounds for foreign investment. Once limits existed, Blomstrom and Cutter would insist that Americans investing in Peru live within them. Critical of the results of assistance that the U.S. government had given private investment in the 60s, the authors ask that, for its own good, the American government endeavor to remove itself from the investment process. This suggested removal of the U.S. government from the investment scene is predicated upon their hope that international agencies would have an expanded role in the resolution of conflicts arising from the presence of foreign investments.

William Mangin, an anthropologist, after taking note of the portents, maintains that "there is ample reason to expect a strong separatist movement [of Quechua- and Aymara-speaking Indians] in the Andean region" (p. 230). Mangin observes that past U.S. policy toward Peruvian Indians has been one of favoring and assisting their integra-

tion, which he feels is "a most unlikely possibility." The distinguished anthropologist advises that in the event of a separatist movement, disengagement from both the Indians and the Peruvian government would be an appropriate policy for the United States to pursue.

Labor relations are overemphasized in this volume. Neither William J. McIntire nor William A. Douglas, in an accompanying article, anticipates what has become a pervasive concern of Peruvian labor elements. Unionized labor now charges that the revolutionary government intends to replace traditional labor unions with newer, revolutionary instruments, namely its "industrial communities."

The book also contains a lengthy contribution by Robert G. Myers, on an appropriate assistance role for the United States in Peruvian education. In a peripheral article, Dan C. McCurry propounds policies for the regulation of U.S. church-sponsored activities in Peru.

Even though the statements in this volume are no longer new, U.S. foreign policy toward Peru has changed little since the provocative papers were presented. Peru has turned to the Soviet Union for deals in military hardware. The fisheries dispute awaits further universal development of the law of the sea. Private investment has not returned to earlier levels. Therefore the contributions retain much of their currency for policy makers and have lost none of their value for concerned academicians and students.

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**Basic Documents of African Regional Organization.** Edited by Louis B. Sohn. (Dobbs Ferry, N.Y.: Oceana Publications, 1971-72. 4 vols. Pp. 1854. \$35.00 each.)

This is a monumental four-volume collection of virtually all the fundamental documents governing institutional relations among groups of African states. It also places the current arrangements in historical perspective by providing relevant antecedent agreements, and by giving a systematic, analytic commentary of events leading up to each arrangement. Finally, the volumes present exhaustive bibliographies of books, articles and, most important, documents themselves—considerably more than can be reproduced in full, even in this exhaustive set. All these useful tools are arranged in accordance with a schema which takes a bit of getting used to, but is helpfully explained at the beginning of Volume 1.

The volumes cover the origins and evolu-

tion of the Organization of African Unity from 1958 forward, African Development Bank, French regional groupings (with separate sections for the general African and Malagasy, the equatorial and the West African arrangements), West African, East African, and Maghrebian regional structures. They vary in importance and subject matter from customs unions and river regimes to locust control networks. In the final volume, African associations with the European Community are set out and discussed, agreements frequently characterized not as regional but as inter-regional or even anti-regional.

It is extremely difficult, even (especially?) in Africa, itself, to have access to all the legal documentation which is so essential to the official and the scholar. The days when African colonies published and sold their laws through the colonial agents' offices in London are largely past and, in the hurly-burly rush to understaffed independence, most African foreign and attorneys general's offices have had to assign very low priority to systematic collecting, indexing, binding and revising of their legal heritage. All the more useful, then, is this remarkable effort by a nongovernmental scholar. Unlike so many other collections, this is by no means merely another siphoning into one container of materials quite readily available in many other places.

Ultimately, the usefulness of Professor Sohn's and Oceana's admirable and patient venture will depend on whether it can be kept up to date—perhaps by a supplementary volume once a decade—and whether it can be paralleled by similar endeavors for other regions.

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#### **Change and the Future International System.**

Edited by David S. Sullivan and Martin J. Sattler. (New York: Columbia University Press, 1972. Pp. ix, 109. \$8.00, cloth; \$2.95, paper.)

—"It does not appear possible, at least not within the next 30 years, that some new technological device will serve to neutralize nuclear weapons" (p. 10).

—"Likely technological developments, in a time frame of 10-20 years are going to generate important performance demands on the international system going well beyond, in scale and intensity, the requirements placed on the system today" (pp. 59-60).

These predictions typify the concerns of the six chapters in this short volume. The articles all deal with factors that will increase the demands for governance in the international system during the next 10 to 20 years. Each, however, examines the impact of different phenomena. Moreover, the predictions stem from quite different methods, ranging from historical analogy to implications drawn from the relationship of key concepts in equation-like formulas.

The chapters by Brodie, by North and Choucri, and by Skolnikoff deal with the impact of technology on international politics. Bernard Brodie reviews several concepts concerned with prediction, and then considers the impact of nuclear weapons and population growth. Nuclear weapons, Brodie argues, will remain a stabilizing factor in international politics reducing the likelihood of any kind of war between major powers. With respect to the population explosion, Brodie finds the trends "inevitable and worsening" but unlikely to lead to international wars because the problem is most severe for impoverished nations.

Robert North and Nazli Choucri take a different and possibly conflicting perspective on the population problem as it relates to technology and resources. They propose that a society's population and its level of technology combine multiplicatively to produce demands for resources. When a society's indigenous supply of resources is small compared to its population and technological skills, it exerts "lateral pressure"—usually some form of imperialism. Thus, according to North and Choucri, although the population explosion generates demands everywhere, it is from the technologically advanced societies that the growth leads to international problems.

Eugene Skolnikoff introduces the reader to an array of technological issues having international political impacts—weather modification, oceans, outer space, natural resources, food, and population. Weather modification receives the greatest attention. In his opening paragraph Skolnikoff argues that technology has exaggerated national sovereignty, proliferated small and weak states, and allowed the "traditional" approaches of governments to international affairs to work a little longer. He concludes that most of these effects will have to be reversed in the future.

Herbert Dinerstein uses historical analysis as a predictive base through extrapolation to the future role of ideology in alliances from its function in the 16th and 17th centuries. The rise of Protestantism and nationalism as counter-revolutionary ideologies is examined in

terms of their effect on alliances. With respect to the future, however, nuclear weapons become an intervening element preventing ideology from serving its traditional role in shifting coalitions. In the course of providing the historical account, Dinerstein offers a number of provocative observations. Example: "The historical accounts of the 16th and 17th centuries suggest that rulers then had better information than rulers now" (p. 45).

In his chapter on international organizations, William Coplin also sees ideological conflict as less probable in the future. He identifies two international bargaining processes. In conflict bargaining (which includes ideological divisions), the other party is defined as the problem whereas in collective problem solving the difficulty is defined separately from the parties to the dispute. Coplin believes that international organizations can reduce the frequency of conflict bargaining by isolating disputes from domestic political forces that he believes push bargaining into a conflict mode. (Interestingly, Skolnikoff contends that domestic interest groups will increasingly circumvent national governments and exert pressure directly on international organizations. Thus, he implies that international organizations will become an alternative arena for conflict bargaining.) Coplin suggests that international organizations can play three progressively more active roles in international bargaining—they can provide a forum; they can play a regulative role; and they can assume a distributive role in which costs and benefits are allocated among states. During the next 20 years, he predicts, international organizations will remain a forum and play a regulative rather than a distributive role.

Although the chapters described above have considerable diversity, they share an assumption that the major future sources of change for international politics are likely to be technology and the environment rather than such matters as ideology or great power wars. In a concluding chapter on race and international relations, Tilden LeMelle and George Shepherd jolt the consensus on sources of future problems. In contrast to the other chapters, human relations become the origins of future problems. The authors propose a deterministic and inevitable sequence for racially stratified societies. At first, such societies will generate considerable cohesion or "centripetal force" through "pseudo-assimilation," and police powers. Eventually, this stage is replaced by a "centrifugal" phase in which open racial conflict develops. The authors see this second stage as prevalent in the world today. The

present centrifugal stage creates such international behaviors as an increase in transnational racial ties, an increased concern with external intervention, and a tendency for existing regimes to become defensive, closed; and inwardly directed. In alternative third stages, states become uniraical, or a "pluralistic consensus" develops in which race no longer stratifies society. The authors make no predictions about the conditions leading to one or the other of these final phases.

As a collection, these essays stimulate thought about changes in international relations. The lack of explicit attention to the means for generating predictions, however, is troublesome. For example, LeMelle and Shepherd ignore the forces that move a society from the first to the second stage. Skolnikoff depends upon the citation of authorities—some of whose calculations are extremely controversial—for projecting technological and environmental developments. Dinerstein, after devoting the longest chapter in the volume to a review of historical factors, concludes with predictions based on recent technological developments. Such shortcomings may be attributable to the limited space allocated to each selection, or, alternatively, they may reflect the state of prediction about the international system.

CHARLES F. HERMANN

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**Communist Penetration of the Third World.** By Edward Taborsky. (New York: Robert Speller & Sons Publishers, Inc., 1973. Pp. iii, 500. \$12.50.)

Despite diligent effort, and an obviously sincere concern for his topic, Edward Taborsky, Professor of Government at the University of Texas, and a former personal aide to Eduard Benes, the late President of Czechoslovakia, has written a study of "Communist strategy" in the third world which fails to enlighten, which confuses and distracts, rather than clarifies.

For Mr. Taborsky, communism is not an objective condition created by the realities of the third world. It is not something which the third world itself chooses. It is an alien force, introduced and imposed from the outside. As a result of such assumptions, Mr. Taborsky does not focus on the way in which the third world itself produces a Communist response. He is concerned to tell us all the things that "communism" is doing to "exploit" the conditions of the third world, which is a "target" for "Communist" penetration.

Mr. Taborsky's difficulty is instructive, for

it is typical of a kind of methodological confusion found in many studies of communism, particularly those informed by a spirit of moral hostility towards communism. Mr. Taborsky's basic flaw is not that he oversimplifies communism, or fails to appreciate the significance of polycentrism, but that he has reified the word communism. It is this reification which creates the illusion of a monolithic "communism," and the confused notion that "communism is spreading."

Mr. Taborsky, naturally enough, focuses on an analysis of Soviet strategy in the Third World, since he assumes that the Soviet Union is the leader of "world communism." Beginning with Lenin's work on imperialism, Mr. Taborsky devotes an enormous amount of space to "Marxist-Leninist" analyses of the problems of the third world produced by Soviet ideologues, on the curious assumption, apparently, that such analyses "create" communism in the third world; that is, we are to assume that the Soviet Union "exploits" these problems for the sole purpose of "spreading communism," even though Mr. Taborsky does not relate Soviet activity to the specific state interests of the Soviet Union. Even more confusing, he freely equates the activities of communist parties in the Third World with those of the Soviet state; so that the victory of Mao Tse-tung in China is viewed as a victory for something called "communism," even though Mao Tse-tung's political success cannot be shown to have benefited the Soviet Union in any way; or to have benefited any political group, for that matter, except for Mao Tse-tung's own Communist Party. In that tautological sense, of course, Mao Tse-tung's victory in China, was a victory for "communism."

In fact, Mr. Taborsky is not very much interested in the condition of the Third World, only in the methods by which "communism" "exploits" these conditions. But who are these Communists exploiting the Third World? Is it the Soviet Union? Mr. Taborsky does not consider such a question, for although he identifies the Soviet Union as the leader of "world communism," he can find evidence in support of his analysis in the activities of *any* Communists, whether or not they have any links to the Soviet Union, and he uses the same terminology of exploitation, even when the Communists he describes are themselves Third World figures, whose "exploitation" consists of their attempt to solve their own problems. In fact, in Mr. Taborsky's curious conceptual scheme, China and Cuba are no longer Third World countries, for they are now part of the "world socialist system."

Mr. Taborsky laboriously explores the dif-

ferences between the "official" Communist line, which is, presumably, the position of the Soviet Union, and that of other Communist parties, in China and elsewhere. He concludes that their differences are not ideologically significant, without ever recognizing the obvious point, that the differences between the Chinese and Soviet Communist parties, for instance, reflect the fact that these parties exist to serve different interests. The Chinese Communist Party differs from the Soviet Communist Party for one overwhelming reason: it functions to serve the interests of the Chinese state, *not*, the Soviet state.

Mr. Taborsky's treatment of "communism" in the third world eventually produces the kind of "twilight zone" effect frequently found in such turgid ideological studies. The book shifts back and forth from a consideration of Soviet strategy to something called "communist strategy" (pp. 36 ff, 204 ff, 447 ff.). Communist "conspirators," predictably, are endowed with superhuman powers in the manipulation of the leaders of the third world, who are too "inexperienced" and "emotional" to cope with "Communist exploitation," while their peoples "possess a highly excitable mentality oriented toward emotionalism, inclined more toward anarchism than strict discipline" (pp. 28, 98, 140) although it is not clear why these questionable attributes of Third World peoples make them more vulnerable to manipulation by people who call themselves Communists than by people who call themselves CIA agents.

Concealed within the subject matter of Mr. Taborsky's book are some very real political issues. Communism in the third world is real. The "Communist penetration" which Mr. Taborsky reads into this reality is a semantic illusion.

BERNARD P. KIERNAN

*Concord College*

**West Africa's Council of the Entente.** By Virginia Thompson. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 313. \$14.50.)

Since Virginia Thompson's study was published, three of the five countries of the Entente have undergone internal upheavals: yet another Dahomean military coup unhorsed an ungainly presidential troika (itself a contrivance of an earlier military regime); the Upper Volta military retook power, sending the country's civilian government and legislature packing, and in Niger, the military also seized power from President Hamani Diori, ostensibly because his government had been unable to come to grips with the problems posed by the Sahelian drought. It would



be trite—and unfortunate—to dismiss Professor Thompson's solid study because events had overtaken her account; I think it fair to argue that anyone who read her book would not be surprised at these latest developments. Herein, then, one of the more important contributions of the study: it provides not only excellent background to an understanding of recent events within the five countries, but also places these events within the broader contexts of association with each other and with their other francophone and anglophone neighbors.

The author traces the origins of the Entente to an older dispute (1956–58) about how French-speaking West Africa was to be structured after the demise of the French West African Federation, looking toward new relationships between its components and between them and France. The “federalists,” championed by Senegal's Senghor (and echoed by various attempts at union involving Ghana, Guinea, and Mali), argued for a united, autonomous stand with lessened dependence on France and Europe; the Ivory Coast's Houphouët Boigny, co-author of the *Loi Cadre* and leader of the transterritorial RDA, formed the Entente in 1959 as a loose association that would simultaneously insure the Ivory Coast's dominance in its area *and* its close ties with metropolitan France. The author clearly indicates that the Entente has been—and remains—dominated by Houphouët; his personality, coupled with the economic and political strength of the Ivory Coast, has managed to preserve the Entente as an organization that provides minimal, but indispensable services to its members (financial assistance, brokerage for French and other foreign aid, employment for job-hungry northerners, etc.). The relatively spectacular economic success of the Ivory Coast (compared to its neighbors) has accentuated its dominance even further.

The Entente, however, has never become a true regional organization, a failing that must also be laid partly at Houphouët's door. Thompson demonstrates that the deliberately loose structures of the Entente, its inability to coordinate developmental planning and policy making, the relative weakness of the elites (low capability, lack of cohesion, personality clashes, etc.) outside of the Ivory Coast, the diffuse nature of group activity and goals, have all contributed (with Houphouët's help, to be sure) to keeping the Entente in a continuous state of semi-disarray. Houphouët was once reputed to have said that he did not want the Ivory Coast to become a “milch-cow” for its hungry neighbors; the Entente not only prevents that from happening, but puts the Ivory Coast in the happy position of being able to

dispense its benevolence as political rewards. The lesson—as Professor Thompson's lucid analysis amply demonstrates—is clear for those who seek interstate association in Africa: without ample structural underpinnings and firm elite consensus, a union of equals (much less an association of unequals) has little hope of success.

Among the better chapters in the book is that focusing on “Foreign Relations and Divisive Issues,” a useful compendium of recent problems and dilemmas confronting the Entente members. Relations with France are given due coverage, as are such matters as dual nationality, immigrant and migratory labor (a particular problem for the Ivory Coast, Upper Volta, Niger, and Dahomey), as well as particular issues such as relations with neighboring Ghana, the Biafran episode (the Ivory Coast was one of the few African countries friendly to Biafra) and Houphouët's ambiguous campaign to sponsor “dialogue” with South Africa.

For all the attention paid to the internal politics of the five Entente states, the book contains little information about the Entente itself as an organization—however diffuse or weak it may be. The entente remains, after all, a structure of interstate collaboration: details on that collaboration, the nature of whatever decision making takes place, the institutional arrangements that facilitate interaction, all remain somewhat vague or mysterious throughout the work. The Entente heads of state, or its ministers, do meet from time to time, and networks of interstate cooperation do function, however spasmodically. All this attests to the presence of a structure of interaction that gives form—and name—to the Entente; failure to give due attention to these matters tends to unbalance what is an otherwise admirable work.

In sum, with only the above small reservations, I commend Professor Thompson's book to as wide a readership as possible. There is little enough scholarship devoted to French-speaking Africa, and her book is a welcome addition to that literature.

VICTOR T. LE VINE

*Washington University*

**Environmental Policy: Concepts and International Implications.** Edited by Albert E. Utton and Daniel H. Henning. (New York and London: Praeger Publishers, 1973. Pp. ix, 266. \$15.00.)

The trouble with collected articles on environmental politics has been that they were usually fat with descriptions of environmental problems but lean on the discussion of politi-

cal aspects. This book concentrates resolutely on the political aspects of environmental degradation and thereby earns some distinction among a generally dismal collection of collections on ecological politics. The major problem is that these twenty-six short, but generally useful, studies on many facets of environmental politics have almost been made to appear a cheerless potpourri of scholarly trivia by utterly negligent editing—*almost*. If the reader will sample some articles, perhaps using the topical guide I shall suggest, he will find the material usually interesting and often stimulating if seldom exciting. It is a good collection of reference articles for scholars, classroom teachers, and students, one that raises a variety of relevant issues on many aspects of environmental politics.

This collection of articles from *Natural Resources Journal* (July, 1971, and April, 1972), each customarily running between 10 and 12 pages, covers (by my logic) five broad topics: international aspects of environmental politics (ten articles), general political implications (five articles), environmental law and litigation (four articles), the public and environmental politics (three articles), and problems of governmental policy-making (four articles). A number of the writers, including Grant McConnell, Lynton K. Caldwell, and Richard Falk will be familiar to scholars in public policy, while others such as Charles R. Ross (formerly a member of the Federal Power Commission) and Dean Arnold W. Bolle (Chairman of the University of Montana committee whose report on the Bitterroot National Forest precipitated a national battle over clear-cutting) had important practical experience in environmental policy making. Substantively, the most useful material for scholars is likely to be (1) the articles on international environmental control—an important topic about which scant research presently exists—and (2) the studies of legislative, administrative, and judicial policy implementation. On implementation, the articles by David Sive on power-plant siting, L. F. E. Goldie on pollution from deep-sea mining, and Ian Brownlie's on standard setting in general are instructive not only because they provide an operational understanding of the technical and political problems associated with policy implementation but because they are a healthy antidote to the sometimes unworkable political prescriptions urged by overzealous environmental reformers—a lesson in practical politics (and applied science) that's just the thing for students to read, for instance, before setting out to reform the system.

The international environmental articles, the largest and most varied group in the collection, are especially welcome because they discuss a number of specific matters including the protection of migratory animals, the special problems of environmental regulations in Africa, and administrative designs for international regulatory agencies, albeit more briefly than one would wish. Perhaps the least interesting material relates to environmental litigation, which has been extensively investigated by a number of experts in far more exhaustive studies, and the treatment of the general political problems associated with environmental protection; most of the latter is either a distillation of earlier, better studies by the authors, or (in the case of Senator Henry Jackson's opening piece) mainly exhortatory. In general, however, most of the material merits at least some study.

The most pervasive problem with the articles themselves is that they are too brief; they are often suggestive in outlining general concepts and problems, or in giving the reader a few selected insights into topics, but they seldom carry him into much depth. The material would serve best as a stimulating introduction to selected topics for laymen, students, and scholars, as a goad to the development of research topics and collection of empirical material, as a reference book suitable for library purchase or for the collection of a scholar with a continuing interest in environmental politics. Also, the material uniformly illustrates one of the major current problems in research on environmental policy: the almost total absence of carefully integrated empirical approaches to the major research problems associated with environmental politics. Except for Matthew Crenson's fine *The Un-Politics of Air Pollution* (1971) and a scattering of other studies, such material combining conceptual and empirical approaches is regrettably rare.

As for the "editing," these authors—any authors—deserve better. There are no introductions to the material, no topical sections into which articles can be organized for the reader's convenience, no attempts to inform the reader of the authors' often considerable authority or distinction on their chosen areas (as in the case of Charles R. Ross and Dean Arnold W. Bolle), no effort by editors or publishers to make a potentially useful set of studies actually useful. Well, the editors were not wholly negligent: they did include their own biographic entries while omitting those of the authors. It's that kind of job.

WALTER A. ROSENBAUM  
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**Strategic Energy Supply and National Security.**

By Carl Vansant. (New York: Praeger Publishers, 1971. Pp. 150. \$15.00.)

If you really want to know that the Navy bought 11,375,000 barrels of JP-5 jet fuel in 1963 and the Army 898,000 barrels of Arctic diesel fuel, and are willing to pay \$15 for that and a lot of similar information, this may be just the book for you. But if you are interested in the somewhat broader questions implied in the title of the book you had best turn elsewhere.

One of the starting points in looking at such questions is a comparison of military demands for petroleum products with U.S. total consumption. Vansant, an operations analyst in private practice, doesn't bother to make that simple calculation, and even makes it unnecessarily difficult for his readers. Like many others who write on energy, he uses inconsistent units, giving figures for national consumption in metric tons per year and for military consumption in barrels. He then goes the last mile in further confusing things by providing, without explanation, disparate figures for military deliveries (or procurements): 416, 339, and 393 million barrels (in Tables 15, 16, and 17 respectively), all for 1968. Using the intermediate value and making appropriate conversions, one can calculate that military consumption of petroleum products in 1968, a war year, was about 10 per cent of civil consumption. With U.S. imports at about 20 per cent of total consumption at the time, it would seem that the nation might make do with belt tightening in an emergency. One is led, then to a further series of questions. Will the situation change significantly during the next few years? How much would military demand drop during peacetime, or increase in a "hotter" war? How long could the military operate on reserves? What fraction of civil demand would be essential to effective conduct of a war? Are there critical products that the military might require which would pose special problems if foreign oil were unavailable? Alas, Vansant doesn't address such questions. Nor does he treat such issues as the impact of increased oil prices on the economy and political stability of oil-importing countries, presently a critical security issue in the case of Italy and India and potentially one for many other states as well.

Next, there are the omissions. There is nary a word in the book about whether Alaskan north-slope oil will make a difference in the future U.S. oil position or North Sea oil in that of the U.K. Oil shale and tar sands are merely mentioned, with no estimates of

amounts of oil that might be obtained from such sources or of cost factors; and geothermal power is not treated at all. Generally, single estimates are given for fuel reserves, when in fact the estimates are critically dependent on assumptions about what costs of exploitation are acceptable, and otherwise vary enormously.

Third, the book is not without errors of fact. Did you know, for example, that the population of Israel is half the population of the United Arab Republic (p. 9)?

Finally, there are a host of other problems, some, but not all of which, may have their antecedents in the book's obsolescence. Published in 1971, it is now hopelessly out of date. The author observes (p. 4) that "As far as oil is concerned, the world is clearly divided down the middle—East and West." This is clearly not true, and it is doubtful that it could have been reasonably so described three years ago. As a minimum, one would do better to divide it into four groups: the western-industrial importing states, the OPEC nations, the oil-importing developing nations, and the Soviet bloc.

On page 11, Vansant observes "The Russians got a fantastic bargain for their money [in Egypt]." That might have been a reasonable judgment in 1971 although there were signs as early as May of that year that Sadat would resist Soviet domination. Certainly few would now argue that the Russians have gotten a good return on their investment.

When I agreed to review Vansant's book I did so because I thought, and still do think, that the resources issue may be displacing ideology as the major source of instability and conflict in international relations. I had hoped to read something that was informative and thought-provoking, but these hopes were disappointed.

G. W. RATHJENS

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**Russia and Asia: Essays on the Influence of Russia on the Asian Peoples.** Edited by Wayne S. Vucinich. (Stanford, Calif.: Hoover Institution Press, 1972. Pp. 521. \$15.00.)

This is a most useful volume offering a broad review of Russia's role in Asia, even though it concentrates primarily on the impact of this role on the Asians now within the Soviet boundaries and on the Russians' perception of this role. A treatment of most of the neighbors of the USSR (only two are included here) must await another volume. As in most symposia, contributions are not always even in quality, but the central theme is well de-

veloped. An impressive wealth of data, in the text as well as in the 96 pages of exceptionally comprehensive footnotes, is a major strength of the book. It is an invaluable source of factual information on a little-known subject and frequently covers virgin territory. Its major weakness, I believe, is its relative absence of analysis (with few notable exceptions such as chapters by Mark Mancall and Alexandre Bennigsen) and its relative concentration on the prerevolutionary and early Soviet periods. Only four chapters (by Vucinich, Bennigsen, Lang, and the Dunns), treat the post-Stalinist period in depth. This is unfortunate because, as shown by recent scholarship, it is only in this period that the impact on Soviet Asians of Russian culture, modernization, and sovietization began to emerge into a new synthesis. Thematically the book has three parts: three chapters are devoted to the Russian view of Asia and to Russian and Soviet orientology; five chapters review Soviet Asian groups; two chapters discuss Russia's impact on the two most important of its Far Eastern neighbors: China and Japan.

In the first group Nicholas V. Riasanovsky contrasts the ancient root hatred of Asian invaders by the Russian masses with Europe-inspired imperialism and sense of *mission civilisatrice* of the post-Petrine educated classes, and with the fascination on the part of groups of creative intelligentsia at the turn of the century with the new integrative Eurasian concept. In the follow-up discussion of Russian Oriental Studies, Richard N. Frye, a noted Iranian rather than Russian scholar, offers the controversial views that Mongol invasion neither cut Russia off from the West nor redirected its destiny eastward (pp. 31-32) and that the future of Oriental studies in the USSR is "in the hands of classical scholars or academics" (p. 51). No refutation of the latter view is needed here as the reader then progresses to the next chapter (Soviet orientology) by the volume's editor, in which the political nature of governmental sponsorship of Soviet orientology is exhaustively documented, without detracting from the quality of its scholarship. Truly encyclopedic, Vucinich's chapter escapes being a catalogue of institutions, resources, scholars and bibliographies of Soviet orientalia, because of the author's knowledge of historical, political and cultural context. The five specific chapters, all informative and well-documented, are uneven in analytical emphasis and in period coverage. They treat Moslems of European Russia and the Caucasus (Alexandre Bennigsen), Armenia (Vartan Gregorian), Georgia (David M.

Lang), Central Asia (Manuel Sarkisyanz) and Peoples of Siberia and the Far East (Stephen P. and Ethel Dunn). One might wish to have a more differentiated and more recent coverage of the five major groups of Central Asia. Had the post-1956 period been included, perhaps the statement that "no original Uzbek thought, literary or political, has shown itself since the Time of the Purges" (p. 284) would have been omitted. One also finds Lang's characterization of Soviet policies in Georgia as non-integrationist (p. 245), somewhat optimistic in view of the 1973 fall of Mzhavanadze, "a very fine patriot and gentleman" (p. 239).

The last part of the book gives a comprehensive survey of Russo-Japanese relations by George Alexander Lensen, and Mark Mancall's analysis of "The Structure of Contact" between Russia and China. Contrasting the discrepancy between the nonessential nature and volume of Russo-Chinese political and economic contacts and the far-reaching impact of Russian influence on the modern Chinese political culture, Mancall advances a theory that the change in the Chinese worldview took place because of the preoccupation of Chinese intellectuals with the classical Russian literature and their identification with oppression described. (The impact also came with ideological communist writings, many transmitted by the Russians, if not of Russian origin, pp. 329-331). It is interesting that Lenson makes the first point also, in application to the Japanese intellectuals (pp. 364-366). Mancall argues that the Russian concept of synchronic conflict explaining social conflict and discontinuity, hitherto unknown to the Chinese paradigm of universe of harmony and endless repetitiveness, has added new dimensions to this paradigm, with profound results for the Chinese society (pp. 332-336).

In summary, the book adds to the still little known area of the impact of Russians in contiguous Asia and is an important source of instruction for interested laymen and scholars.

TERESA RAKOWSKA-HARMSTONE

Carleton University, Ottawa, Canada

**The Austrian Example.** By Kurt Waldheim. Translated by Ewald Osers. (New York: Macmillan, 1973. Pp. 230. \$6.95.)

Much the greater part of this book was published as *Der Österreichische Weg* in 1971 when its author, having failed to become federal president of Austria, was a prominent candidate for the succession to U Thant. It now appears in English with short chapters added at each end which reflect the successful

candidate's new status in a studied inoffensiveness, a style appropriate to the international character of his office as Secretary-General of the United Nations. The rest of the book is hardly less bland. Waldheim's record of careful and correct behavior, his diplomatic professionalism in fact, must have been one of the main reasons for his acceptability, and if his writing is singularly uncontroversial this is just the other side of the coin.

Successive chapters deal with Austrian history since 1918 and aspects of foreign policy under the Second Republic, the main lines of which Waldheim endorses with something like enthusiasm. On neutrality, Waldheim is at pains to emphasize the policy's status in Austrian parliamentary legislation, and its domestic origins in the thinking of Karl Renner and other Austrian statesmen. He is anxious, perhaps unnecessarily, lest it be thought that neutralization was simply imposed on a passive Austria by fiat of the Powers. Although he was actively involved in Austria's diplomatic efforts to secure full independence, Allied military withdrawal, and the international treaty of 1955, his account scrupulously restricts itself to secondary sources, the published memoirs of diplomats, politicians, and historians. He has very little to say—or, perhaps, he has chosen to say little here—on Austria's relations with the Powers, with Eastern European states or with Yugoslavia. Even the chapter on economic aid to less-developed countries is only in part an account of Austrian experience and largely a rehearsal of familiar facts and figures.

The most interesting chapters are those concerned with two of the outstanding problems which faced Austria when Waldheim was Foreign Minister (1968–70): the South Tyrol dispute and relations with the European Economic Community.

Waldheim's account of the South Tyrol dispute is the most closely detailed part of the book and sheds light particularly on the later phases of the "timetable" settlement negotiations, conducted in London, Paris and (finally) Copenhagen. He concludes, in what we may take to be the quintessence of his general approach to international problems: "On the South Tyrol issue, as on any other issue, it was ultimately necessary to think in terms of realistic politics and not to indulge in illusions that might have had disastrous consequences for the minority concerned."

He goes on immediately (p. 150) to reaffirm that Austria's "protective rôle" has not expired: on the contrary, the Gruber-De Gasperi Agreement of 5 September 1946 "is

still in force." What emerges from this chapter and from the one on European integration is an understandable resentment of erstwhile Italian efforts to link the erection of barriers against Austrian participation in the EEC to the South Tyrol dispute. Waldheim and his predecessors already had their share of difficulties arising from Soviet and (at times) French opposition to a closer relationship between Austria and the Community.

Unfortunately, the book suffers from different translation and production. The Foreign Minister of Sweden from 1945 to 1962 was Östen Undén, not Lunden (p. 104). The Pearson Report of 1969 had to do not with *Pattern* but with *Partners in Development* (pp. 211–12). As ill luck would have it, the brief chapter on Austria in the UN is the one most thickly studded with inaccuracies, to the embarrassment, no doubt, of the unwitting author who appears not to have checked the translation. There is, regrettably, no "Committee for the Protection of Minorities" (p. 160) in the UN system, although there is a *subcommission* which brackets the protection of minorities with the prevention of discrimination. The "second United Nations decade" (p. 164) turns out to be the Second Development Decade. The reference to "the space agency" (p. 159) suggests a specialized agency; but there isn't one, so from the context one concludes that a reference must be intended to Ambassador Peter Jankowitsch's chairmanship of the UN Committee on the Peaceful Uses of Outer Space. "The first international state conference" (p. 159), however, had me defeated; as did the typesetting on p. 160 where two successive paragraphs totter off the page into incomprehensibility.

This is all the sadder because the subject is of the utmost interest. Austria having long since achieved the reputation of a "good" UN member, it is disappointing that Waldheim, who headed the Austrian mission to the UN for so many years (1955–56, 1960–64, 1970–71), modestly refrains from telling us how this was done. Perhaps there is a clue to Austria's success early in the book (p. 8), where Waldheim writes: "Today Austria's diplomats represent a small state . . . Since [small states] are deprived of any real power, there is all the more reason for them to demonstrate their usefulness to other nations through an intelligent, well thought-out and often conciliatory foreign policy, thereby also serving their own interests."

NICHOLAS A. SIMS

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Political Science (University of London)*

**Documents on International Affairs, 1963.** Edited by D. C. Watt, James Mayall, and Cornelia Navari. (New York: Oxford University Press, 1973. Pp. xiii, 517. \$38.50.)

With this volume the Royal Institute of International Affairs (Chatham House) has issued its final collection of documents in a series which began with volumes on world affairs in the 1920s. The series contained two components: the *Survey of International Affairs*, which analyzed world events and *Documents on International Affairs*, which presented documentation on those events.

The final *Survey* for 1963 has yet to be published. This makes a complete evaluation of the book under review difficult, for it is designed in part as a documentary supplement, with the material arranged to correspond to the chapters in the *Survey*. Perhaps because of this two-part format there are no editorial introductions to the some 150 documents in this volume, a serious deficiency in light of the great range of issues dealt with.

Material is set forth topically in five broad categories: the Great Powers, the Middle East, the Far East, Africa, and the Western Hemi-

sphere. The book concludes with a useful chronological listing of all documents. Ranging in form from treaties and communiques to speeches and memoranda, the documents cover a multitude of topics. Among these are the Nuclear Test Ban Treaty, the Sino-Soviet rift, the war in Yemen, American involvement in Vietnam, and the creation of the Organization of African Unity.

The strength of such a book is also its major weakness. The presentation in one volume of documentation on such diverse events gives the scholar a useful overview of available source material. But limitations of space preclude documentation sufficient for in-depth research.

Chatham House has indicated that a major factor in the decision to end the series was the growing complexity of world affairs. It is true that an increasing number of nations in a multipolar world makes the task of synthesis more difficult. But it is sad to see the passing of such a long-standing effort to make the world, however complex, more comprehensible.

ROBERT C. GRAY

*Franklin and Marshall College*



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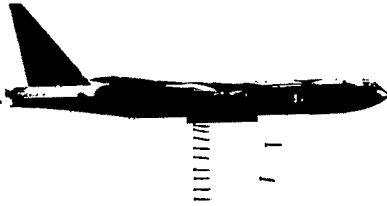
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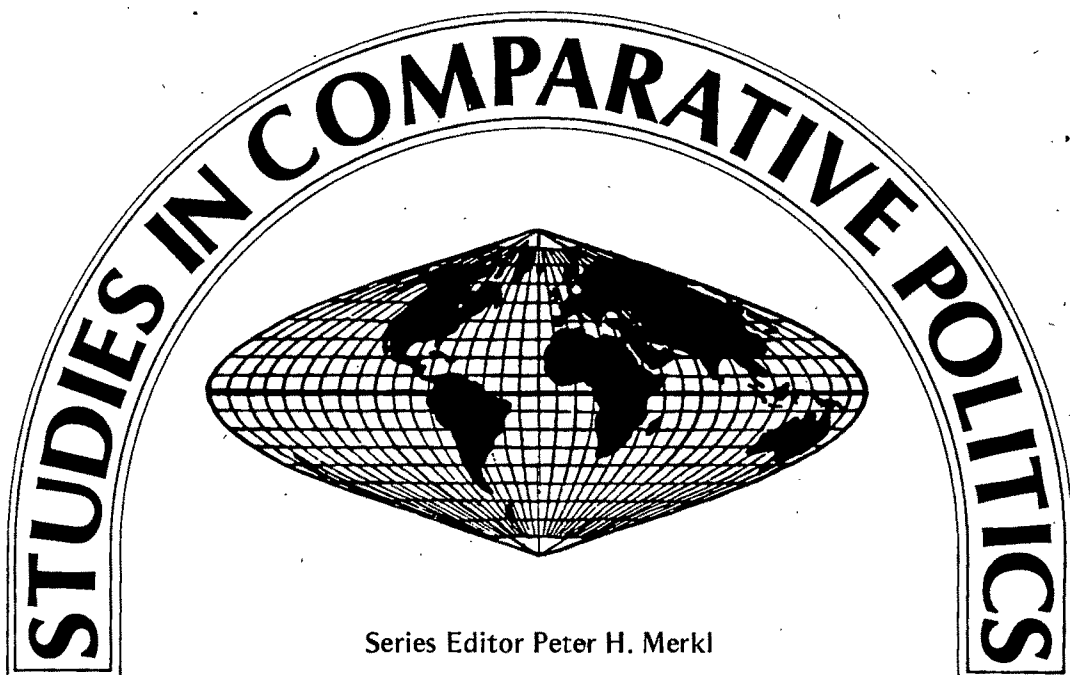
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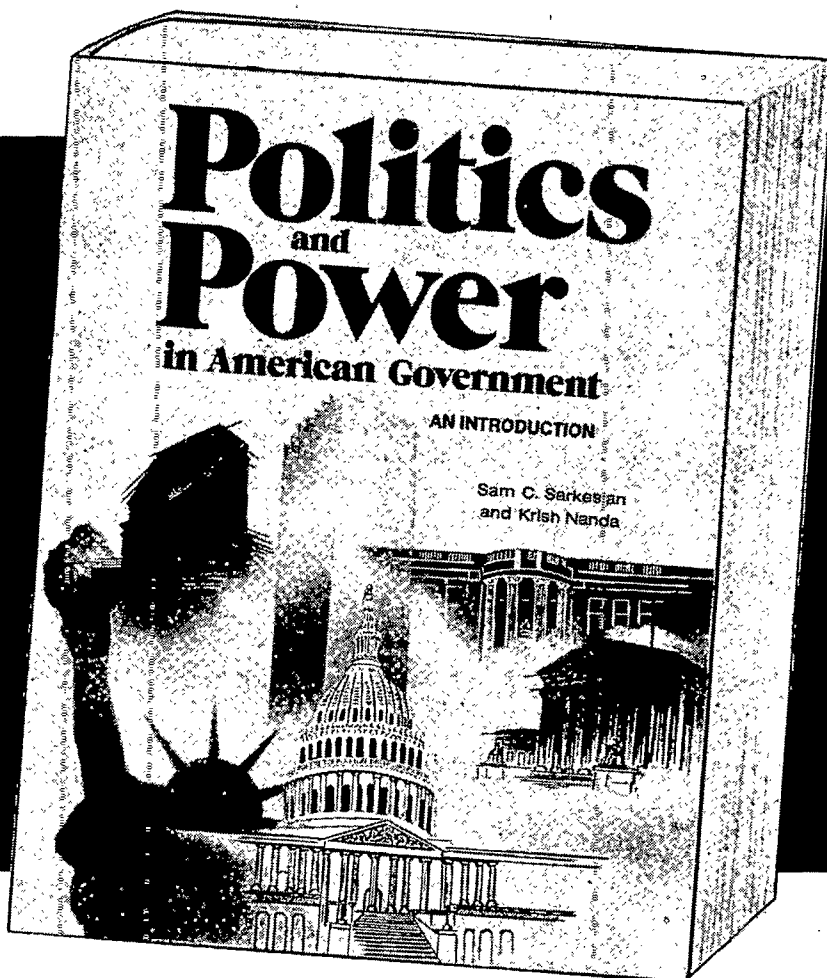
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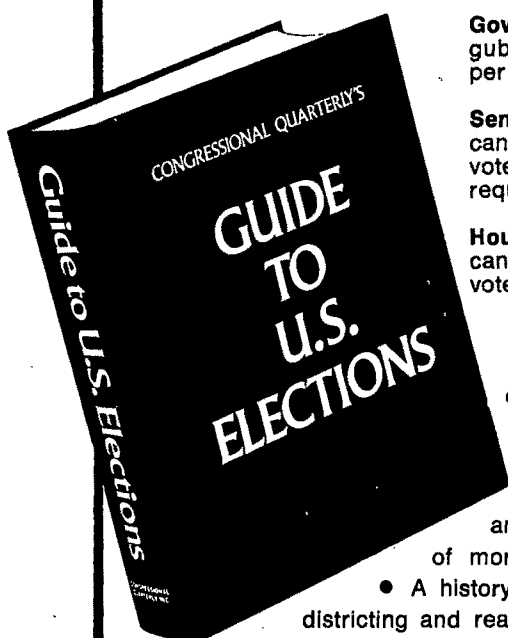
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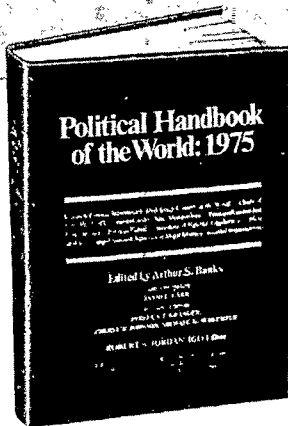
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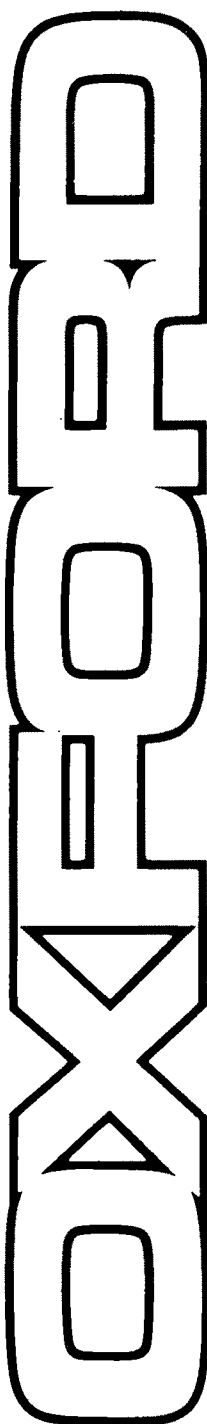
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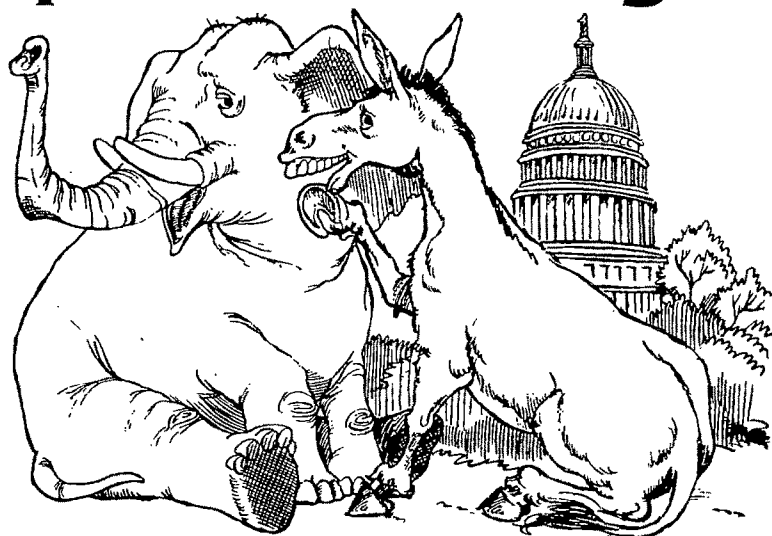
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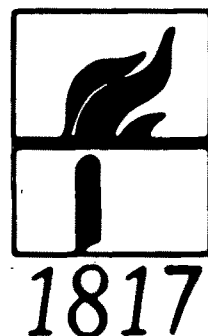
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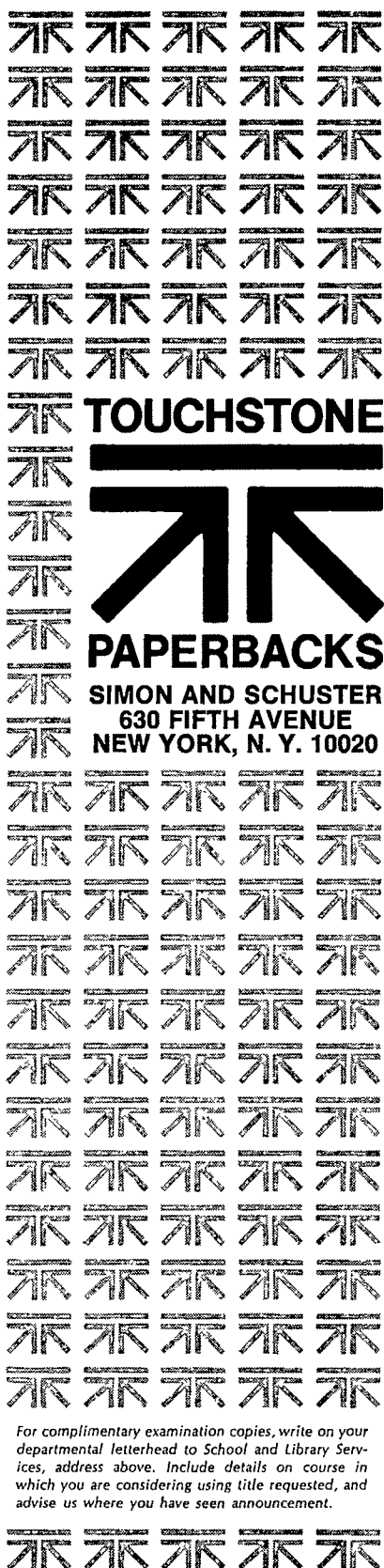
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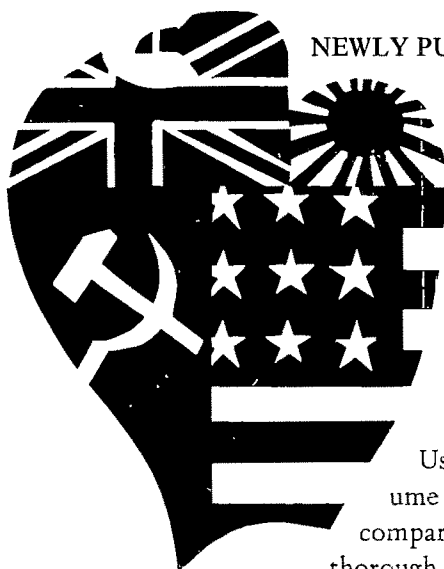
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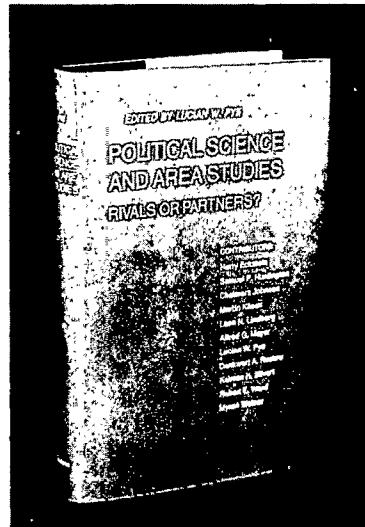
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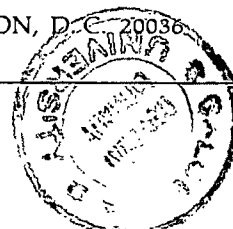
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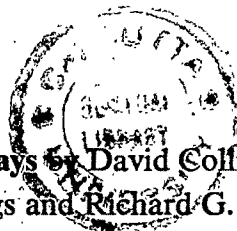
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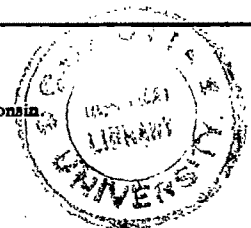
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*Office of publication:* Curtis Reed Plaza, Menasha, Wisconsin.

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## ARTICLES

- 181 Participation, Political Structure, and Concurrence.** Under what circumstances do citizens in a democracy influence their leaders? This paper uses an index of citizen-leader agreement on community problems to examine the effects of political factors on linkage. This index, termed concurrence, was based on parallel questions on community problems asked of citizens, government heads, and other local leaders sampled in sixty-four smaller American communities.

Concurrence was significantly higher in communities with high levels of citizen participation, contested elections, partisan ballots, and active political parties. Regression analysis showed that while voting rates had the largest direct impact on concurrence, participation had more impact when salient electoral alternatives were available. Partisan, contested elections also were associated with higher concurrence between leaders and persons of low socioeconomic status. Political factors also affected concurrence rates in both consensual and nonconsensual communities.

Alternative explanations for these findings (popular control of leaders, leaders' efforts to influence citizens or manipulate participation) are considered. Since concurrence scores of nonelected local leaders were also higher in participant communities with contested elections, it is suggested that political factors may affect citizen-leader agreement by facilitating communication between leaders and citizens, as well as by aiding electoral accountability.

By SUSAN BLACKALL HANSEN, Assistant Professor of Political Science, University of Illinois, Champaign-Urbana.

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- 1200 Courts and Conflict Resolution: Problems in the Mobilization of Adjudication.** This article attempts to assess the role of courts and other adjudicative institutions in the definition, interpretation, and management of conflict. Understanding the function of courts requires an understanding of a society's entire range of conflict management mechanisms. Particular emphasis is placed on those variables most likely to determine where and how conflicts will be solved.

Adjudicative institutions can be effectively differentiated by a typology which measures the level of formality in procedures and the degree of "publicness." The structure of a dispute-resolving institution will have an important effect on which disputes are presented to it and how they are decided. The nature of the dispute, goals of the disputants, social context, and political culture are also important variables.

Government has an important stake in the manner in which disputes arise and are resolved. It may promote or require the resolution of some disputes in the courts while allowing others to be resolved in less public and formal arenas. Formal litigation may provide a model for private dispute resolution. It may also absorb and deflect grievances before they escalate into more organized and intense demands on the political system. Finally, litigation may have an important effect on system stability by promoting support for regime values.

By AUSTIN SARAT, Assistant Professor of Political Science, Amherst College and JOEL B. GROSSMAN, Professor of Political Science, University of Wisconsin, Madison.

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- 1218 Mass Political Attitudes and the Survey Response.** Students of public opinion research have argued that voters show very little consistency and structure in their political attitudes. A model of the survey response is proposed which takes account of the vagueness in opinion survey questions and in response categories. When estimates are made of this vagueness or "measurement error" and the estimates applied to the principal previous study, nearly all the inconsistency is shown to be the result of the vagueness of the questions rather than of any failure by the respondents.

By CHRISTOPHER H. ACHEN, Assistant Professor of Political Science, University of California, Berkeley.

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- 1232 The Effect of Aggregate Economic Variables on Congressional Elections.** This paper uses rational voting behavior as an organizing device to develop a framework within which to consider the effect of economic aggregates on voters. Unlike most previous studies, ours permits the voter to vote for candidates of either party or to abstain. A principal finding is that the effect of the main economic aggregates on the participation rate is much clearer than the effects on either party. Our results deny that an incumbent administration can affect the control of Congress by stimulating the economy. Voters appear to make judgments about inflation, unemployment and economic growth. We investigated on the basis of long-term, not short-term performance.

By FRANCISCO ARCELUS, Assistant Professor of Economics and Commerce, Simon Fraser University, and ALLAN H. MELTZER, Maurice Falk Professor of Economics and Social Science, Carnegie-Mellon University.

**Comment.** By HOWARD S. BLOOM, Ph.D. Candidate in Political Economy and Government, Harvard University, and H. DOUGLAS PRICE, Professor of Government, Harvard University.

**Comment.** By SAUL GOODMAN, L.L.B. Candidate, University of Virginia Law School and GERALD H. KRAMER, Professor of Political Science, Yale University.

**Rejoinder.** By FRANCISCO ARCELUS and ALLAN H. MELTZER.

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- 1270 The Limits of Consensual Decision.** This essay criticizes the ideal of consensual decision as it appears in liberal political theory. A historical survey begins with Locke's view of consent, its criticism and extension by 19th century figures such as Godwin, Calhoun, and Mill, its reappearance in the guise of economic efficiency within the works of Wicksell or Buchanan and Tullock and as moral autonomy in Wolff's *Defense of Anarchy*. The paper offers a structural account of political decision making in which vulnerability to the authority of others seems inescapable and in which neither unanimity nor a universal right of consent is possible. On this telling, consensual decision is logically unattainable and misdirects constitutional theory.

By DOUGLAS W. RAE, Professor of Political Science, Yale University.

**Comment.** By GORDON TULLOCK, University Professor, Virginia Polytechnic Institute and State University

**Rejoinder.** By DOUGLAS W. RAE.

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- 1299 Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption.** Cross-national research has, with a few exceptions, dealt exclusively with hypotheses that focus on causal relations within nations. It is increasingly clear both on substantive and methodological grounds, however, that diffusor effects among nations must also be considered. The present research combines these alternative perspectives in an analysis of the timing of the first adoption of social security in nations. It is found that not only prerequisites explanations—which focus on causes within each nation—but also spatial and hierarchical diffusion effects must be considered in explaining patterns of social security adoption. The most important overall pattern, which appears to result from diffusion, is the tendency for later adopters to adopt at lower levels of modernization. This finding is interpreted as being due in part to a general tendency toward a larger role of the state in later developing countries—involving an important difference in the *sequence* in which different aspects of modernization occur—and in part to special characteristics of social security as a public policy.

By DAVID COLLIER, Associate Professor of Political Science, Indiana University, and RICHARD E. MESSICK, Research Assistant, Energy Policy Research Project, George Washington University.

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- 1316 Continuity and Change in Political Orientations: a Longitudinal Study of Two Generations.** This paper utilizes a national panel study of two biologically linked generations to study political change and continuity between 1965 and 1973. Four basic processes and combinations thereof are posited: absolute continuity, generational effects, life-cycle effects, and period effects. Data at the aggregate level give strong support for each type of change and continuity progression, depending upon the substantive political orientation examined. There are also strong traces of hybrid effects, especially the combination of period and life-cycle processes acting to propel the younger generation at a faster clip than the older. Over the eight-year span the absolute cleavage between the generations tended to decline, the major exception occurring with respect to specific issues and partisanship. The anomaly of this strain toward convergence in the light of the generation gap controversy is discussed.

By M. KENT JENNINGS, Professor of Political Science and Program Director, Center for Political Studies, University of Michigan, and RICHARD G. NIEMI, Professor of Political Science, University of Rochester.

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- 3236 Hobbes's Doctrine of Method.** A persistent problem in the interpretation of Hobbes's self-proclaimed founding of modern political science is the nature of the link between that political science and Hobbes's understanding of modern natural science and scientific method. The intention of this essay is to suggest that Hobbes's doctrine of method reveals the unity of his teaching about science, man, and politics. The unifying role of the doctrine of method can be understood only as a function of Hobbes's intention to reform what he saw as the previously defective relationship between practice and theory. In the light of this intention, the doctrine of method will be shown to consist in a new rhetoric which links the resolution of the human problem to the conquest of nature facilitated by the new science of nature. This rhetoric will be shown to be the substantial core of the doctrine of method itself.

By J. WEINBERGER, Assistant Professor of Political Science, Michigan State University.

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- 1354 Nonincremental Policy Making: Notes Toward an Alternative Paradigm.** Much of the literature of policy analysis and public administration is dominated by incremental and "divisible goods" paradigms. Policy is assumed to be a process of marginal and adjustive decision making in which benefits are dispensed piecemeal—proportionate to prevailing distributions of power or publicized need. This essay asserts the existence of a class of nonincremental, indivisible policy pursuits for which the analytical weaponry of political science is largely inappropriate. Such policies display a distinctive set of political and administrative characteristics. These characteristics are explained and examined in connection with manned space exploration policy. An assessment is offered of the challenges posed by nonincremental policy to contemporary outlooks in political science.

By PAUL R. SCHULMAN, Assistant Professor of Political Science, University of Tennessee.

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- 371 **The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies.** This analysis of open-ended interviews conducted in 1969–70 with small samples of English, French, and white and black American children focuses on orientations toward the heads of state of the three nations and the Prime Ministers of Britain and France. The English children exhibit remarkably positive views of the Queen. Many of them believe her to be the nation's effective leader rather than a figurehead. Any political animus they express is directed toward the Prime Minister. The French children tend to describe the President of the Republic positively when they express feelings toward him at all but expect him to behave harshly and arrogantly in actual situations. Their descriptions of political leaders exude authoritarian imagery and perceptions; they perceive the President of the Republic in an impersonal, undifferentiated manner, and are only barely aware of the Premier. The general descriptions of the President of the United States by white American children interviewed in 1969–70 are remarkably similar to the benevolent-leader perceptions of children in the Eisenhower-Kennedy years, but the 1969–70 children exhibit much less idealized views of a president depicted as a law-breaker. A post-Watergate white American comparison group interviewed in June 1973 is generally aware of, but puzzled by, the Watergate events. At this early stage in the Watergate revelations, white children were only slightly less likely than the 1969–70 respondents to idealize the President, but were substantially more likely to perceive the president depicted as a law-breaker in terms implying that the President is "above the law." The American black comparison group, which is too small and special in its geographical circumstances to offer more than suggestive findings, is the most negative of the four groups in general responses to the head of state, but is more like the white American group than like the English or French children in expectations about the actual behavior of the leader. Even though the black and white American children seem to be similar in their expectations about how certain political encounters would ensue, interpretations of encounters are strikingly different.

By FRED I. GREENSTEIN, Henry Luce Professor of Politics, Law, and Society, Princeton University.

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By NANCY I. LIEBER, Lecturer in Political Science, University of California, Davis.

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### INSTRUCTIONS TO CONTRIBUTORS

Address correspondence about contributions to the *Review* concerning issues before the issue of September 1977 to Nelson W. Polsby, Department of Political Science, University of California, Berkeley, Berkeley, California, 94720, and issues after September 1977 (including all new manuscripts) starting January 1, 1976, to Charles O. Jones, Department of Political Science, Mervis Hall, University of Pittsburgh, Pittsburgh, Pennsylvania 15260. Each manuscript should be accompanied by an abstract of up to 150 words briefly describing the article's contents. All manuscripts and abstracts should be submitted IN DUPLICATE. They should be double-spaced and may be in typed, mimeographed, hectographed, or other legible form. Footnotes should appear at the end of the manuscript, not at the bottom of the page, and should also be double-spaced. Manuscripts that do not follow this format will be returned to the authors for retyping.

Since manuscripts are sent out anonymously for editorial evaluation, the author's name and affiliations should appear only on a separate covering page. All footnotes identifying the author should also appear on a separate page.

Address books intended for review to Philip Siegelman, *American Political Science Review*, 210 Barrows Hall, University of California, Berkeley, Berkeley, California 94720. Information, including News and Notes, for the Association's newsjournal, *PS*, should be sent to 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036; advertising, reprint and circulation correspondence should be sent to the Executive Director at the Washington office. Domestic claims for non-receipt of issues must be made within six months of the month of publication; overseas claims, one year. Advertising information and rates are available from Nancy Edgerton, Adv. Manager, APSA, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.

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## EDITORIAL COMMENT

**Changing the Guard.** Starting January 1, 1976, all new manuscripts should go to the managing editor elect, Professor Charles O. Jones, Department of Political Science, Mervis Hall, University of Pittsburgh, Pittsburgh, Pennsylvania 15260.

Current management will receive complaints and correspondence about all issues up to and including the issue of September, 1977. After January 1, Professor Jones will be making all decisions about the contents of forthcoming issues of the *APSR*.

**Charles O. Jones.** The process prescribed by the Association's Constitution and by-laws is now complete, and the choice of the Council for Managing Editor of the Review 1977-1980 has fallen upon Charles O. Jones, Maurice Falk Professor of Political Science at the University of Pittsburgh. Professor Jones was born in 1931 in Worthing, South Dakota, and is a Phi Beta Kappa graduate of the University of South Dakota, where he learned political science from an extraordinary teacher, William O. Farber. Professor Farber sent Jones on to graduate work at the University of Wisconsin, where he earned master's and doctoral degrees. His work at Madison was punctuated by a year's study at the London School of Economics and Political Science. At Wisconsin, Jones studied

with Leon Epstein and Henry Hart, among others, and he wrote a distinguished doctoral dissertation under the supervision of Ralph K. Huitt, describing the patterns of interest group access to the Agriculture Committee of the U.S. House of Representatives, and showing how these patterns of access influenced the organizational structure of the committee.

Professor Jones's teaching career has provided him with an unusually wide range of experiences, including service at Wellesley, a small liberal arts college in New England, at the University of Arizona in Tucson, a middle-sized Western public university, and at Wisconsin, one of the great institutions of the Middle West. The University of Pittsburgh, his present academic home, is a semi-public institution which draws its character from its unmistakably urban milieu. The Department of Political Science is one of the larger and better known departments of the University, and houses a number of scholars having national reputations in several subfields of the discipline.

The scholarly work for which Professor Jones is best known falls approximately into three clusters of articles and books. One cluster concerns the organization and functioning of Congress, with special attention to the constraints upon and opportunities for policy leadership that

exist for party leaders and committee leaders. A second cluster is concerned with the prospects for the vitality of the American two-party system in light of the evolving character of the Republican party as a semi-permanent minority party. A third cluster explores diverse public policies and political reforms—including the four year congressional term, the revision of the Hatch act, and alternative solutions to the problem of air pollution. This last work, which has most recently engaged his interest, has carried Jones into an examination of the functioning of American federalism, and into the appraisal of economic and technological trade-offs in the formation and execution of public policy.

Professor Jones's organizational skills were developed in the course of a sojourn as Associate Director of the National Center for Education in Politics, and the American Political Science Association has at various times called upon his services as member of the Council, Treasurer, Chairman of the Association's Trust Fund, and twice as member of the Program Committee for the Annual Meeting.

The foregoing description can properly be characterized as a portrait of an indecently full, distinguished, and well-rounded curriculum vitae, but, of course, it fails utterly to capture the human qualities that make Charles O. Jones such a felicitous choice as managing editor. A number of these qualities have impressed Jones's colleagues as they have come across him in a variety of settings. That he is energetic and efficient in the use of his time can of course be ascertained by a glance at the enormous variety of things he has accomplished in his career so far. Nobody who participates in the selection process can fail to attend to the presence or absence of the gift of sheer working capacity in a prospective *APSR* managing editor.

Those who know Charles O. Jones best, however, pass over this gift lightly in order to call attention to other, subtler qualities. He is a man of the most exquisite sensitivity about other human beings, and the predicaments, large and small, of human existence on this planet. This sensitivity leads him to restraint and moderation in the expectations he entertains about others, to a gentle pessimism about large and pretentious enterprises, and to a thoroughgoing toleration of all sorts and conditions of people and their works.

To be even superficially acquainted with Charles Jones is to realize he has an ample fund of common sense. To know him a little better is to see that behind this fund there stands a veritable Fort Knox of human insight that can only have arisen from a capacity to observe his fellow creatures with a steady and sympathetic eye, and to think long and hard about what he sees.

An active manifestation of this quality is his scrupulous sense of responsibility—toward family, friends, colleagues and profession. The fair-mindedness and balance that inform his intellectual judgments arise not out of indifference but out of careful attentiveness to the essential task of playing by rules of the game that give a just weight to the needs and expectations of others.

Best of all, however, Charles O. Jones is that precious rarity, a joyful man, someone to sit next to while a dull meeting drones on, someone who can perceive the music as well as the words in the messages we send. He brings the solace of laughter with him, reducing tension, lightening the load.

With the future of our common enterprise in the hands of Charles O. Jones, we could rest easy if we wanted to. But of course under his leadership it will be far more fun to join in than to hang back.

**Another Tribute.** We are always on the prowl for unobtrusive measures of the impact of the *APSR*. One of the best such measures is the appearance of the *APSR* in a starring role in foolish statements by political scientists, especially in carelessly drawn indictments of the discipline at large. Here is one recent example (readers are invited to send in others):

In one of the recent issues of the *American Political Science Review*, 162 pages were devoted to articles; about two-thirds of this space dealt with voting, elections, and Congressional representation, even though the profession as a whole is well aware that there are many more significant phenomena.\*

Typically, statements such as these are embedded in arguments deploring the marshalling of quantitative evidence in the course of political inquiry. And no wonder, since the talisman of the *APSR* can be so readily invoked to ward off charges of sampling error. We can hardly imagine a higher or more ingenuous tribute;

#### Articles Accepted For Future Publication

Joel D. Aberbach, University of Michigan and Bert A. Rockman, University of Pittsburgh, "Clashing Beliefs Within the Executive Branch: The Nixon Administration Bureaucracy"

Paul R. Abramson, Michigan State University, "Generational Change and the Decline of Party Identification"

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

John A. Armstrong, University of Wisconsin, "Mobilized and Proletarian Diasporas"

John M. Bacheller, Kirkland College, "Lobbyists"

\* Citation cheerfully furnished on request.

- and the Legislative Process: The Impact of Environmental Constraints"
- Richard M. Bank, University of California, Santa Barbara and Steven R. McCarl, University of Denver, "Virtue, Obligation and Politics: Revisited"
- Robert A. Bernstein, Texas A and M University, "Divisive Primaries Do Hurt: U. S. Senate Races, 1956-1972"
- Steven R. Brown, Kent State University, "Toward An Experimental Analysis of Political Literature"
- Charles D. Cary, University of Iowa, "A Technique of Computer Content Analysis of Transliterated Russian Language Textual Materials: A Research Note"
- Jonathan D. Casper, Stanford University, "The Supreme Court and National Policy Making"
- Roger Cobb, Brown University, Jennie-Keith Ross, Swarthmore College, and Marc Howard Ross, Bryn Mawr College, "Agenda Building as a Comparative Political Process"
- Clarke E. Cochran, Texas Tech University, "Authority and Community: The Contributions of Carl Friedrich, Yves Simon, and Michael Polanyi"
- Richard K. Dagger, University of Minnesota, "What is Political Obligation?"
- Fred R. Dallmayr, Purdue University, "Beyond Dogma and Despair: Toward a Critical Theory of Politics"
- John P. Diggins, University of California, Irvine, "Four Theories in Search of a Reality: James Burnham, Soviet Communism, and the Cold War"
- Shaheen F. Dil, Princeton University, "The Cabal in Kabul: Great Power Interaction in Afghanistan"
- Lowell Dittmer, State University of New York, Buffalo, "Thought Reform and the Cultural Revolution: An Analysis of the Symbolism of Chinese Polemics"
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- Robert Eyestone, University of Minnesota, "Confusion, Diffusion and Innovation"
- Norman I. Fainstein, Columbia University and Susan S. Fainstein, Rutgers University, "The Future of Community Control"
- John A. Ferejohn, California Institute of Technology, "On the Decline of Competition in Congressional Elections"
- Robert C. Fried, University of California, Los Angeles, "Party and Policy in West German Cities"
- John V. Gillespie, Dina A. Zinnes, Philip A. Schrod, G. S. Tahim and Richard Rubison, Indiana University, "An Optimal Control Model of Arms Races"
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- David K. Hildebrand, University of Pennsylvania, James D. Laing and Howard Rosenthal, Carnegie-Mellon University, "Prediction Analysis in Political Research"
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## Participation, Political Structure, and Concurrence\*

SUSAN BLACKALL HANSEN

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<sup>1</sup> The authors of *The Civic Culture* have stated, "Unless there is control of governmental elites by non-elites, it is hard to consider a political system democratic."<sup>1</sup> In theory, citizen participation should play an important role in democratic control over political leaders. Yet while many studies have considered factors that affect participation rates (individual attitudes, social class, political setting)<sup>2</sup> few have examined the impact of participation on the political system: certainly serious theoretical and methodological problems would have to be solved before linkage could be established.

It is the premise of this paper that the setting within which citizen participation takes place must be examined before we can establish connections between citizens' preferences and leaders' actions. I will compare citizen-leader agreement on the salience of local problems in a sample of American communities with various political structures and levels of participation, in order to suggest conditions under which participation is most likely to influence leaders' attitudes and behavior.

Citizen participation can influence the behavior of political leaders in two ways. First, the threat of being evicted from office (or the lure of attaining office) may induce political leaders to anticipate and to respond to popular demands. Voting is the form of participation most likely to exert such influence, but other activities by citizens may also provide evidence of support or opposition to leaders' policies.<sup>3</sup>

\* The data used in this analysis were made available by Sidney Verba and Norman H. Nie, directors of the Cross-National Project on Political Participation and Social Change. Facilities, assistance, and computer time were provided by the National Opinion Research Center and the computing centers of the Universities of Illinois, Washington State, and British Columbia. My thanks to Norman H. Nie, Taketsugu Tsurutani, and Wilfred J. Hansen for their comments and suggestions.

<sup>1</sup> Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown & Co., 1965), p. 341.

<sup>2</sup> See Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), and William H. Flanigan, *Political Behavior of the American Electorate* (Boston: Allyn & Bacon, 1968), for a summary of this literature.

<sup>3</sup> These include demonstrations, rent strikes, nonvoting, and threats of violence; and may be used by groups excluded from the electoral system. See Jerome Skolnick, *The Politics of Protest* (Washington, D.C.: U.S. Government Printing Office, 1969), and William F. Mullen, "Community Control and Black Political Participation," in *Participatory Democracy*, ed. Terrence Cook and Patrick Morgan (San Francisco: Canfield Press, 1971), pp. 256-265.

Whether voting and elections actually operate to control leaders' behavior, however, is a difficult question: the answer depends on the voters' level of information and interest,<sup>4</sup> the electoral situation (whether opposition is present or whether alternatives exist),<sup>5</sup> and the political ambitions of public officials seeking election.<sup>6</sup> Even if conditions for strict electoral accountability are not met, however, participation may affect leaders' behavior in another way: by communicating information about citizen preferences.<sup>7</sup> Without this information, even leaders who wish to act responsively may be unable to do so; the term "mandate uncertainty" has been suggested in this regard.<sup>8</sup> Under certain conditions (such as referenda) voting may provide specific information regarding citizen preferences. But usually, elections are too blunt an instrument to convey anything but the most general information; forms of participation which provide for more direct citizen contact with leaders may offer more detailed information about issues and alternatives.

In *Participation in America*, Verba and Nie found that citizen participation was associated with greater citizen-leader agreement on community problems. This was the case especially for

<sup>4</sup> An early critique of the "rational-activist citizen" was provided by Walter Lippman in *The Phantom Public* (New York: Harcourt, Brace, 1925). Flanigan, *Political Behavior of the American Electorate*, has reviewed survey and other data about the electorate's political intelligence. See also V. O. Key, *The Responsible Electorate* (Cambridge, Mass.: Harvard University Press, 1966).

<sup>5</sup> As Kenneth Prewitt and Norman H. Nie noted, "Rather than probe and probe into the rationality of the individual voter, it may be more fruitful to inquire about the choices available under present conditions." "Election Studies of the Survey Research Center," *British Journal of Political Science*, 1 (October, 1971), 493. See also John L. Sullivan and Robert E. O'Connor, "Electoral Choice and Popular Control of Public Policy," *American Political Science Review*, 66 (December, 1972), 1256-1268.

<sup>6</sup> Kenneth Prewitt, "Political Ambition, Volunteerism, and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5-13.

<sup>7</sup> Different channels for citizen influence are discussed by Stein Rokkan in "Mass Suffrage, Secret Voting, and Participation," in *Political Sociology*, ed. Lewis A. Coser (New York: Harper Torchbooks, 1966), pp. 101-131.

<sup>8</sup> This term was first used by Kenneth Janda, "Democratic Theory and Legislative Behavior: A Study of Legislator-Constituency Relationships" (Ph.D. thesis, Indiana University, 1961), pp. 169-179. Cited in Heinz Eulau, *Micro-Macro Political Analysis: Accents of Inquiry* (Chicago: Aldine, 1969), p. 99.

voting, but also held for the other modes of participation they described: campaign activity, citizen-initiated contacts with public officials, and cooperative participation in organized groups.<sup>9</sup> Several problems must be considered, however, before one attempts to interpret this as evidence of leaders' responsiveness:

(1) Verba and Nie found that voting rates showed the strongest association with their "concurrency index" of citizen-leader agreement on community problems.<sup>10</sup> This is somewhat paradoxical, since concurrence depended on responses to open-ended questions about a very wide range of problems: just the sort of specific information about citizen priorities that is unlikely to emerge from most elections. Other forms of participation which should provide more information to leaders (such as citizen contacts with officials) had an impact on concurrence only within communities with high voting rates.<sup>11</sup> This suggests that we should look more closely at kinds of electoral alternatives available to voters in these communities.

(2) One might predict that popularly elected heads of local government would respond to electoral participation. But the leadership sample used by Verba and Nie included school, business, political-party, and communications leaders: the various modes of participation might affect these people differently or to a lesser extent.

(3) The correlations noted between participation rates and citizen-leader agreement, while statistically significant, were only moderate. There was a considerable range of concurrence scores across the communities described in *Participation in America*, and only a small part of the variance in concurrence was due to participation.<sup>12</sup> Other factors must be found to account for the variance in concurrence not explained by participation.

(4) Verba and Nie found marked differences in the social-class background and in the policy concerns of different types of participators.<sup>13</sup> Citizen-leader agreement may depend on whether com-

munity political processes aggregate conflicting interests, exacerbate conflicts, or encourage participation by particular social groups.

(5) No causal pattern has been established. A positive association between participation and concurrence could indicate that citizen activity was inducing leaders to respond. But it could also mean that participation makes citizens more open to persuasion by political leaders.<sup>14</sup> Example abound: political machines have been built and maintained because they could "deliver the vote" and use this resource for their own ends.<sup>15</sup> The War on Poverty has also been accused of manipulating participation for political reasons.<sup>16</sup> Mobilization of the masses under the guidance of a leadership cadre is one purpose for which political parties have been created in many countries.<sup>17</sup>

In order to sort out the causal patterns at work in these communities, additional data should be considered on local leaders and on the political and social setting within which leaders and citizens interact. The more we can learn about the circumstances under which citizen participation is associated with citizen-leader agreement, the more plausible will be the causal inferences that can be drawn.<sup>18</sup> Community *political structure* should be examined in order to specify those conditions under which participation may have an impact on citizen-leader agreement. By "political structure" I mean both legal-institutional factors (partisanship, type of government) and noninstitutionalized aspects (party and organizational activity,

<sup>14</sup> See Murray Edelman, *The Symbolic Uses of Politics* (University of Illinois Press, 1970), pp. 188-194, and Sidney Verba, *Small Groups and Political Behavior* (Princeton, N.J.: Princeton University Press, 1971), pp. 219-225.

<sup>15</sup> Numerous examples of such activity are offered in Mike Royko, *Boss* (New York: New American Library, 1971), and H. F. Gosnell, *Machine Politics: Chicago Model* (Chicago: University of Chicago Press, 1937).

<sup>16</sup> As Dolbeare has suggested of the poverty program, "maximum feasible participation" of the poor may have been used as a device to give deprived groups a stake in the established system. Kenneth M. Dolbeare, "Public Policy Analysis and the Coming Struggle for the Soul of the Post-Behavioral Revolution," in *Power and Community*, ed. Philip Green and Sanford Levinson (New York: Vintage, 1970), p. 106.

<sup>17</sup> See Joseph LaPalombara and Myron Weiner, *Political Parties and Political Development* (Princeton: Princeton University Press, 1966).

<sup>18</sup> Verba and Nie considered some of these possibilities: they found that such aspects of the community setting as its wealth and its insulation from the larger society did not affect the association between participation and concurrence on community problems. *Participation in America*, pp. 329-330. See also Susan B. Hansen, "Concurrence in American Communities: The Response of Local Leaders to the Community Political Agenda" (Ph.D. dissertation, Stanford University, 1972), chap. 9, for a fuller discussion of the relationship between concurrence and community setting.

<sup>9</sup> Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality* (New York: Harper and Row, 1972), pp. 44-81. These modes, based on a factor analysis of fifteen participatory acts, are also described in Sidney Verba, Norman H. Nie, and Jae-On Kim, *The Modes of Democratic Participation* (Beverly Hills, Calif.: Sage Publications, 1971), pp. 15-19.

<sup>10</sup> Verba and Nie, pp. 299-333.

<sup>11</sup> This index is described in Verba and Nie, Chapter 17 and Appendix I. A parallel measure will be used in this paper: see below and Appendix.

<sup>12</sup> The exact proportion of the variance in concurrence explained by participation depended on the mode of participation and on community consensus. See Verba and Nie, pp. 326-332.

<sup>13</sup> Citizen consensus and the "responsiveness to whom?" question are discussed in *Participation in America*, chaps. 17-19.

electoral competition). Each may affect the choices available to citizens, the ways in which citizens can communicate their preferences to community leaders, and leaders' potential for structuring citizen preferences.

#### Political Structure and Concurrence: Some Hypotheses

Previous research has suggested that political structure can affect city policies and the orientations of local governmental officials. Lineberry and Fowler found that "reformed" and "unreformed" governments differed in taxing and spending policies.<sup>19</sup> Hawley showed that certain groups of citizens benefit from partisan as opposed to nonpartisan local elections.<sup>20</sup> Yet even communities with the same formal structures may have different levels of party activity, or varying degrees of electoral competition. As Eulau and Prewitt noted in their study of California city councils (all nonpartisan, five-member groups), the effectiveness of local elections influenced the representational styles adopted by the councils.<sup>21</sup>

A community's political environment may affect citizen-leader agreement in several ways:

(1) Concurrence should be higher where citizens have a *choice* among contending leaders or policies. Elections may more effectively influence officeholders if they are frequently and vigorously contested, if the races tend to be close, or if incumbents are at least occasionally turned out of their positions.<sup>22</sup> Conversely, where one party or faction dominates local politics, elected officials have less to fear from adverse public opinion when they follow their own inclinations or the counsel of groups which support them.

(2) Concurrence should be greater the more *visible* the alternatives available to citizens. The presence of party labels on local ballots can help voters identify candidates with their records in office or their policy stands. Direct election of the local governmental executive should increase his responsibility and his salience to citizens.<sup>23</sup>

<sup>19</sup> Robert L. Lineberry and Edmund P. Fowler, "Reformism and Public Policies in American Cities," *American Political Science Review*, 61 (September, 1967), 701-716.

<sup>20</sup> Willis D. Hawley, *Nonpartisan Elections and the Case for Party Politics* (New York: John Wiley & Sons, 1974).

<sup>21</sup> Heinz Eulau and Kenneth Prewitt, "Political Matrix and Political Representation," *American Political Science Review*, 63 (June, 1969), 427-441. See by the same authors *The Labyrinth of Democracy* (Indianapolis: Bobbs-Merrill, 1973).

<sup>22</sup> These dimensions of electoral competition are those used by Eulau and Prewitt in *The Labyrinth of Democracy*, pp. 229-235. The importance of opposition is also stressed in *Political Opposition in Western Democracies*, ed. Robert A. Dahl (New Haven: Yale University Press, 1966).

<sup>23</sup> See Donald B. Rosenthal and Robert L. Crain,

(3) Competitive elections, partisanship, and direct election of heads of government may affect concurrence by making it easier for citizens to hold their leaders accountable. But the political environment can also aid the *communication* of citizen concerns to local leaders. Active political parties and community organizations can aggregate individual opinions into coherent policy proposals, and can articulate these to local political elites. Contested local elections may generate citizen interest and encourage community-wide discussion of local issues.

(4) The political structure of a community may affect concurrence by *impeding or facilitating participation*. Some forms of participation (writing letters to public officials, for example) take place entirely at the initiative of the citizen, at the time and place he chooses, and on whatever topic he picks. Yet according to Verba and Nie, voting and campaign activity have the greatest impact upon concurrence; and electoral participation does not depend entirely upon citizen initiative. Highly restrictive voter registration laws may limit the electorate to a great extent.<sup>24</sup> By juggling the voting hours, form of ballot, and location of polling places, local officials can further limit people's opportunity to vote.<sup>25</sup>

5. The *impact* of participation may also be influenced by political factors. People should be more inclined to vote if something is at stake, or if some contest is involved. Even if registration and voting conditions are not restrictive, people have little incentive to participate if the outcome is already established, or if no alternatives are salient. If no real choice is available, participation may play only a symbolic or supportive role, rather than influencing leaders or policy.<sup>26</sup>

"Structure and Values in Local Political Systems: The Case of Fluoridation Decisions," in *City Politics and Public Policy*, ed. James Q. Wilson (New York: John Wiley & Sons, 1968), pp. 217-242, for a discussion of the importance of the mayor's office and of his role as a focus of local decision making.

<sup>24</sup> The importance of registration has been emphasized by Stanley Kelley, Richard E. Ayers, and William G. Bowen in "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June, 1967), 359-379. See also Jae-On Kim, John R. Petrocik, and Stephen N. Enokson, "Voter Turnout among the American States: Systemic and Individual Components," *American Political Science Review*, 69 (March, 1975), 107-124.

<sup>25</sup> An example of this is offered by Arthur J. Vidich and Joseph Bensman, *Small Town in Mass Society* (Princeton: Princeton University Press, 1968), p. 20. The local political leaders deliberately timed elections so as to discourage voting by people who worked in nearby cities and who had interests quite different from those of the local farmers and businessmen who dominated politics in Springdale.

<sup>26</sup> On "pseudo-participation," see Verba, *Small Groups and Political Behavior*, pp. 217-225. Several researchers have suggested that citizen participation does have a

In terms of effects on concurrence, then, the communication functions of political structure may be as important as the control functions. Various communications theories of the political process have stressed the importance of information flows, feedback, and the structures that channel these.<sup>27</sup> A highly developed local political life could play a significant role in reducing "mandate uncertainty" and in giving local leaders a better idea what programs have popular support.

This paper will analyze both the communications and control aspects of political structure and participation, and will assess their relative impact on concurrence. I will use the data Verba and Nie reported in *Participation in America*, plus additional information on local leaders and community political structure. The next section will describe the data, concurrence index, and level of analysis used in this study. Then the relationship between participation and concurrence will be compared for two types of community leaders.

The effect of political factors such as partisanship, party activity, type of government, and electoral competition on concurrence and participation, will be examined, and several models of the concurrence process will be evaluated. The differential impact of participation and political structure on leaders' agreement with social-class and participatory subgroups within each community, and the implications of these results for theories about democracy and participation, will be the topic of the concluding section.

#### Data, Methods, and Level of Analysis

Under some circumstances, on some issues, the sanctioning effect of citizens' votes or opinions on an individual officeholder or on the party in power may be demonstrated. But in a complex modern society, with representation based on arbitrary geographic units, with collective groups of representatives at several levels of government considering a large number of issues at any given time, it is a difficult task to establish linkage between citizens and their leaders. The choice of an appropriate indicator of linkage poses major theoretical and methodological problems for the researcher.<sup>28</sup>

rational basis, and that a decision to vote is predicated on the perceived probability of one's preferred candidate winning, the size of the difference between alternatives, and the costs involved. See Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), and William E. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25-42.

<sup>27</sup> See Karl W. Deutsch, *The Nerves of Government* (Glencoe, Illinois: The Free Press, 1963).

<sup>28</sup> Different models and indicators of linkage have been reviewed by V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1961),

Ideally, direct comparisons of citizens and leaders should be made. But attention should also be paid to the interaction between these two groups, and to the context within which their interaction occurs. Such analysis was made possible using a concurrence measure based on parallel surveys of citizens' and leaders' views of community problems. As part of the Verba-Nie data gathered for *Participation in America*, interviews were conducted in 1967 with a sample of 1438 citizen respondents in sixty-four randomly selected communities less than 60,000 in population.<sup>29</sup> In each of these communities, several local leaders were chosen on the basis of their purposive roles (heads of government, Democratic and Republican party leaders, school officials, newspaper editors, and businessmen). Between three and seven interviews were conducted per community, the exact figures depending on the number of roles that were occupied in a given community. The total elite sample size was 306 persons.<sup>30</sup>

No attempt was made to identify those local leaders who were in fact most influential in community decision making. For a large survey of this type, this would have been difficult. But by choosing respondents with backgrounds in a variety of social sectors, it was expected that many persons with a high potential for influence in local affairs would be included. Several roles were selected, as many researchers have found that governmental officials share responsibility for public policy with other community leaders.<sup>31</sup>

pp. 411-431; Gerald M. Pomper, *Elections in America* (New York: Dodd, Mead, 1970); Norman R. Luttbeg, *Public Opinion and Public Policy: Models of Political Linkage* (Homewood, Illinois: The Dorsey Press, 1968), pp. 1-9.

<sup>29</sup> These sixty-four communities include half of those under 60,000 that fell into a standard multistage area probability sample used by the National Opinion Research Center for its amalgam surveys. Within these communities, quota sampling was used (quotas based on census tract statistics on age, sex, race, and employment) to select interviews from the noninstitutionalized population over age twenty-one. Between twenty-five and thirty interviews were conducted in each community. See Verba and Nie, *Participation in America*, Appendix A, for a full description of sampling methods.

Because of the quota sampling used, sampling errors cannot be computed directly. Substantial correlations, however, were noted between sample and population data (the latter based on the census and on election statistics) with respect to voting turnout, age, race, income, education, percentage in white-collar occupations, and ethnic background. For this reason the citizen sample can be considered a fairly reliable estimate of the population.

<sup>30</sup> The elite sample is discussed in Verba and Nie, Appendix H, and in Hansen, dissertation, chapter 2. The N of 306 includes 56 heads of government, 77 school officials, 61 business leaders, 56 party leaders, and 56 newspaper editors.

<sup>31</sup> See Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press,

In each community, citizen respondents were asked,

"What is the most important problem facing this community?"

The community leaders were asked the same question. They were also asked,

"What does the average person around here think are their problems and needs?"

"Are there some community problems that you have been particularly active in trying to solve—which are the problems to which you have given the most time and effort?"

For each problem which an elite respondent mentioned, he or she was given a concurrence score based on the percentage of community citizens who mentioned that particular problem. Thus the more problems a leader mentioned that were also mentioned by one or more persons in the community, the higher the score.<sup>32</sup> These three sets of problems (those which leaders either mentioned, perceived as citizen problems, or worked on) have been considered separately elsewhere. Since only minor differences in emphasis were noted they have been averaged together for the purpose of this paper.<sup>33</sup>

This concurrence index considers only one facet of the complex process of linkage: whether citizens and leaders agree on the issues facing their communities. Other analysts have considered whether citizens and leaders agreed on specific policy alternatives for issues. But it has been suggested that biases in the types of issues which reach the public agenda may be more important and extensive than differences of opinion on the way specific issues should be handled.<sup>34</sup> For this reason, I have been concerned with relative *priorities*. The interviewers did not probe for policy dimensions in the open-ended questions on community problems. A detailed community-by-community check on the open-ended responses, however, indicated that citizens and leaders who mentioned policy alternatives did tend to define local problems in the same way. The major exception (citizens' complaints about high taxes as opposed to leaders' desire to provide better community

services) was coded separately in constructing the concurrence index.<sup>35</sup>

Another aspect of linkage involves policy *outcomes*. Analysis of the disposition of issues, however, would require detailed case studies and analysis over time: both tasks were beyond the resources of this study. The concurrence index I have used is concerned with leaders' awareness of community problems and their efforts to solve them, not their rate of success: as many students of local government have stressed, even leaders who try to be responsive to citizens may be constrained by state laws and scarcity of local resources.<sup>36</sup>

Stokes and Miller have observed that the process of representation may vary depending on the issue being considered.<sup>37</sup> For this study, because of the large number of issues cited and the small number of respondents in each community, I preferred to use agreement across all issues rather than selecting particular issue areas for comparative analysis.<sup>38</sup> This approach might miss some differences in political factors across issue areas, but it has the advantage of using more of the available data, and does not require the researcher to decide which issues are "important" in each community.

This concurrence index of citizen-leader agreement on community problems offers many advantages. An open-ended approach to defining the local political agenda mitigates the problem of examining community political processes in relation to issues or policy alternatives chosen by the researcher. Citizens as well as leaders have the opportunity to mention problems, so even issues not currently under consideration by the local leadership may be raised. Most important, concurrence in a large number of political units may

1963); M. Kent Jennings, *Community Influentials* (New York: The Free Press of Glencoe, 1964).

<sup>32</sup> See Appendix for more detailed description of the construction of the concurrence index.

<sup>33</sup> See Hansen, doctoral dissertation, chapter 4.

<sup>34</sup> The classic statement is E. E. Schattschneider on the "scope and bias of the pressure system," in *The Semi-Sovereign People* (New York: Holt, Rinehart, & Winston, 1960). See also Roger Cobb and Charles D. Elder, *Participation in American Politics: The Dynamics of Agenda-Building* (Boston: Allyn and Bacon, 1972); and William Gamson, "Stable Unrepresentation in American Society," *American Behavioral Scientist*, 12 (November-December, 1968), 15-21.

<sup>35</sup> The original research design called for specific questions on policy dimensions. Several of these were inadvertently omitted from the elite questionnaire. Verba and Nie, *Participation in America*, pp. 267-285, and Hansen, chapter 4, have reported on those that could be used to compare leaders' and citizens' opinions. Agreement on these was greater in participant communities with competitive political structures, the same setting that was associated with agreement on the salience of issues.

<sup>36</sup> See discussion by Robert L. Lineberry and Ira Sharkansky, *Urban Politics and Public Policy* (New York: Harper and Row, 1971), chapter 2.

<sup>37</sup> Warren E. Miller and Donald Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March, 1963), 45-56.

<sup>38</sup> See Susan B. Hansen, "Citizen-Leader Concurrence on Community Problems" (mimeo, University of Illinois, 1974) for discussion of concurrence in different issue areas. Agreement on the salience of economic and welfare problems is more likely in competitive participant communities, while political factors are less important for concurrence on public services.

be compared even if the issues that concern each community are different.

Even if it were possible to determine whether citizens and leaders agreed on some question of public policy, simple association indicates nothing about the direction of causality. As V. O. Key has cautioned,<sup>39</sup> "One must discard simplistic conceptions, such as the notion that in some way public opinion exudes from the mass of men and produces guidelines for governmental action. A complex interaction occurs . . ." The factors that may be associated with high levels of citizen-leader agreement in a polity may tell us something about the process of linkage and enable us to make plausible causal inferences. Using a standard index of concurrence in a variety of communities makes such a comparative analysis possible.

The concurrence index could also be used to compare individual local *leaders* with respect to their agreement with citizens concerning community problems.<sup>40</sup> My purpose here, however, is to compare the extent to which *communities* vary in concurrence, and to focus on interaction between leaders and citizens within a particular political setting. For this reason, concurrence scores were averaged across sets of leaders interviewed in a given community. This enables me to consider the relationship between citizen opinion, on the one hand, and the attitudes of the *collective* leadership of political units, on the other. Eulau and Prewitt have used a similar approach in their analysis of the representational roles of city councils,<sup>41</sup> following the suggestion of Hanna Pitkin that "representation must be understood at the public level."<sup>42</sup>

Data concerning the economic, social, and political characteristics of these communities were also gathered.<sup>43</sup> The communities vary considerably in political form, degree of electoral competition, party activity, and levels of citizen participa-

tion. By using the standardized concurrence measure for all communities, citizen-leader agreement on local problems can be compared under the varied conditions.

### Participation and Concurrence: A Closer Look

If the concurrence scores of elected officials were higher in communities with contested elections and active citizen participation, this would suggest that political structure aided the process of electoral influence over leaders by citizens. But if concurrence scores of nonelected officials were also higher in such communities, this would support the notion that political structures and processes can help communicate citizen preference to leaders. These two patterns can be compared since the tenure of the elected heads of government in this study depends upon the electoral process, but the other community leaders are not accountable to voters.<sup>44</sup>

Table 1 shows the Pearson correlations between concurrence scores and participation rates for the two groups of leaders, using both election statistics and community averages of citizens' scores for each mode of participation.<sup>45</sup> Elected and nonelected leaders do not differ significantly in their levels of agreement with citizens: concurrence scores average 18.8 for heads of government and 18.0 for the other elite respondents. Participation is positively associated with the concurrence

<sup>39</sup>NORC interviewers were instructed to interview the "elected head of local government," who could be the mayor, first selectman, justice of the peace, or township trustee. Eight of them were picked from the members of an elected board (councilmen, county supervisors), the rest elected directly by citizens. One might well argue that the Democratic and Republican party leaders should be considered together with the heads of local government; while the party officials are not officeholders, in many ways they must be sensitive to political processes and to elections. In contrast, the business, school and newspaper elites are further removed from accountability to voters.

The analysis reported here was repeated considering three groups of leaders instead of two: heads of government, party leaders, and other leaders. But political structure and electoral participation had less impact on party leaders' concurrence than on the scores of elected officials. In fact, there was little difference between party officials and nonelected leaders in these respects. A strict definition of electoral accountability was therefore adopted, and those officials whose jobs depended upon election were considered separately from the others.

<sup>40</sup>The election statistics for the most recent local election were obtained by NORC interviewers from the local government heads. For the sample data, each cross-section respondent was assigned a score on the four modes and an overall index of participation. The portion of each respondent's score that could be attributed to social class was then partialled out, and the averages for these residual participation indices were computed for all the respondents sampled in a given community. These procedures are described more fully in Verba and Nie, pp. 309-314.

<sup>39</sup>Key, *Public Opinion and American Democracy*, p. 410.

<sup>40</sup>See Hansen, chapter 4. The different types of leaders in this study did not differ significantly in levels of concurrence, although there were some minor variations among leaders across the three sets of problems considered.

<sup>41</sup>Eulau and Prewitt, "Political Matrix and Political Representation."

<sup>42</sup>Hanna F. Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1967), p. 224.

<sup>43</sup>Reports of NORC interviewers and of the elite respondents were used as sources of information on the sixty-four communities in the study, as were the following published sources: *The Rand McNally Commercial Atlas and Marketing Guide* (Chicago, 1968); U.S. Bureau of the Census, *Census of Population 1960* and *Census of Government 1962* (Washington, D.C.: U.S. Government Printing Office, 1963); *The County and City Data Book* (U.S. Bureau of the Census Statistical Abstract Supplement, 1967 and 1968).

**Table 1. Pearson Correlations: Community Participation Levels and Concurrence Scores of Local Leaders**

	Correlations with Concurrence Scores	
	Heads of Govern- ment	Other Local Leaders
<i>Cross-section Sample Data:</i>		
<i>Community Averages of Participation Indices, 64 Communities</i>		
Voting	.40**	.36**
Campaign Activity	.30*	.23*
Cooperative Participation	.06	.05
Citizen Contacts	.08	.12
Overall Index	.30*	.27*
<i>Election Statistics for Last Local Election, 39 Communities</i>		
% Voting	.13	.27*

\* Significant at .05 level

\*\* Significant at .01 level.

scores of both groups. Voting and campaign activity have strong correlations not only with concurrence scores of elected heads of government, but also with the scores of school officials, party and business leaders, and newspapermen. Voting turnout in the last local election is more strongly linked to the concurrence scores of the nonelected leaders than to those of heads of government.

The expected differences between the communications and control aspects of participation, then, did not appear. Although citizen contacts are more highly correlated with nonelected leaders' concurrence scores, and electoral participation is somewhat more salient for heads of government, the differences between the two groups are not significant.

These results suggest that *both* the communications and control aspects of participation are important for understanding linkage. The concurrence scores of both types of leaders are significantly associated with citizen electoral activity. Specific issues are rarely clarified in election campaigns, and the information that citizen voting or electoral activity communicate to local leaders may not be very precise. Nevertheless, evidence of citizen *interest* in local elections may persuade elected officials to make more of an effort to find out what citizens of their communities are concerned about.<sup>46</sup> Nonelected leaders may try to maintain access to those holding formal political

power by making sure their opinions and actions are in tune with those of successfully elected leaders. Educational, business, and economic leaders might gain influence with elected officials if they could demonstrate that their policies have the support of involved citizens, parties, or organizations. Popular support can be important for both governmental and nongovernmental leaders as a resource in the political bargaining process.

Let us now turn to the political setting within which participation takes place in order to test some additional hypotheses about the process of linkage.

#### Political Structure: Effects on Concurrence and Participation

As suggested earlier, community political structure may affect concurrence in several ways: by making alternatives more or less salient; by offering citizens a choice among candidates and policies; by facilitating communication between citizens and leaders; and by encouraging participation. Let us consider each of these in turn. As before, the concurrence scores of elected government executives will be contrasted with the scores of other types of community leaders, in order to give a clearer indication of the dynamics of the processes involved.

**Salience of Alternatives.** Two aspects of local governmental structure may make electoral alternatives more visible to voters: presence of party labels on election ballots, and direct election of the head of local government. Concurrence, it is hypothesized, should therefore be higher in communities where elections are legally partisan, or where the mayor or other governmental executive officer is elected directly by the voters.

As Table 2 indicates, this is the case, but the relationships are stronger (as might be expected) for heads of government than for the nongovernmental leaders. In those twenty-two communities which are legally partisan, concurrence scores of local government heads are significantly higher than in nonpartisan communities. The scores for the other local leaders vary in the expected direction, but the difference is not significant.

Proponents of electoral reform have long argued that direct election of officeholders would give the electorate a better basis for voting decisions than elections which only ratified decisions made by party caucuses or by appointive boards. Present data do not permit a direct test of citizen awareness of the local government's chief executive or of his policies. Nevertheless, concurrence scores of directly elected executives (including mayors, first selectmen, and town supervisors or township trustees) are significantly higher than the scores of those selected from the membership

<sup>46</sup> In communities with high voting rates, these nonelectoral forms of participation do make a difference in concurrence (Verba and Nie, p. 326).

Table 2. Concurrence, Partisanship, Type of Election

	Mean Community Concurrence Scores		
	Heads of Local Government	Other Community Leaders	N
<i>Local elections:</i>			
Partisan	22.4	18.7	23
Nonpartisan	16.2*	17.1	32
<i>Method of Election of Local Government Executive: All</i>			
Direct (by popular vote)	19.6	18.2	47
Indirect (chosen by membership of elected council)	11.4*	17.7	8
<i>Mayors only:</i>			
Directly Elected	21.8	19.1	31
Indirectly Elected	11.5*	12.9*	6

\* Difference of means significant at .05 level (*t*-test).

of a local council or board. Scores of nongovernmental elites are slightly higher in communities with elected heads of government, and significantly higher if that governmental head is a mayor. At least for elected heads of government, then, concurrence scores are higher the more visible the alternatives presented to citizens.

**Availability of alternatives.** Formal-legal structure is only the skeleton of a community's political system. Opportunity for popular election of a mayor, or legal partisanship, may have little meaning for voters if there is no choice among candidates, parties, or policies. The absence of contending groups, such as political parties or organizations concerned with community problems, may limit the discussion of alternative policies in a community.

Ideally, any index of competition or of the availability of alternatives should incorporate data from several elections over a period of years.<sup>47</sup> Two limited indicators of competition are available for these communities, however. One is the percentage of votes received by the runner-up in the most recent election of the head of local government (available for 35 communities). The other is an assessment by the local government executive of the frequency of contested elections in his community. These two indicators roughly correspond with two of the methods Eulau and Prewitt used to assess the competitiveness of local

political systems: closeness of vote and frequency of contests (or more accurately, *perception* of frequency of contests.)<sup>48</sup> Data on their third measure, incidence of incumbent eviction, are not available here.<sup>49</sup>

As Figure 1 indicates, there is a moderately strong association between each of these indices and the concurrence scores of local leaders—elected or not. Where elections are seldom contested, concurrence scores average 13.7 for elected local officials and 15.0 for other leaders. But where elections are contested often, concurrence scores average 23.1 and 21.1 for the two groups, respectively. The same pattern holds for the communities where data are available on the last local election. The closer the outcome of that election, the more likely *both* types of local leaders were to share citizen perspectives on community problems. The Pearson correlations are significant at the .05 level.

Prewitt and others have suggested that electoral accountability cannot operate unless the political ambitions of elected officials are taken into account.<sup>50</sup> Electoral sanctions presumably apply

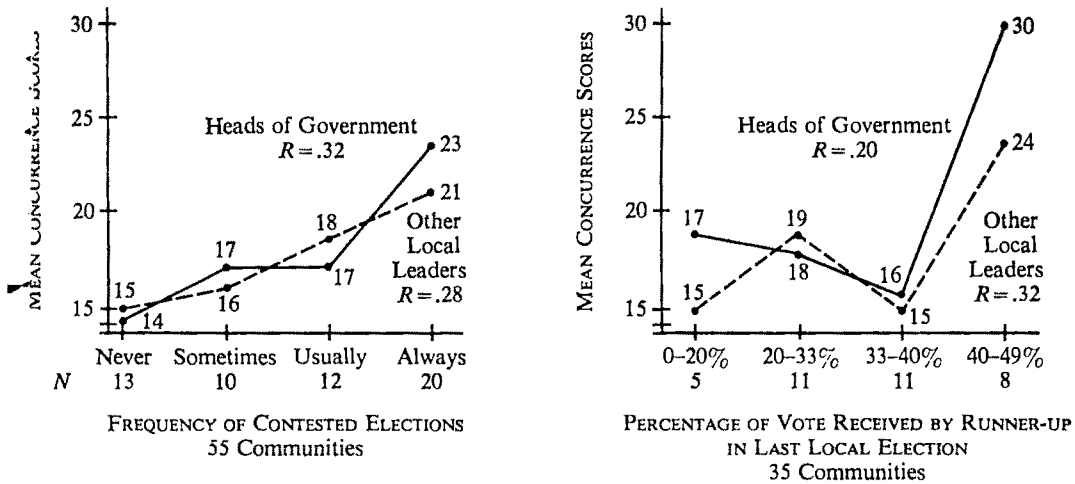
<sup>48</sup> Perceptions of competition for office by candidates may be as important as the actual level of competition. See John W. Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1968), pp. 147–150; David Leuthold, *Electioneering in a Democracy: Campaigns for Congress* (New York: John Wiley and Sons, 1968), pp. 20–30.

<sup>49</sup> Eulau and Prewitt, *The Labyrinth of Democracy*, pp. 229–235. It is possible for a community to be competitive in one sense (that there are at least two candidates for office in most local elections) but not in others (incumbents may very seldom be turned out of office, or the proportion of the vote received by the challenging party or faction may be quite small).

<sup>50</sup> Prewitt, "Political Ambition"; Joseph Schlesinger, *Ambition and Politics* (Chicago: Rand McNally, 1966).

<sup>47</sup> One such measure (the mean deviation of the vote among all candidates for city council across five elections) was used by Gordon S. Black, "A Theory of Political Ambition: Career Coice and the Role of Structural Incentives," *American Political Science Review*, 66 (March, 1972), 147. A classic index of party competition is that suggested by Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967), pp. 44–67, 114–132.





\* The correlation coefficients (Pearson product-moment) are all significant at about the .05 level.

Figure 1. Concurrence and Electoral Competition\*

only to the elected governmental heads in these communities, but there is no reason to assume these are effective. Data on whether these heads of government plan to continue in office or to seek higher office are not available. How, then, can one explain the significantly higher concurrence scores for *both* elected and nonelected officials that occur in competitive communities?

Competitive elections do not induce local leaders to share citizen priorities only by making it easier for citizens to hold leaders accountable. Both the direct and indirect functions of elections should be considered.<sup>51</sup> Perceptions of competition for office, or close elections, could persuade community leaders to expend more effort to find out what concerns citizens have. Their success in this enterprise, however, may depend on how local political processes affect the communication of citizen preferences.

**Communication of Citizens' Views.** Political parties and local organizations can articulate citizen demands, aggregate conflicting interests, and otherwise facilitate communications between citizens and their leaders. But party and organizational activity may also be the expression of divisions within a community, or may stimulate controversy. If a community is deeply divided along political, economic, or ethnic lines, leaders will not be able to please everyone. Thus a curvilinear relationship might be expected between concurrence and party or organizational activity. Nonexistent political parties and inactive organizations cannot function to aggregate or articulate

interests. If these groups are moderately active, concurrence should be higher. But where both parties are active, or if numerous organizations become involved in local politics, considerable diversity of interests among citizens is likely, and concurrence may suffer as a result.<sup>52</sup>

The predicted relationship does not appear when the involvement of organizations in community affairs (specifically, whether they take stands on local issues rather than functioning purely as social or professional groups) is considered in relation to concurrence. This may be due to the limitations of this index of organizational activity.<sup>53</sup> Or the impact of organizations may lie in their encouragement of electoral competition (more frequent in communities with active organizations), rather than in their direct effects on citizen-leader agreement.

But as Table 3 shows, the predicted curvilinear relationship does appear when concurrence scores are compared across communities with varying levels of party activity. In many of these small

<sup>52</sup> See discussion by Peter H. Rossi, "Power and Community Structure," in *Political Sociology*, ed. Coser, pp. 132-145. Rossi suggests that patterns of community power (caucus rule, polythetic, amorphous, or pyramidal) may be related to coincidence of party, class, and ethnic cleavages.

<sup>53</sup> Neither the presence of these organizations in the community nor leaders' membership in such organizations proved to bear any relationship to concurrence. More complete data on organizational membership or activity, of course, might show some relationship to concurrence. The scale used here is a simple count for each community of the number of local organizations (business, labor, farm, professional, or nationality groups) which took stands on local issues or backed candidates.

<sup>51</sup> See Pomper, *Elections in America*, p. 13 ff., for a comparison of long and short-term election effects.

Table 3. Political Party Strength, Organizational Activity, and Concurrence

	Average Community Concurrence Scores			
	Heads of Local Government	N	Other Local Leaders	N
Organizations Take Stands on Local Issues:				
Never	10.1	14	16.8	14
Seldom	23.8	15	19.9	15
Sometimes	16.8	13	15.0	13
Frequently	21.3	14	19.4	14
Pearson <i>r</i>	.17		.04	
Political Party Activity				
None	16.0	26	16.7	28
Both parties weak	16.3	6	18.2	8
Republicans active	24.6	8	19.2	9
Democrats active	28.8	7	19.7	9
Both parties active	15.8	9	19.0	10
Pearson <i>r</i> <sup>a</sup>	.16		.12	

<sup>a</sup> Since the relationship is clearly curvilinear, the interclass correlation should be used. This is .37 for heads of government; and since this is more than twice the Pearson correlation, considerable curvilinearity is indicated. For other local leaders, the interclass correlation is nearly identical with the Pearson correlation (.14) indicating the relationship is essentially linear, although extremely weak.

communities, there is no indication of party activity (and the fact that over half of these communities are nonpartisan certainly does not encourage party activity).<sup>54</sup> Where one party is quite active and the other much less active, the concurrence scores of heads of government are considerably higher. Where the parties are equal in strength (whether both are active, weak, or non-existent), citizen-leader agreement tends to be lower than in predominantly one-party areas. A similar pattern holds for nonelected local leaders, but the relationship is much weaker.

A curvilinear relationship might have been expected also between community concurrence scores and the indices of electoral competition discussed earlier. If a community was closely divided over an election, concurrence should be relatively low; whoever won the election would have the support of only a bare majority or plurality of citizens. In relatively homogeneous communities, the presence of competition could encourage all candidates to ascertain citizen priorities. They might well reach similar conclusions, and competition would focus on candidates' personal appeals or administrative competence (campaigns characteristic of many suburbs and small towns.) But if electoral competition coincided with party or class cleavages, competition should

lead to *lower* overall concurrence.

To test for this possibility, another community agreement score was calculated, analogous to citizen-elite concurrence, but measuring how well the priorities of active citizen participators agreed with those of less active citizens.<sup>55</sup> The relationship between electoral competition and concurrence was then plotted for communities ranking above and below the median on this index of consensus among citizens.

The results are shown in Figure 2. In low-consensus communities, the predicted curvilinear relationship between competition and concurrence does appear, although it is very weak. In high-consensus communities, however, citizen-leader agreement increases as the frequency of contested elections increases.

Of all the aspects of community political structure considered thus far, *electoral competition* appears to have the greatest impact on the concurrence scores of local leaders, elected or not. But the degree of competition, and its effect on citizen-leader agreement, is dependent on many aspects of community political life. Where elections are partisan, political parties are active, and organizations are politically involved, the probability of electoral competition is significantly higher.<sup>56</sup> All this political activity may encourage

<sup>54</sup> The indices of party activity were computed from information supplied by local party leaders on party resources (money, staff, volunteers); efforts to recruit candidates; and activity in local elections. Fifty-five per cent of the partisan communities in this sample have one or both local parties active, as compared with only thirty-four per cent of the non-partisan communities.

<sup>55</sup> This measure was used by Verba and Nie, and is described in *Participation in America*, chapter 19, p. 320.

<sup>56</sup> Gamma correlations between frequency of contested elections and other political factors are .59 for partisanship, .42 for direct election of government executive, .32 for organizational involvement, and .21 for party activity.

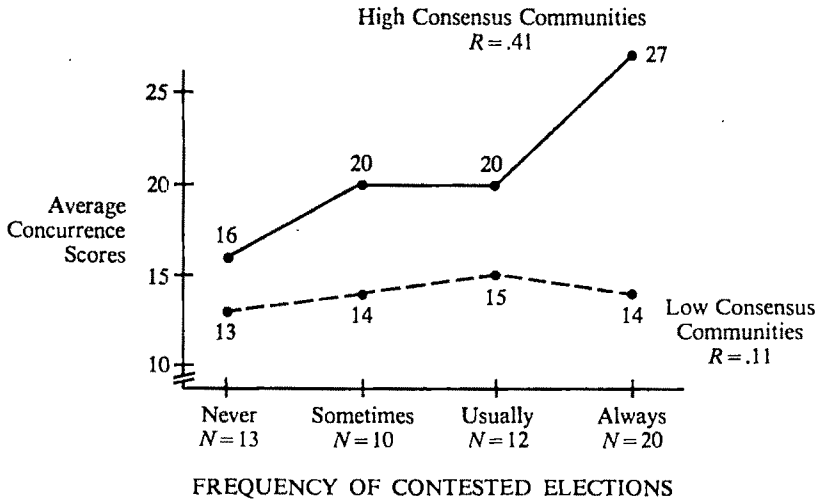


Figure 2. Electoral Competition and Concurrence in High and Low Consensus Communities

communication and help citizens hold their leaders accountable. But such activity may also be the expression of divisions within a community. In that case, leaders can only be responsive to some of the citizens.<sup>57</sup>

**Political Setting and Participation.** Previous research has suggested that the political setting of a community can affect rates of citizen participation.<sup>58</sup> Alford and Lee found that voting turnout was substantially higher in communities with partisan elections, mayor-council form of government, and ward rather than at-large representation.<sup>59</sup> Political party activity may also increase voting turnout in local elections, especially primaries and referenda.<sup>60</sup>

In these communities also, citizen participation (particularly voting) is affected by the local politi-

cal environment. Partisanship, political party activity, and electoral competition encourage campaign activity as well as voting by citizens, as the simple correlations in Table 4 indicate. Organizational involvement and direct election of the head of government seem to have little effect on any mode of participation.

Table 4 also shows the independent effect (indicated by beta coefficients) of each of the political structure variables on participation, controlling for other structural variables. Electoral competition and partisanship have the largest independent effects on voting and campaign activity. Party and organizational activity have a negative impact on voting turnout when the effects of the other factors are controlled. About twenty-six per cent of the variation in citizen voting can be explained by political structure.

As might be expected, information on a community's political structure does not help us as much in predicting nonelectoral participation as it does with voting. Citizens can be active in organizations and contact public officials on their own initiative, regardless of the legal or institutional environment. Political structure explains less than ten per cent of the variance in participation other than voting. Citizens' organizational activity and rates of contacting, however, are greater in partisan communities, while communal participation rates are *lower* in communities with active political parties: perhaps organizational involvement substitutes for party activity for some people.

Another important influence on participation is the degree of coincidence of state and national elections with local contests. Citizens participate more when other elections do coincide. Even within such communities, however, the local po-

<sup>57</sup> Charles F. Cnudde has examined linkages between policy outputs and public opinion structures in the American states: he also notes a curvilinear relationship with party competition. "Public Opinion and State Politics," in *State Politics: Readings in Political Behavior*, ed. Robert Crew (Belmont, California: Wadsworth, 1968), pp. 165-184.

<sup>58</sup> Some of the relevant literature is reviewed in "Political Participation as a Function of Political Setting," chapter 4 of *Political Participation*, by Lester W. Milbrath (Chicago: Rand McNally, 1965); and Robert R. Alford and Harry M. Scoble, "Sources of Local Political Involvement," *American Political Science Review*, 62 (December, 1968), 1192-1206.

<sup>59</sup> Robert R. Alford and Eugene C. Lee noted correlations between voter turnout and partisanship ranging from .28 to .43 in their sample of American cities over 25,000 in population. "Voting Turnout in American Cities," *American Political Science Review*, 62 (September, 1968), 1192-1206.

<sup>60</sup> Leuthold found this to be the case in his comparison of competitive and non-competitive congressional districts: *Electioneering in a Democracy*, p. 120.

Table 4. Political Structure and Participation: Correlation and Regression Coefficients for 53 Communities

	Partisanship	Frequency of Contested Elections	Party Activity	Organizational Involvement	Direct Election of Head of Government	R	R <sup>2</sup>
Voting							
<i>r</i> <sup>a</sup>	.32*	.38*	-.15	-.07	.10		
B	.21*	.39*	-.25	-.15	-.04	.51	.26
Electoral Participation							
<i>r</i>	.15	.19	.17	.02	.02		
B	.08	.16	.13	.06	.04	.25	.06
Communal Participation							
<i>r</i>	.12	-.03	-.19	-.15	-.03		
B	.18	-.01	-.19	-.14	-.09	.29	.08
Citizen Contacts							
<i>r</i>	.10	-.04	.03	.07	.11		
B	.13	-.17	.01	.13	.12	.21	.04
Overall Participation							
<i>r</i>	.25*	.19	-.06	-.08	.04		
B	.23*	.17	-.11	-.12	-.08	.32	.10

<sup>a</sup> *r* = Pearson correlation; B = standardized regression coefficient (beta).

\* Significant at .05 level.

litical environment affects voting turnout and other types of citizen participation.<sup>61</sup>

#### Models of Concurrence

Both participation and political structure are related to concurrence. Let us now turn to a question raised earlier: under what conditions will participation have the greatest impact on community leaders' agreement with citizens? Several models may be suggested:<sup>62</sup>

(1) Concurrence may be high in communities with certain political structures, whether or not people participate.

<sup>61</sup> Alford and Lee and others have found turnout to be higher when local elections are held at the same time as state or federal elections. This holds for these communities as well: even controlling for overlap with other elections, local political environment still affects participation. This problem is discussed in "Citizen Participation, Political Parties, and Democratic Theory. An Analysis of Local Politics in England," by Paul E. Peterson and Paul Kantor (paper presented at the American Political Science Association convention, Los Angeles, Sept., 1970), pp. 3-5, 17-20.

<sup>62</sup> Other models could be postulated that might apply to these data. Concurrence between leaders and citizens might encourage participation, or might affect political structure. Without time-series data, these alternative models cannot be fully evaluated. The models discussed here are more logical in temporal sequence: political structure and participation rates are relatively stable features of community life, while the substance of local issues, and citizen-leader agreement on them, may vary considerably over time. As Arthur S. Goldberg has suggested, "Scientifically relevant causal explanations inhere only in our theories." "Discerning a Causal Pattern Among Data on Voting Behavior," *American Political Science Review* 60 (1966), 913.

(2) Participant communities may have high levels of concurrence, regardless of the political structure.

(3) The conjoint influence of participation and political structure may have a greater effect on concurrence than either alone.

Several methods are available for testing alternative hypotheses. Path analysis and Simon-Blalock techniques for causal modeling were considered.<sup>63</sup> Because of the number of variables in the models, the generally low levels of the correlations, and the evidence (considered below) of strong interaction effects, efforts to establish or eliminate alternative causal paths within these models proved inconclusive.<sup>64</sup> I have chosen multiple regression in order to evaluate the first and second models and to establish the relative importance of several factors associated with concurrence. For the third model, levels of concurrence in communities with varying levels of participation and competition will be compared.

<sup>63</sup> Otis Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, 72 (July, 1966), 1-16. This and several other articles explaining techniques applicable to social science are included in *Causal Models in the Social Sciences*, ed. Hubert M. Blalock, Jr. (Chicago: Aldine-Atherton, 1971). The technique developed by Blalock (*Causal Inference in Non-Experimental Research*, Chapel Hill: University of North Carolina Press, 1971), and Herbert M. Simon (*Models of Man*, New York: John Wiley and Sons, 1957), relies on partial correlations.

<sup>64</sup> See Hugh D. Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," *American Political Science Review*, 62 (December, 1968), 1258-1264, for a critical analysis of multicollinearity and the reporting of assumptions as conclusions.

Table 5. Regression Analysis: Participation, Political Structure, and Concurrence for 53 Communities

Independent Variables:	Heads of Government			Other Local Leaders		
	<i>b</i>	<i>B</i>	sig.	<i>b</i>	<i>B</i>	sig.
Voting	1.70	.38	.001	1.15	.38	.001
Electoral Competition	*		n.s.	1.41	.19	.10
Partisanship	*		n.s.	-2.34	-.13	n.s.
Party Activity	1.09	.10	n.s.	1.16	.15	n.s.
Organizational Activity	2.14	.18	.10	.13	.02	n.s.
Direct Election of Head of Government	6.69	.19	.10	*		n.s.
Intercept	17.84			14.09		
<i>R</i>	.46			.46		
<i>R</i> <sup>2</sup>	.22			.21		

\* *F*-level too low for computation of regression coefficients.

The *b* and beta coefficients<sup>65</sup> in Table 5 indicate the independent contribution of voting and various political structures<sup>66</sup> to the concurrence scores of elected and nonelected leaders. Frequency of contested elections shows some independent association with concurrence for nonelected leaders, as do organizational activity and type of election for elected leaders. Otherwise, none of the other political-structure variables have much relationships to concurrence for either set of community elites. The beta for participation, however, is highly significant when the effects of political structure on concurrence are controlled.<sup>67</sup>

One might have expected that political factors would be more important for concurrence with

elected leaders. For organizational activity and type of government, this is the case, but party activity, partisanship, and competition show larger regression coefficients for nonelected leaders. The beta coefficient for voting (.38) is the same for both groups, and about 21 per cent of the variance in concurrence for each set of leaders is explained by participation and political structure.

One might well conclude on the basis of Table 5 that Model 2 is most reasonable. Voting has a significant direct relationship with concurrence, accounting for approximately fifteen per cent of the variance in the scores of both groups of leaders. An additional five or six per cent of the variance can be explained, however, by including political structure.

Nevertheless, the linear additive statistical model which was assumed in computing the regression coefficients in Table 5 does not give a complete picture of the relationships in the data. It has already been shown that political structure (especially electoral competition) influences participation rates. This suggests that participation may have a greater impact on concurrence within competitive communities: that there may be some *interaction* between political structure and participation. To test for this, concurrence scores for the two groups of leaders were computed in four types of communities: "apolitical" (low participation, infrequently contested elections); "competitive elitist" (contested elections, relatively low participation); "nonpartisan" (high participation, but few contests for local office); and finally, "competitive democracies" (active participation and frequently contested elections).

As can be clearly seen from Table 6, it is the interaction between participation and competition which most influences concurrence. There is

<sup>65</sup> Blalock has argued that unstandardized regression coefficients may offer certain interpretive advantages ("Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review*, 61 [March, 1967], 130-136). Duncan, ("Path Analysis") and Sewall Wright ("Path Coefficients and Path Regressions," in Blalock, *Causal Models in the Social Sciences*, 1971, pp. 101-114) prefer beta coefficients. I have reported both here, but because I have used a variety of scales and dummy variables, the beta coefficients can be more readily interpreted for these data. See also Susan B. Hansen, "Participation, Political Structure, and Concurrence" (paper presented at the American Political Science Association conference, New Orleans, 1973), where a path analysis of these data is reported.

<sup>66</sup> The indices of electoral competition and organizational activity have been described earlier. Because of the curvilinear relationship noted between party activity and concurrence, dummy variables could have been used for different categories of party strength. But to reduce the number of variables required, a unidimensional party scale was used (both parties absent, both weak, one strong, both strong).

<sup>67</sup> Citizen voting (the residual factor index based on cross-section sample data) was used to indicate participation rates in communities. The relationships observed here do not depend upon the indicators of political structure or participation that are used. If the percentage of votes received by the runner-up is taken as an indicator of competition, and if overall participation or campaign activity is used as an indi-

cator of community participation, the same pattern holds. Modes of participation other than voting explain less of the variance in concurrence, however.

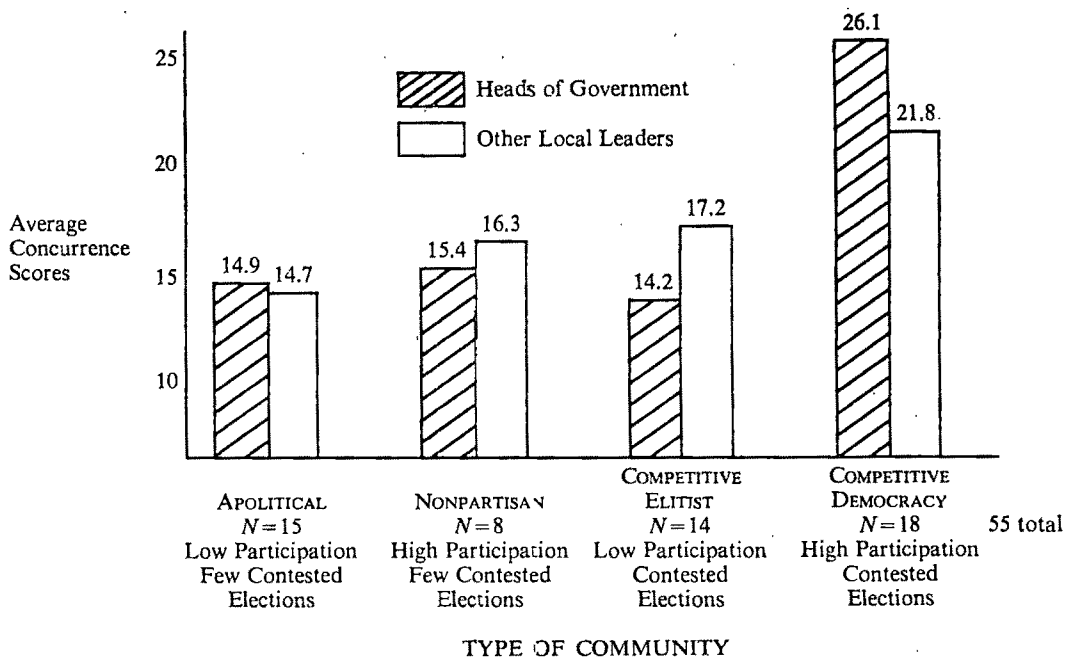


Table 6. Mean Concurrence Scores of Local Leaders by Political Typology of Communities

very little difference in concurrence for either set of leaders among the apolitical, nonpartisan, and competitive-elitist communities (although scores for nonelected leaders are somewhat higher in competitive-elitist communities). The highest concurrence scores for both groups of leaders are found in *competitive democracies* where citizens are active and elections are contested. High participation has little effect if there are no alternative outcomes available; and competition among a small group of activists is not sufficient to influence the agreement of local leaders with citizens. The political structure of a community, then, serves to create conditions under which citizen participation may have the greatest impact on concurrence.<sup>68</sup>

#### Differential Concurrence as a Function of Political Setting

In terms of concurrence with local leaders, which groups benefit the most from different political structures? Not all citizens participate at

the local level: voting turnout in local elections is often extremely low. When few participate, persons of fairly high social status are most likely to be active and thus to reap whatever benefits may be gained from participation. If high and low participators generally agreed on community problems, participation would increase concurrence to all. But if less active persons had different policy priorities, conditions that encouraged their participation should also lead to higher concurrence for them. It has been suggested that lower classes should benefit from "unreformed" political systems.<sup>69</sup> The classic argument of V. O. Key in *Southern Politics* is that not only blacks, but lower-class whites are adversely affected by one-party politics, factionalism, and restricted opportunities for participation.<sup>70</sup>

We have so far examined the level of community agreement between *all* citizens and two groups of local leaders. But it is possible to test the effects of participation and political structure on concurrence for different groups within each community by calculating concurrence scores separately for persons of different social classes, or different levels of political activity.

The first row of figures in Table 7 represents community averages of concurrence scores for persons of lower, middle, and upper socioeco-

<sup>68</sup> Interaction could also be demonstrated by using dummy variables or interaction terms within the multiple regression equations. Because of the number of variables, large number of potential interactions terms, and relatively few cases, this was not feasible. If a simple model was used (voting and rates of competition, plus a product of these terms to indicate interaction), the resulting beta coefficients were voting,  $-.29$ ; competition,  $.11$ ; interaction term,  $.71$ ;  $R^2 = .25$ . Thus the *interaction* between participation and competition explains more of the variation in concurrence than either alone.

<sup>69</sup> Lineberry and Fowler, "Reformism and Public Policy."

<sup>70</sup> V. O. Key, *Southern Politics* (New York: Alfred A. Knopf, 1949), pp. 307-310, 655-661.

Table 7. Political Structure and Concurrence to Socioeconomic and Participant Citizen Groups

	Community Concurrence Scores						No. of Communities
	Social Class <sup>a</sup>			Participation Level <sup>b</sup>			
	Low	Med	High	Low	Med	High	
<i>Average Across All Communities.</i>	21.	23.	27.	23.	23.	28.	64
<i>Elections Contested:</i>							
Never	16.	19.	26.	18.	19.	22.	13
Sometimes	19.	16.	26.	19.	23.	20.	10
Usually	14.	20.	24.	20.	18.	25.	12
Always	29.	32.	30.	29.	29.	37.	20
Pearson <i>r</i>	.25	.31	.08	.22	.21	.38	55
<i>% of Vote Received by Runner-up:</i>							
0-20%	14.	21.	21.	22.	17.	29.	5
20-33%	16.	24.	30.	23.	23.	29.	11
34-40%	22.	20.	26.	19.	21.	23.	11
41-49%	40.	37.	24.	30.	36.	40.	8
Pearson <i>r</i>	.36	.25	.07	.13	.33	.22	35
<i>Partisanship</i>							
Absent	19.	21.	29.	22.	22.	26.	32
Present	24.	27.	24.	23.	25.	30.	22
Pearson <i>r</i>	.12	.16	-.13	.03	.08	.11	54
<i>Political Parties:</i>							
Neither exists	16.2	22.9	28.7	19.5	22.3	23.4	28
Both Weak	19.2	22.3	30.7	20.2	24.6	27.3	8
Strong Rep.	25.0	23.5	31.6	25.1	28.3	32.4	9
Strong Dem.	29.3	30.0	27.3	30.0	26.1	33.3	9
Both Strong	24.7	21.2	24.2	18.8	23.7	30.3	10
Pearson <i>r</i>	.25	.02	-.05	.08	.08	.23	64
<i>Multiple R for Political Structure</i>	.32	.35	.25	.25	.22	.43	

<sup>a</sup> Education, income, and occupation weighted equally: scale divided in thirds.

<sup>b</sup> Based on three equal divisions of overall participation index.

conomic status; and for persons scoring at three different levels on the overall Verba-Nie index of participation. First, looking at the average concurrence scores for the three socioeconomic groups across all communities, one can see that persons of higher social status are more likely to find local leaders in agreement with them. This pattern may occur because local leaders themselves are drawn from the upper social strata, and could be expected to have better communications with, and be more sympathetic to, the desires of this stratum. Also, upper-status persons participate more: they have more of a say in local elections and in political parties and local organizations.<sup>71</sup>

But these advantages of upper-status citizens may be overcome to some extent by community

political structure. In Table 7, subgroup scores are averaged within communities with different political structures. Consider the concurrence scores for the three social classes in communities with varying degrees of competition, party activity, and partisan local elections. Competition benefits *all* social groups to some extent. But persons in the lowest third of the SES scale benefit the most. Where elections are infrequently contested, concurrence between all local leaders and this group averages 16.6; where electoral contests are frequent, the average jumps to 29—very little different from the concurrence scores of the middle and upper classes. The pattern is even more striking when closeness of election outcomes is considered: leaders' concurrence with lower-status persons' increases from 14.4 to 40.0 as electoral competition increases. The middle SES group also benefits from competition, while upper-status citizens benefit very little from increased electoral competition. Also, active political parties help lower-status persons more.

<sup>71</sup> See Verba and Nie, chapter 17, for a fuller discussion of the relationships among social class, participation and concurrence; and chapter 19 for a more detailed discussion of the "who benefits" problem with reference to types of participators.

Both the lower and middle SES groups fare better in terms of concurrence in partisan communities. But as Lineberry and Fowler's analysis suggested, partisanship has a *negative* effect on the concurrence scores of upper-status persons. The regression coefficients indicate that political structure explains more of the variation in the concurrence scores of lower- than of upper-status persons.

Turning now to concurrence scores of participatory subgroups, we see that the most active participators (those in the upper third of participators according to the Verba-Nie index) tend to agree with local leaders somewhat more strongly. Since in devising the participation index used here to divide the citizen sample the effects of social class were partialled out, this differential advantage may be presumed to be due to the greater participation of this group rather than to their social class (although of course many of these active citizens do score at a high level on the SES scale).

But in this case the differential impact of political structure does not overcome the advantage in concurrence held by active participators: rather, they benefit *more* in partisan, competitive situations. All participants benefit from electoral competition and partisanship; each group's scores average about ten points higher on the concurrence index in communities with active parties and contested elections than in areas where elections are seldom contested. Increased electoral competition, nevertheless, tends to *augment* the differences in concurrence among these three subgroups: the correlations between concurrence and political structure are higher for active participators.

To some extent, then, community political structure can serve to overcome the advantage of greater agreement with leaders that upper classes have. Participators (no matter what their social class) find that their advantage over non-participators is increased in a competitive political setting. Active participation seems to make little difference in an amorphous political environment where policy alternatives are not available or are not salient. This evidence confirms that reported earlier: concurrence is highest with participators in "competitive democracies."

Where there is disagreement between active and less active participators, leaders may only concur with a portion of the citizenry; and active participation may make class, race, or ethnic differences more salient. How might political structure affect the relationship between citizen consensus and citizen-leader concurrence?

Table 8 compares the impact of voting rates and political structure on citizen-leader agreement in high- and low-consensus communities. As Verba and Nie reported, electoral participation has little effect on concurrence in low-consensus

Table 8. Political Structure, Participation, and Concurrence in High- and Low-Consensus Communities

	Multiple Regression Coefficients	
	Heads of Government	Other Local Leaders
<i>Low Consensus Communities</i> (N = 29)		
Political Structure <sup>a</sup>	.42	.34
Voting rates	-.20	.02
Both	.43	.37
<i>High Consensus Communities</i> (N = 26)		
Political Structure <sup>a</sup>	.39	.51
Voting Rates	.50	.46
Both	.54	.68

<sup>a</sup> Includes electoral competition, partisanship, political party activity, organizational involvement, and method of selection of head of local government.

communities.<sup>72</sup> Here a negative relationship holds for heads of local government, and essentially no relationship for other local leaders. But political structure *is* associated with citizen-leader agreement in low-consensus communities, especially for local government executives ( $R = .42$ ). This could be due to either information or influence: even if citizens do not agree, political factors may help communicate at least some of their concerns. Or competition may appear more threatening in a divided community, and may lead to greater efforts to ascertain citizen preferences.

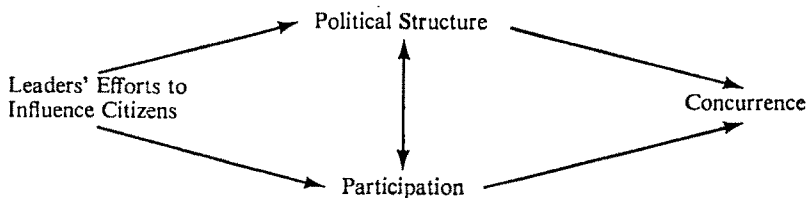
In high-consensus communities, on the other hand, voting increases concurrence with both groups of leaders. For elected officials, the impact of political structure on concurrence is about the same in both high- and low-consensus communities (multiple  $R$ 's of .39 and .42 respectively): participation, however, improves concurrence only when citizens agree. For other local leaders, political factors have a greater impact on concurrence in high- as compared to low-consensus communities (multiple  $R$ 's of .51 and .34 respectively.) This again emphasizes the role which political factors play in communicating citizen views even in communities where citizens share common concerns.

<sup>72</sup> Verba and Nie, pp. 320-327. There they describe the measure of consensus used here, analogous to the concurrence index, but indicating agreement between those ranking in the top third on overall participation and other citizens. Electoral participation shows about the same relationship as that reported here. Non-electoral forms of participation have a negative effect on concurrence in low-consensus communities. But since that relationship was not affected by political structure, only voting has been reported here.



### Concurrence and Causality

The relationships observed in this paper suggest that participation and political structure affect local leaders' responsiveness to citizens. But the relationships that I have observed could also be due to manipulation of participation, political structure, and the community agenda by local leaders. They might be motivated by strong political beliefs, desire for power, quest for economic advantage, or simply a wish to improve their bargaining position vis-à-vis county, state, or national leaders (Mayor Daley's use of the Chicago Democratic machine as a source of national political clout was an example of the latter). Any modern government has vast resources to create issues, justify its actions, and win approval for its policies and leadership. Consider Dahl's use of the term "political entrepreneur" to describe Mayor Lee's salesmanship in convincing New Haven citizens of the benefits of urban renewal.<sup>73</sup> Consider, too, large sums spent by the Army on public relations and on publicizing its assessments of defense needs,<sup>74</sup> or the efforts of Candidate Nixon's campaign committees to rally public support for his issue stands.<sup>75</sup> Citizen-leader agreement on the local political agenda, then, could be due to elite influence; and might be diagrammed as follows:



This is not to attribute wholly Machiavellian motives to local elites. Many hard-working community leaders have a greater knowledge of what projects are possible, and what sorts of support from other levels of government and civic groups might be available, than does the average citizen. They are in a better position to *articulate* vague discontent and desires for local improvements into clearly stated proposals or policies. Given elites' greater command over political resources,

<sup>73</sup> Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961). Motivations and strategies of community leaders are discussed in Edward C. Banfield, *Political Influence* (New York: Free Press of Glencoe, 1960); Norton Long, "The Local Community as an Ecology of Games," *American Journal of Sociology*, 64 (November, 1958), 251-261; and Cobb and Elder, *Participation in American Politics*.

<sup>74</sup> Evidence presented by the controversial 1971 CBS television documentary, "The Selling of the Pentagon."

<sup>75</sup> Joe McGuinness, *The Selling of the President, 1968* (New York: Trident Press, 1969).

it is more than likely that at least part of the time they will be able to affect the choice of issues, and the issue alternatives, facing the community.

As a cynical Missouri newspaper editor in the elite sample remarked, "The average person around here doesn't know what he wants. He has to be told." Few citizens may be interested in community affairs, and participation in local elections is often dismally low. By default, then, local leaders may develop the political agenda: citizens can only ratify or reject it.

Several major difficulties arise, however, when one attempts to sort out the causal patterns at work here. First, additional information on local leaders' efforts to influence political decision making or mobilize public opinion would have to be considered. Some have argued that adequate data on mobilization of bias or non-decision making are impossible to find.<sup>76</sup> I do not take this position, but I would certainly concede that survey research has its limitations in this regard.

The elite interviews did include questions on leaders' attitudes, activities, and organizational activity. If leaders' efforts to communicate with citizens, or their work in local organizations, or their efforts to encourage citizen participation, proved to bear closer relationships with local concurrence levels than did the indicators of citizen

participation, this might constitute evidence that citizen-leader agreement was due to leaders' efforts to influence citizen opinion or activity. But only one of the indicators of leaders' activities that were available for this analysis demonstrated any strong association with concurrence: leaders' attitudes toward citizen interest and participation. Leaders who saw their fellow citizens as interested and involved were more likely to concur with citizen priorities. It is difficult to consider this as evidence of leaders' efforts to manipulate popular opinion since the more competitive the elections in a community, the more likely leaders were to perceive and to support citizen involvement.<sup>77</sup>

<sup>76</sup> Raymond E. Wolfinger has summarized the ongoing debate over the utility and practicability of analyzing nondecisions in "Non-Decisions and the Study of Local Politics," *American Political Science Review*, 65 (December, 1971), 1063-1080.

<sup>77</sup> The relationship between leaders' activities and concurrence was discussed in Verba and Nie, p. 331 ff., and in the author's dissertation, chapter 6.

Many local leaders mentioned their efforts to encourage citizen participation. This could be an indication of desire to reduce "mandate uncertainty;" it could also represent efforts to round up support for elites' issue preferences. Again, I do not have adequate information to draw any firm conclusions about the causal processes involved. However, leaders' efforts to encourage participation were *not* correlated with community participation rates.<sup>78</sup>

A second major problem in causal interpretation is that the relationships I have observed may be due to nonpolitical variables not examined in this paper. Concurrence may be high in communities which for a number of reasons (economic structure, population, rural-urban quality, social integration) may have leaders recruited from a consensual citizen population. These aspects of the community setting might account for the apparent link between political factors and concurrence.

A number of such possibilities (community size, wealth, urbanization, and geographic isolation) were examined, and it was found that many aspects of the community setting do contribute to concurrence. But controlling for these did not appreciably diminish the linkages between political structure or participation and concurrence.<sup>79</sup> The systematic variation attributable to political factors would indicate that these operate independently of the community social or economic structure. The evidence presented here on the differential impact of political structure on concurrence with social class and participant subgroups within a community constitutes further evidence for the importance of political factors,<sup>80</sup> and suggests that they may give lower-status citizens a better chance to influence their leaders.

Third, data on the policy-making process over time would be required for causal analysis. The concurrence score can only compare communities with respect to citizen-leader agreement on priorities: decisions and outcomes should also be compared before we conclude whether leaders influence citizens or vice versa.

<sup>78</sup> See author's dissertation, chapter 8, for further discussion of leaders' attitudes toward participation.

<sup>79</sup> Verba and Nie, pp. 329-330, found that neither wealth nor community isolation "explained" the association between participation and concurrence; controlling for these did not significantly diminish the original relationship. The author's thesis, chapter 9, considered several elements of the community setting (population size and stability, communications, ethnicity) that contributed to concurrence. But these factors also did not diminish the relationships observed between concurrence and political structure.

<sup>80</sup> G. Bingham Powell has developed several models of electoral aggregation which demonstrate the importance of political structure for representation. "Citizen-Elite Linkages in Austrian Communities" (paper presented at the 1974 APSA annual meeting, Chicago, Illinois).

And finally, one important condition for political accountability is the desire for reelection by officeholders. Prewitt has suggested that incumbent eviction is rare and that political ambition may not always govern leaders' actions at the local level;<sup>81</sup> information on these factors would be required before one could conclude that citizen participation and political opposition operate as democratic theory predicts in holding elected community leaders accountable.

But electoral accountability is not the only means of citizen influence over leaders—fortunately so, since important political decisions are often made by nonelected officials, and since many elected officials do not desire reelection or are legally barred from doing so. This paper has explored the influence and information aspects of political structure and participation by using a standard concurrence score to compare two groups of local leaders in a variety of communities. Concurrence scores of elected local officials are significantly higher under conditions favoring citizen input into local politics: high participation, active political parties, partisanship, and competition for local office. But the impact of such factors on agreement between nonelected leaders and citizens indicates that political factors which facilitate communication between leaders and citizens can also increase leaders' responsiveness to citizen concerns.

Such a conclusion may offer grounds for optimism: democratic practices such as participation and competition for office operate as they should to improve linkage between leaders and citizens. But the long-term outlook is less optimistic. Trends toward nonpartisan elections, atrophied political parties, and lack of electoral competition at the local level suggest that conditions for effective citizen participation in American communities may no longer obtain. Despite increased use of "home rule" in some states, local governments remain legally and financially dependent on state or federal governments, and community leaders' ability to meet citizen demands is correspondingly limited. Efforts to encourage participation in such a context may lead to increased citizen frustration rather than more effective control over leaders' actions. Finally, researchers' efforts to measure or model linkage must deal with changing political structures and the complexities of federalism as well as with the actions and expectations of leaders and citizens.

## APPENDIX

### Computation of Concurrence Scores

The citizens and leaders interviewed for this study were asked open-ended questions about

<sup>81</sup> Prewitt, "Political Ambitions."

community needs and problems, with possibilities for multiple responses. The verbatim responses, coded according to a comprehensive master codebook, provided a huge variety of response, but in fact responses by both leaders and citizens tended to cluster around certain problem areas. Therefore, in order to compute the concurrence scores used as an index of citizen-leader agreement, the hundreds of possible answers were divided into twenty-five categories. These represent the set union of the problems mentioned most frequently by citizens and leaders across the sets of "needs and problems" questions mentioned in the text. The categories are:

- 1. Jobs, unemployment
- 2. Inflation
- 3. Lack of income, poverty
- 4. Housing
- 5. Subsistence needs (food, clothing, welfare)
- 6. Education
- 7. Taxes too high
- 8. Road construction, maintenance
- 9. Sewerage, waste disposal
- 10. Power and water supply
- 11. Recreational facilities
- 12. Money for community needs
- 13. Community services (general category covering police and fire protection, zoning, planning, administration)
- 14. Health, medical facilities
- 15. Problems of the aged
- 16. Family problems: marriage, children
- 17. Crime (violence against persons or property), law and order
- 18. Moral issues (drugs, alcohol, pornography)
- 19. Noise and nuisance
- 20. War in Viet Nam
- 21. The draft
- 22. Political participation, community involvement
- 23. Politics and government: structure, personnel, administration
- 24. School integration
- 25. Racial problems (other than school integration)

These categories covered more than over ninety per cent of all responses, and solved several problems in analysis. Respondents in a number of instances mentioned a broad category of problems (such as education) rather than specific ones (higher teacher salaries, more classrooms, sex education, etc.). This could be because they felt the whole area they mentioned was worthy of concern; it could also mean (more likely the case for cross-section than elite respondents) that they were not well informed or articulate enough to specify the problem area more precisely. Problems also arose in fitting more detailed responses into discrete coding categories. The final coding procedure made it possible to look at issue areas of general concern to both groups.

The number of mentions of a particular problem category by community citizens was counted. Then for each problem a local leader mentioned

(as a community problem, focus of his activity, or perceived citizen concern), he was given a score equal to the number of citizens who mentioned that problem in his community. If a leader mentioned more than one problem in a given question set, his score was for the problem which the greatest number of citizens mentioned (rather than counting both responses, which would have inflated the concurrence scores for leaders who happened to mention more than one problem). This score was divided by the total number of citizen respondents in that community, as shown below. The scores could range from 0 (no leader agreed with citizens) to 100 (all leaders and citizens in a community agreed).

Example of Computation of Concurrence Score

Problem	X No. of Mentions by Community Citizens	Y Mentioned by Leader (0 = No, 1 = Yes)	X × Y
1	5	0	0
2	3	1	3
3	0	1	0
4	1	0	0
5	2	0	0
			3

N = Number of citizens in community = 15  
Concurrence =  $(X \times Y) / N = 3 / 15 = .20$   
Scores were multiplied by 100 for table presentation

Some citizens made no codable responses to the open-ended problem questions. They either said they had no problems or made a very general statement that "I have many personal problems" or "things could be better." Such people were included in the compilation of the concurrence scores (as a divisor, that is, the total sampled in a community) so that elite concurrence scores would reflect concurrence with the whole population, not just with that articulate majority who made codable responses. Local leaders who mentioned no problems (or gave responses that could not be coded) were given a concurrence score of zero.

The purpose of this paper was to compare communities and to look at some ways in which community political structure and participation affected agreement between local leaders and citizens. For this reason, concurrence scores were averaged for all of the leaders interviewed in a given community. Thus if two or three of the leaders and several of the cross-section respondents mentioned the same problem, a community would have a higher "concurrence score" than a community where only one leader recognized a problem cited by several citizens.

# Courts and Conflict Resolution: Problems in the Mobilization of Adjudication\*

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If, as Edward Banfield and James Wilson suggest, politics "... arises out of conflict and ... consists of the activities by which conflict is carried on,"<sup>1</sup> then political science should be concerned with the way conflicts are defined, interpreted for public consumption and managed in society. Traditionally, political scientists have concentrated on describing and analyzing the manner in which political institutions react to conflicts.<sup>2</sup> More importantly, when they have tried to measure and assess the role which those institutions play in social processes of conflict and conflict management, political scientists have generally operated in a limited comparative context. They have either compared different political institutions within a single society or similar political institutions across several societies. In either case, their method of comparison has been largely horizontal.

Two generic types of conflict-resolving tendencies in political institutions have been identified. The first, with which political scientists are most familiar, is *nonadjudicative*. Institutions which are primarily nonadjudicative are concerned with the formulative and prescriptive aspects of law-making. Conflict resolution in nonadjudicative institutions, such as legislatures, tends to be universalistic—more concerned with general rules or standards for future application than with the equities between two or more parties currently in dispute. Additionally, nonadjudicative institutions deal with the needs of broad and unnamed classes

or groups rather than of individuals, with "issue and symbols" rather than specific pronouncements, and with general, often imprecise, claims of deprivation or need. They are concerned with individual problems only insofar as these are symptomatic of social problems which are visible, intense and broad in scope.

Institutions which are primarily *adjudicative* deal with conflicts through third party intervention. Often the parties in dispute participate in its resolution through presentation of reasoned arguments. Adjudicative institutions such as courts are particularistic in form and process, and most often concerned with individual level disputes. The impact of adjudicative decisions initially extends only to the parties in dispute, although it may also have much broader policy implications. Theoretically, adjudicative institutions are more concerned with enforcing existing norms than with creating new ones.<sup>3</sup> Furthermore, they are almost totally "reactive."<sup>4</sup> Unlike nonadjudicative bodies, which also search out or try to anticipate problems before they arise, adjudicative institutions rely on private individuals or groups or other government agencies to bring problems to their attention. They rely on the initiative of private parties to set the agenda of issues on which public officials act. They can take no part in defining, interpreting, and managing conflict until they are "mobilized."<sup>5</sup> As a result, adjudicative institutions are not well suited to resolving problems that private parties do not or cannot perceive, or wish to ignore.

The distinction between adjudicative and nonadjudicative types of institutions is imperfect, since institutions identified primarily as one or the other may display a mix of adjudicative and non-

\* A prior version of this paper was delivered at the IX World Congress of the International Political Science Association, August, 1973, Montreal, Canada. The authors wish to acknowledge the helpful comments of a number of readers including Hadley Arkes, Lawrence Baum, Gary Bunch, Mary Grossman, J. Willard Hurst, Malcolm Feeley, J. Woodford Howard, Beth Sarat, Ellen Siegelman, William Taubman, Kevin Upton and Kent Weeks. The assistance of Edith Wilimovsky and James Robert Kirk in preparing the manuscript is also gratefully acknowledged.

<sup>1</sup> Edward Banfield and James Q. Wilson, *City Politics* (New York: Vintage, 1963), p. 7.

<sup>2</sup> This kind of concern is reflected in most studies of the decision-making processes of political institutions. For one of the best of these studies see Raymond Bauer et al., *American Business and Public Policy* (New York: Atherton, 1963).

<sup>3</sup> See Herbert Jacob, *Justice in America*, 2nd ed. (Boston: Little, Brown, 1972).

<sup>4</sup> This term is used by Donald Black to describe one aspect of the legal process, specifically the dependence of some institutions on citizen initiated complaints. See "The Mobilization of Law," *Journal of Legal Studies*, 2 (January, 1973), 128.

<sup>5</sup> Mobilization refers to the process through which adjudicative institutions become involved in the definition, interpretation and management of conflict. Litigation refers exclusively to the mobilization of courts.

adjudicative functions.<sup>6</sup> Political scientists have recognized this problem and have examined the similarities and differences among adjudicative, nonadjudicative and mixed types of governmental institutions.<sup>7</sup> What has not been done, however, is to examine the similarities and differences among governmental institutions and nongovernmental institutions which, in different ways, perform similar—even symbiotic—conflict resolving functions. There is an impressive body of data on the operation of political institutions in many different countries; but we have yet to develop an approach which would emphasize both public and private processes of defining, interpreting, and managing conflict and the “vertical” relationships which exist between them.

Our concern in this paper is with the role of the courts in the process of conflict resolution.<sup>8</sup> In trying to assess that role, we compare courts with other public and private adjudicative institutions which traditionally have not been of much concern to political scientists. The discipline has shown considerable interest in the role of courts in solving interest-group conflict, and in the reciprocal role of litigious interest groups in structuring some of the major political issues of our time. But the emphasis on a few litigious groups has fostered two misimpressions—first that the major political function of courts is to arbitrate group conflict; and, second that most litigation of the “individual” variety is inherently nonpolitical. These individualized disputes were, if recognized at all for their political content and impact, relegated to the dark side of the public law—private law dichotomy. As Shapiro<sup>9</sup> and others have noted, this artificial exclusion has fostered a number of inaccuracies in our understanding both of how adjudicative institutions work and of the extent of their political role.

In this paper, we explore the management of conflict by a variety of adjudicative institutions, both public and private. We look for a common thread of dispute-solving techniques in institu-

tions with different legal statuses and formal structures, varying degrees of visibility and extending over a wide range of substantive areas. Our comparison of courts to other types of adjudicative bodies is designed to show both similarities and differences of task, and to underscore the interrelated nature of their functions. Additionally, since all types of adjudicative institutions are dependent ultimately on the initiative of outside parties, we examine the conditions which facilitate or inhibit their involvement in defining, interpreting and managing conflict. Our intention is to highlight the consequences of judicial resolution of some disputes, but not others, by describing the kind of institutions which deal with these “other” disputes, how such disputes are settled, and why people choose (or are required to use) particular adjudicative mechanisms when the need for dispute settlement arises.

### Courts and Other Adjudicators

The very attributes which distinguish courts from other political institutions—their particularized focus and their reactive nature—are most useful in connecting judicial functions and those of nongovernmental dispute-resolving institutions. Yet adjudicative bodies also differ among themselves. Their differences, as we shall suggest, may be especially important in determining the conditions under which courts and other adjudicators are mobilized and involved in conflict and conflict resolution.<sup>10</sup>

With any particular dispute settlement institution comes a special set of participants, perhaps a different set of rules, and a distinctive style. Each alternative may provide its own kind of “justice”; some proceed without reference to general rules which are known and articulated in advance; others operate according to such rules. Some provide a process of judgment in which it is the status of the parties in dispute, or the preferences of the judge, which determine the outcome; others require impartiality. Courts, for example, generally emphasize greater procedural regularity than do other adjudicative institutions. Furthermore, public adjudicative decision making is, in theory, supposed to encompass the marshaling of reasons and justifications by an impartial arbiter.<sup>11</sup> Yet reasoned decisions and equitable results are not the uniform product of public dispute settling, nor are they uniquely found in the public arena.

<sup>6</sup> A particularly interesting description of the mix of adjudicative and nonadjudicative functions in a single political institution is provided by Philippe Nonet, *Administrative Justice* (New York: Russell Sage Foundation, 1969).

<sup>7</sup> For a comparison of courts, legislatures and executives see Kenneth Dolbeare, *Trial Courts in Urban Politics* (New York: Wiley, 1967), p. 117.

<sup>8</sup> We realize that courts do more than just settle disputes or resolve conflict. As Friedman argues, the most time-consuming activity of trial courts may involve record keeping and other administrative functions. See Lawrence Friedman, “Functions of Trial Courts in the Modern World” (presented at a Conference on Sociology of Judicial Process, University of Bielefeld, German Federal Republic, 1973).

<sup>9</sup> See Martin Shapiro, “From Public Law to Public Policy, or the ‘Public’ in Public Law,” *PS*, 5 (Fall, 1972), 410–418.

<sup>10</sup> The similarities and differences among courts and other dispute-settling institutions have recently been the subject of an extensive treatment by Richard Abel, “A Comparative Theory of Dispute Institutions in Society,” *Law and Society Review*, 8 (Winter, 1974), 217–347.

<sup>11</sup> For a discussion of these distinctions see Lon Fuller, “The Forms and Limits of Adjudication” (unpublished essay, n.d.).

The structure of a dispute-resolving institution may have an important influence on the way the dispute is presented; indeed it may affect the basic nature of the dispute itself. This is as true in moving from informal to more formal means of conflict resolution as it is recognized widely to be true in moving from the trial to appellate stages of litigation.<sup>12</sup> Finally, any particular institution may also carry with it some limitations on the available remedies, and thus indirectly affect the nature of the settlement which is possible. Participants have to agree to play by the rules of the institution; often this requires that they redefine their interests, goals and strategies.<sup>13</sup>

Variables which have been used to distinguish among adjudicative institutions include the level of coerciveness, the scope and severity of available sanctions, the applicable norms, and the extent of third party intervention.<sup>14</sup> We have chosen two other variables which appear to differentiate particularly well among the most common adjudicative alternatives. These variables are the level of formality in the procedures used and the degree of "publicness." The level of formality is defined by the presence of a specialized judicial role, specialized rules of evidence and procedure, written records, established channels of appeal and the swearing in of witnesses.<sup>15</sup> An institution is considered public if it is part of the established government apparatus and if the coercive power of the state can be used to enforce its decisions.

<sup>12</sup> Richard Richardson and Kenneth Vines, *The Politics of the Federal Courts* (Boston: Little, Brown, 1970), and J. Woodford Howard, "Litigation Flow in the U.S. Courts of Appeals for the D.C., 2nd and 5th Circuits" (paper delivered at the 1971 annual meeting of the American Political Science Association).

<sup>13</sup> P. H. Gulliver, "Negotiations as a Mode of Dispute Settlement: Towards a General Model," *Law and Society Review*, 7 (Summer, 1973), 667-692.

<sup>14</sup> *Ibid.*

<sup>15</sup> William L. F. Felstiner, "Influences of Social Organization on Dispute Processing," *Law and Society Review*, 9 (Fall, 1974), 63-94.

The four-cell table presented below depicts, somewhat artificially, four "ideal types" of adjudicative dispute-settling alternatives; each represents a combination of our key variables, hence the cell labels: Private-Formal, Private-Informal, Public-Formal, and Public-Informal. The categories are not pure types, merely regions sliced from a graph of two continuous variables. We are looking for the predominant characteristics of each type of adjudicative alternative while recognizing the impurities in the forms which we have assigned to each. Below we elaborate on these characteristics and we illustrate the way differences among adjudicative institutions may help account for differences in the way they are mobilized and involved in conflict and conflict management.

**Private-Informal Dispute Settlement.** The simplest form of private-informal dispute settlement occurs in the interaction between two individuals or groups.<sup>16</sup> Conflicts generally arise in these dyadic settings, and the first attempts at resolution also generally occur at this level,<sup>17</sup> usually in the form of direct negotiation and two-party bargaining. Only if such efforts prove unsuccessful, or if the expectation of failure in this context is so great that no attempt at direct negotiation is made, does

<sup>16</sup> We recognize that many "potential" disputes are either suppressed by the individual feeling aggrieved or are displaced onto others not directly involved. This self-suppression or displacement is often practiced by people who fear being labeled as troublemakers. It is exemplified when people complain about family life while at work and about work life while at home but never make their feelings known in the appropriate setting.

<sup>17</sup> A study of consumer disputes in Philadelphia reports that most consumers first try to settle disputes with businessmen by negotiating directly with the retailer. Only when and if that fails do they seek other remedies. See John Steadman and Richard Rosenstein, "Small Claims Consumer Plaintiffs in the Philadelphia Municipal Court," *University of Pennsylvania Law Review*, 121 (June, 1973), 1331.

Table 1. A Typology of Adjudicative Dispute Resolution

	Formal	Informal
Public	Court trials/some types of administrative agency procedures, e.g., wage-price boards	Police in family quarrels/plea bargaining/administrative agency inquiries (hearing examiners) and consent agreements, such as the resolution of most tax disputes and anti-trust actions/pre-trial conferences/parole boards/state civil rights commissions/juvenile courts pre-Gault/police review boards
Private	College disciplinary boards/rabbinical or other ecclesiastical courts/arbitration of labor disputes/some bar association grievance committees	Settlement of automobile accident claims/marriage counseling/debt adjustment/reconciliation by family, community, or religious leader

third party resolution within an adjudicative context become relevant. When a third party is called into the dispute he may act as a "judge," or mediator, but may not necessarily be bound by any formal, codified rules.<sup>18</sup>

Of course there are rules which he must follow, but they will not predominate; at most they may provide a context of expectations in which private, informal settlements may occur. The adjusters of disputes in a private, informal setting will not have any formal connection with the state, although in some cases they may have to be licensed by the state or by a quasi-private organization of professionals which exercises a delegated licensing function. The third-party mediator may be chosen because he has status, position, respect, power, money or the alleged power to invoke sanctions in behalf of a deity or some other supra-human force. Or he may have none of these but simply be the designated agent of an organization set up to handle specific disputes. The technique of bringing disputes to a third party may be the choice of both disputants, or of one but not the other party to a conflict, or it may be the result of private norms or expectations of a subcultural group which "require" that disputes be settled, as much as possible, within the group.<sup>19</sup>

Private-informal dispute resolution is found in all societies. In the United States, it is found in religious and ethnic communities,<sup>20</sup> in the work of trained professionals such as marriage counselors,<sup>21</sup> in various commercial relationships,<sup>22</sup> and even in supermarkets.<sup>23</sup> In all of these settings, private-informal adjudication is based on the assumption that the parties to a dispute will agree about what matters are in dispute and will trust the person called upon to act in an adjudicative role.

<sup>18</sup> See Torstein Eckhoff, "The Mediator and the Judge," in *Sociology of Law*, ed. Vilhelm Aubert (Baltimore: Penguin, 1969).

<sup>19</sup> For a description of this phenomenon as it applies to organized crime in the United States, see Donald R. Cressey, *Theft of a Nation: The Structure and Operations of Organized Crime in America* (New York: Harper & Row, 1969), chapters 8 and 9.

<sup>20</sup> "Roman Catholic Ecclesiastical Courts and the Law of Marriage," *Columbia Journal of Law and Social Problems*, 7 (Spring, 1971), 230.

<sup>21</sup> Robert Coulson, "Family Arbitration—An Exercise in Sensitivity," *Family Law Quarterly*, 3 (March, 1969), 22.

<sup>22</sup> Mary Cameron, *The Booster and the Snitch* (New York: Free Press, 1964), p. 121. See also Stewart Macaulay, *Law and the Balance of Power* (New York: Russell Sage Foundation, 1966), p. 153, and "Non-Contractual Relations in Business: A Preliminary Study," *American Sociological Review*, 28 (February, 1963), 55–67.

<sup>23</sup> Spencer MacCallum, "Dispute Resolution in an American Supermarket," in *Law and Warfare*, ed. Paul Bohannon (Garden City, New York: Natural History Press, 1967), pp. 293–294.

Societies differ in the extent to which private, informal modes of dispute settlement predominate, and in the extent to which the norms of private settlement affect the operations of more public and formal institutions. Japan, for instance, seems marked by the ubiquity of such private-informal conflict resolving devices. Henderson reports that private adjudication in Japan has been and still is effective "... in settling the vast majority of disputes."<sup>24</sup> The third party is generally a man of higher social status than the disputants, and the role of the adjudicator or mediator is enhanced by the persistence of remnants of traditional social deference. It is promoted by a strong cultural preference for harmony and saving face over claiming one's "rights." In Japan, in contrast to the United States, law and right are entirely different concepts and not just two sides of the same coin.<sup>25</sup> Similarly, Hahm notes the strong cultural preference for private-informal settlement in Korea. The goal of this kind of dispute settlement in Korea is the restoration of normal relations between the disputants as fast as possible. As a result, "... the central concern is to discover what is proper and decent, not so much what is legal. ... A good, and therefore a right, solution is what the parties can agree on and approve."<sup>26</sup>

Private-informal dispute settlement has also been found to be relatively most extensive in primitive or underdeveloped societies. In such societies, dispute settlement most often occurs within primary groups or within a village setting.<sup>27</sup> A respected elder intervenes and after listening to the disputants makes a decision. His decision will be accepted by the disputants out of respect for his position or because of their allegiance to a close knit social community.<sup>28</sup>

In both underdeveloped and developed soci-

<sup>24</sup> Don Henderson, "Law and Political Modernization in Japan," in *Political Development in Modern Japan*, ed. Robert Ward (Princeton: Princeton University Press, 1969), p. 449.

<sup>25</sup> See T. Kawashima, "Dispute Resolution in Contemporary Japan," in *Law in Japan*, ed. Arthur Von Mehren (Cambridge: Harvard University Press, 1963); and Henderson, p. 448.

<sup>26</sup> Pyong-Choon Hahm, "The Decision-Process in Korea," in *Comparative Judicial Behavior*, ed. Glendon Schubert and David Danelski (New York: Oxford University Press, 1969), p. 36.

<sup>27</sup> Leopold Pospisil, *Kapauka Papauns and Their Law* (New Haven: Yale University Publications in Anthropology, 1958), pp. 254–255 and *passim*.

<sup>28</sup> The importance of community allegiance and group norms in facilitating acceptance of judgments, a common observation throughout this literature, seems analogous to the findings of political scientists about the importance of community and peer-group pressures in determining individual attitudes toward acceptance of Supreme Court decisions specifically and toward legal compliance generally.

eties, private-informal processes may be favored over litigation because of their "proximity" to the disputants. This proximity facilitates dispute settlement by allowing people to work out their problems without clearly having to acknowledge that their relationship is "in trouble." This is especially attractive to people who are reluctant to declare trouble because they fear being labeled troublemakers or fear being held responsible for not preventing the trouble.<sup>29</sup> Members of distinct subcultural groups may feel more comfortable being judged by "their own kind" or at least by someone perceived as close to their own lifestyle and values. They may be reluctant to participate in formal processes of adjudication because they lack faith in the fairness and justice of results which might be obtained from these processes.<sup>30</sup> This lack of faith is accentuated where public dispute settlers (including lawyers) are predominantly of a different race, religion, ethnic group or social status from that of the disputants.

**Public-Informal Dispute Settlement.** Adjudication of disputes also proceeds through public-informal methods. These methods involve the informal intervention by agents of the state to solve disputes. Such dispute resolution is utilized to complement the needs of public, formal institutions by reducing their operating burdens through the delegation of discretion to actors whose behavior in working out disputes is less often guided by standardized norms or procedures than by role relevant routines. Likewise, dispute settlement in this context reflects not only official norms of substance and procedure but also, within broad limits, the organizational commitments of the public agents involved. Thus, the needs of the police to maintain order have been shown often

<sup>29</sup> In our discussion we take the presence of trouble or conflict as a starting point. We realize, however, that the willingness of people to engage in conflict or acknowledge trouble is itself a variable. Where people are reluctant to declare trouble, they may continue in a relationship in which one or both members perceive that something is wrong without communicating this perception to the other party or to any third party. They may, however, allow the tension built up in such situations to be released in other relationships.

<sup>30</sup> See David H. Bayley and Harold Mendelsohn, *Minorities and the Police* (New York: Free Press, 1968), and Charles E. Reardon and Jack L. Kuykendall, eds., *Race, Crime and Justice* (Pacific Palisades, California: Goodyear, 1972). See also Kate Claghorn, *The Immigrants' Day in Court* (New York: Arno Press, 1969), p. 147. Suspicion of the motives or values of public dispute settlers, so characteristic of ghetto dwellers today, was a widespread phenomenon among immigrant groups in the early part of the 20th century. In addition to avoiding contact with feared outsiders, people may feel more comfortable being judged by "their own kind," by someone perceived as closer to their own life style and values.

to be incompatible with the norms of enforcing the law, and both are not necessarily consistent with due process rights of arrested persons and rules defining proper police behavior.<sup>31</sup> Wilson argues that when a choice must be made, most policemen will choose the order maintenance orientation, which gives them "substantial discretion over matters of greatest importance in a situation that is, by definition, one of conflict."<sup>32</sup> This choice is perhaps best illustrated when police are called upon to act as resolvers of marital disputes. More often than not the officer acts to "cool things off" rather than to make an arrest. He does not see it as his primary duty to determine who is right and who is wrong; indeed to do so might only exacerbate the disturbance he is trying to quell. Rather his main concern is to restore calm and find a way "of mollifying everyone." He can act this way because his presence carries with it the authority of state power and the threat of arrest, although he does not follow the procedural regularities that would be observed in a more formal setting.<sup>33</sup>

Avoidance of procedural regularity in the pursuit of more immediate and more efficient conflict resolution is also characteristic of plea bargaining, of what used to be the norm of informality in juvenile courts before the United States Supreme Court's *Gault* decision,<sup>34</sup> and in the pretrial conference which has come to play an increasing role in facilitating the settlement of civil suits in the United States. In the case of plea bargaining and juvenile justice, the defendant is induced to settle his "conflict with the law" through an informal, bargaining process. Thus plea bargaining may be less like other types of adjudication, in which the roles of "judge" and adversary are clearly separated, than the kind of dyadic negotiations which often precede resort to adjudication. Plea bargaining is best seen in the context of exchange theory.<sup>35</sup> The defendant concedes his guilt and accepts some predictable punishment in exchange for reduction in the number and seriousness of charges against him or the promise to recommend a light sentence to the judge. He also avoids the predictably harsher sentence he would receive if convicted by

<sup>31</sup> Jerome Skolnick, *Justice Without Trial* (New York: Wiley, 1966), pp. 6-9.

<sup>32</sup> James Q. Wilson, *Varieties of Police Behavior* (Cambridge: Harvard University Press, 1968), p. 21.

<sup>33</sup> *Ibid.*, chapter 4 and *passim*. Wilson notes the variety of factors which enter into a policeman's discretionary decision as to how and whether to enforce the law to the letter.

<sup>34</sup> *In Re Gault*, 387 U.S. 1 (1967).

<sup>35</sup> George F. Cole, "The Decision to Prosecute," *Law and Society Review*, 4 (February, 1970), 313-343. Also see Lief H. Carter, "The Limits of Order: Uncertainty and Adaptation in a District Attorney's Office" (Ph.D. dissertation, University of California, Berkeley, 1972).



jury. The public prosecutor becomes the "discrete settler" and "has virtually complete discretion" as to whether to prosecute or on what charge. He has available both the coercive power of the state (in the form of a probable guilty verdict if the case goes to trial) and the discretion to work out an acceptable compromise. Mediating toward a compromise is also in the prosecutor's interest. It reduces the political risk of (too many) acquittals, it enables him to keep up with his caseload, and it gives him better control of the outcome. His role becomes managerial and bureaucratized. In place of the formality of court adjudication of guilt or innocence, the prosecutor becomes the manager of a ritualized conflict resolution ceremony.<sup>36</sup> While in some ways his handling of guilty pleas resembles the resolution of private-informal disputes, it is much less open-ended. The prosecutor is a public official designated to resolve such conflicts and the defendant cannot imply opt out of the exchange relationship.

The emerging institution of the pretrial conference performs essentially the same function for civil cases.<sup>37</sup> If the parties are unable to resolve their differences privately, the judge, in chambers, will informally discuss the case with both sides in the hope that a compromise will emerge. The judge cannot announce in advance of trial how he would decide the case. But, based on his experience, he can suggest possible outcomes which would ensue if the case went to a jury or suggest the limits of damages "normally" awarded in similar types of cases. The judge has no power to compel an agreement, but "helping" the parties to do so is certainly in his interest. The pretrial conference is considerably less ritualized and the role of the judge as mediator less managerial than that of the prosecutor in plea bargaining. But it serves a comparable function in allowing the judge to keep up with his caseload while reducing the costs and risks to the disputants. This is not just an American phenomenon. Japanese courts are noted for their emphasis on pretrial influences leading to settlement without invoking the formal procedures or coercive power of the state.<sup>38</sup>

The common thread connecting all public-informal methods of dispute resolution is their proximity to the formal arena of the courts and to third-party adjudication by a judge or other public official, as well as their reliance on private bargaining within this context. There are no formal "rules," but a set of norms and mutual expectations gives these processes some structure. Indeed, in some cases, of which plea bargaining is the best example, the process by which informal settlements are reached has itself become bureaucratized. A relatively small group of actors is involved in any one jurisdiction, regularized relationships and mutual dependencies develop, and the needs of the bureaucracy for compromise and accommodation frequently predominate.<sup>39</sup>

In both plea bargaining and pretrial conferences, the process of mobilizing the judicial process and of litigating a dispute has already begun. Unlike private-informal alternatives, which act to deflect disputes from formal processes before they are carried into litigation, these examples, although not all, of public-informal processes act to deflect disputes from formal processes only after the process of mobilization has been initiated. In a sense, these devices do not act as alternatives to such formal adjudication; they function instead to reduce it from a highly conflictual process to one in which all that is sought is certification of an informally agreed upon solution.

**Private-Formal Dispute Settlement.** In this category the dispute settlers remain private actors but they carry out their functions in accord with certain agreed upon and standardized procedures. They are more likely to act as "judges" than mediators, although role definitions may be fluid. Such formal but private tribunals are found within the confines of private organizations or associations, professional groups, as well as within certain subcultural groups. These tribunals exist to settle conflicts between group members, conflicts which cannot be settled informally but which, for some reason, the parties are reluctant to move into the public arena. Where group mem-

<sup>36</sup> It is now commonplace to describe the reality of the criminal justice process in terms of a "crime-control," "bureaucratic," or "dispositional" model which diverges from the ideal "due process" or "adversarial" model. See Abraham Blumberg, *Criminal Justice* (Chicago: Quadrangle, 1967); Herbert L. Packer, *The Limits of the Criminal Sanction* (Stanford: Stanford University Press, 1968), part II; and Herbert Jacob, *Urban Justice* (Englewood Cliffs, New Jersey: Prentice-Hall, 1973), chapter 4; and "Criminal Courts as Organizational Phenomena" (unpublished paper, 1973).

<sup>37</sup> Maurice Rosenberg, *The Pretrial Conference and Effective Justice* (New York: Columbia University Press, 1964).

<sup>38</sup> Kawashima, p. 43.

<sup>39</sup> Similar devices have evolved in non-American settings. Cohen, in his study of criminal justice in China, detected an emphasis among public officials on informal settlement. He observed that almost immediately upon capturing the major cities of China in 1949, the Communists established out-of-court mediation committees there. Party agents were designated with special responsibility to seek, with the agreement of both parties, resolution of ordinary civil disputes and minor criminal problems. Local norms and values were applied in an effort to keep cases out of formal court. "Coping with disputes and anti-social conduct by means of persuasion and informal pressures" was the prevailing orientation. Jerome Cohen, *The Criminal Process in the People's Republic of China* (Cambridge: Harvard University Press, 1968), p. 123.

bership is conditioned by a voluntary or imposed acceptance of certain norms, or a code of conduct, the enforcement of such norms will typically be handled at several levels within the group. Those breaches which cannot be settled or sanctioned informally will usually have to be settled by more formal means. Although the rules and procedures followed by such tribunals may bear some resemblance to those of the courts, those rules and procedures remain private and often diverge substantially from those employed in the public sector.

Many trade associations have established their own formal arbitration machinery for the settlement of disputes among members.<sup>40</sup> Similarly, labor-management disputes are often brought before arbiters, designated in advance, whose decisions are binding by mutual consent of the disputants and ultimately enforceable by private sanctions and by the courts.<sup>41</sup> Agreement to submit disputes to private but formal arbitration is characteristic of parties whose relationship involves long-term performance or other aspects of permanence.

Private-formal tribunals have also emerged within distinctive subcultural groups. On Indian reservations tribal courts operate independently of state judicial systems. They "are established and administered by the tribes themselves and enforce tribal codes," are staffed with lay judges, and deal with civil disputes and tribal code violations. Their operation is private in that they have no direct means of coercing compliance, relying instead on the "normative consensus" of the subgroup to psychologically induce compliance.<sup>42</sup> A similar arrangement can be found in some tightly knit ethnic<sup>43</sup> or religious communities.<sup>44</sup>

Private-formal dispute-settling devices thus share some similarities but also vary widely. What they have in common is a court-like procedure for settling disputes among group members (even the complaint of an "outsider" about the conduct of group member, e.g., a legal or medical malpractice accusation, is considered as the occasion for an inquiry rather than as a strict adversary

proceeding. Thus it becomes a dispute between group and one of its members). Decision makers are not agents of the state, but may be regarded by group members as possessing some sort of jural authority based on the stipulations of a prior contract or flowing from delegated state authority. Compliance may be purely voluntary, as in the case of a religious court, or induced by potential sanctions (e.g., loss of license, loss of hospital privileges for a doctor, etc.) or the ultimate threat of a spillover into the public courts with greater potential consequences. It should be noted that in the United States at least, the development of the industrial state has increased the scope of "private government" in a dramatic fashion. With this increase in scope has come increased concern and pressure to make the decisions of private government bodies conform to democratic norms and to emerging public standards of due process and legality.<sup>45</sup> Increasingly there seem to be a convergence between the norms and procedures of private tribunals and their public counterparts.<sup>46</sup>

**Public-Formal Dispute Settlement.** The final arena of adjudicative conflict resolution is both public and formal, and is best exemplified by courts. Although there is a wide gap between the highest appellate courts and the now nearly extinct justice of the peace courts, certain characteristics are shared. All are agencies of the state and vested with its coercive power. Most judicial personnel are trained in the law. Though individual discretion is a significant factor in the operation of courts, on the whole they are expected to operate congruently with procedural norms such as those described by Lon Fuller as the "inner morality" of the law.<sup>47</sup> Nonjudicial public bodies are frequently held to comparable, if not identical, standards in their adjudicative functions.

Whereas the optimal goal of dispute resolution at a simpler level is the mutual satisfaction of the parties, at this level the emphasis falls more heavily on rights and duties. Moreover the institutional self-interest of adjudicative bodies which are public and formal plays some role in the outcome of disputes they help to settle. A concern to

<sup>40</sup> Soja Mentschikoff, "Commercial Arbitration," *Columbia Law Review*, 61 (May, 1961), 849. Also see Note 71 below.

<sup>41</sup> Morris Handshaker, "Grievance Arbitration and Mediated Settlements," *Labor Law Journal*, 17 (October, 1966), 580.

<sup>42</sup> James Kerr, "Constitutional Rights, Tribal Justice and the American Indian," *Journal of Public Law*, 18, No. 2 (1969), 322.

<sup>43</sup> See Leigh-Wai Doo, "Dispute Settlement in Chinese-American Communities," *American Journal of Comparative Law*, 21 (Fall, 1973), 627-663.

<sup>44</sup> "Roman Catholic Ecclesiastical Courts and the Law of Marriage"; see also "Rabbinical Courts," *Columbia Journal of Law and Social Problems*, 6 (January, 1970), 57.

<sup>45</sup> See Arthur S. Miller, "Private Governments and the Constitution" (Santa Barbara, California: Center for the Study of Democratic Institutions, 1959), and Peter Bachrach, *The Theory of Democratic Elitism* (Boston: Little, Brown, 1967), chapter 7 and *passim*.

<sup>46</sup> See Charles W. Anderson, "Public Policy, Pluralism, and the Further Evolution of Advanced Industrial Society" (paper delivered at the 1973 annual meeting of the American Political Science Association, New Orleans).

<sup>47</sup> Lon Fuller, *The Morality of Law* (New Haven: Yale University Press, 1964), chapter 2.

o justice to the parties while maintaining institutional power and prestige often requires preference for one or the other. This potential internal conflict is but one expression of the traditional ambivalence of Americans toward law and the judicial system.<sup>48</sup> The conflicting demands we make of our courts also require them to balance considerations of state with considerations of private justice. Though technically they must be particularistic, in fact they cannot function legitimately without paying some attention to a broader universe of concerns. This is particularly true as one moves up the judicial ladder, where the specific grievances of the original disputants are rarely the sole focus of attention.

The rules which govern access and establish the procedural framework of adjudicative bodies are variables of critical importance. What kinds of disputes are to be decided, who can bring these disputes, and what kinds of solutions are possible are among the most important determinants of the involvement of public-formal adjudicators in defining, managing, and interpreting conflict. In contrast to private, informal mechanisms, the "rules" for decision in courts do not come from the parties themselves. Their sources are many—statutes, prior decisions governed by the rule of precedent, and evolving policy considerations responsive to current demands. Recent relaxation of the rules of standing and class actions, and those governing habeas corpus challenges to criminal convictions, have opened new channels of access. Changes in the substantive law also promote increased use of the courts as interests seek to take advantage of newly favorable rules.<sup>49</sup>

Unlike other adjudicative institutions, those that are both public and formal generally require, either explicitly or implicitly, that parties in dispute be represented by legal specialists, people who claim unique knowledge of the procedural and substantive rules which govern access to and the operation of public-formal adjudicative insti-

tutions.<sup>50</sup> These specialists act on behalf of the parties to try to shape and structure the issues presented to these institutions and the way the issues are perceived and handled by them. Legal specialists play a critical "gatekeeping" role. They can influence and often determine the conditions under which courts and similar types of adjudicative institutions become involved in conflict management;<sup>51</sup> and they will play a crucial role in defining the goals and objectives of litigation.

Public-formal dispute mechanisms are usually more oriented toward zero-sum decisions than are the less formal mechanisms we have noted.<sup>52</sup> Additionally, they generally require disputants to narrow their definitions of issues in such a way as to identify unambiguously, if sometimes artificially, the nature of their problems. One example is in divorce cases, where the courts may focus on one incident in what is a complex and often not very clear-cut series of problems. In spite of the zero-sum nature and formality of many of their procedures, public-formal adjudicators occasionally do seek compromise and flexibility. Since they are substantively more concerned with right or wrong, and with enforcing general norms, than with the resumption of "normal" relations between the parties, the processes through which this flexibility is introduced may be quite unique. A number of means have been devised for reconciling new experiences and expectations with past values, while at the same time preserving at least the illusion that the law is consistent over time. The development of equity is one well-known technique of providing "justice" to the parties where a strict application of the law would be unjust.<sup>53</sup> Furthermore, the formal adjudication of disputes is, at least in some countries, an essential

<sup>48</sup> See Daniel Boorstin, "The Perils of Indwelling Law," in *The Rule of Law*, ed. Robert Paul Wolff (New York: Simon and Schuster, 1971), pp. 75-97.

<sup>49</sup> See *A Research Plan for a Caseload Forecasting Study: Final Report* (Washington, D.C.: Federal Judicial Center, 1972), p. 24. The influence of legal changes on litigation can be seen in reference to the growth of prisoner petitions in the federal courts. Recent Supreme Court decisions, for instance *Fay v. Noia* (1963), *Miranda v. Arizona* (1966) and *Gideon v. Wainwright* (1963), have gone far to stimulate the extraordinary growth in the number of prisoner petitions filed since 1960. In that year approximately 2,000 petitions were filed; by 1970 more than 16,000 were being filed annually. See *The Annual Report of the Administrative Office of the U.S. Courts* (Washington, D.C.: U.S. Government Printing Office, 1972), p. 119.

<sup>50</sup> This requirement is not adhered to in all courts. In fact, some courts have been established so as to avoid the need to rely upon legal specialists. For a discussion of one type of court where lawyers are generally not employed see Barbara Yngvesson and Patricia Hennessey, "Small Claims, Complex Disputes: A Review of the Small Claims Literature" *Law and Society Review*, 9 (Winter, 1975), 219-274.

<sup>51</sup> The role of lawyers in adjudicative bodies is discussed by Herbert Jacob, *Justice in America*, pp. 60-64. See also Richard Wasserstrom, "Lawyers and Revolution," in *Radical Lawyers*, ed. Jonathan Black (New York: Avon, 1971), pp. 74-84.

<sup>52</sup> It should be noted, of course, that within a formal "zero-sum" structure of rules and rights the working ideology may be significantly more oriented toward compromise and official discretion. The disparity between the two has recently become the basis of much concern. See, *inter alia*, Kenneth Davis, *Discretionary Justice* (Urbana: University of Illinois Press, 1971).

<sup>53</sup> Paul Freund, "Social Justice and the Law," in *Social Justice*, ed. Richard Brandt (Englewood Cliffs, New Jersey: Prentice-Hall, 1962), pp. 110-117.

if somewhat awkward means of reinforcing or changing public policy. This is bound to have an important effect on the way in which essentially private or localized disputes are settled. For those who seek to bring public norms to bear in essentially private disputes, and thus broaden the range of perspectives relevant to their problem, litigation is an attractive mode of participation.<sup>54</sup> For them the "public regardness"<sup>55</sup> which is attached to all disputes brought to the courts is an advantage which outweighs group ties or cultural norms designed to discourage this form of political participation.

We have described and analyzed a variety of adjudicative alternatives which disputants employ to resolve particular disputes. As a general rule, we, like Herbert Jacob, believe that the likelihood that parties will bring disputes to courts varies inversely with the availability of less formal, and less public alternatives.<sup>56</sup> Where informal public or private remedies of all types are available they generally provide less costly means (in the psychological as well as material sense) of settling disputes and as such will be more attractive than formal, public bodies. The desire to minimize costs may be offset, however, by other factors, including the nature of the social context in which the dispute occurs, the nature of the dispute itself, the goals of the disputants, and their overall cost-benefit calculations. In the next section of the paper we will examine the influence of these factors on the process by which adjudicative institutions become involved in the definition, interpretation, and management of conflict.

### The Process of Mobilization

Understanding how adjudicative institutions become involved in conflict and conflict management is complicated by the absence, in most societies, of any statistics on the use of such institutions and by the inscrutability of the available data. In order fully to assess the role of courts and other adjudicators in dispute settlement it would be necessary to construct what might be called a "use-dispute index." Such an index would relate the frequency with which adjudicative institutions are mobilized to the total number of disputes in society. Construction of such an index is not possible at present since there is no available empirical information about the total number of dis-

putes in this or any other society; indeed, in most societies the only available usage information deals solely with the mobilization of courts.<sup>57</sup> The frequency and range of mobilization of other kinds of adjudicators remain a mystery.

**The Social Context.** Mobilization sets the parameters of adjudicative activity. There is little consensus about the factors most important in explaining why individuals or groups turn to adjudicative institutions to help in the resolution of social conflict. Anthropologists and sociologists have conceived of the mobilization of adjudication as one among many means of regulating social relations. They have tried to determine whether and why different types of adjudicative institutions are involved in dispute resolution by positing what might be called a "social develop-

<sup>57</sup> Such data as there are suggest that there are wide-ranging differences among societies in the frequency with which judicial intervention in dispute settlement is sought. This is amply demonstrated by the data presented below, a simple index of civil cases initiated per unit of population for selected countries. Even allowing for the crudeness of these data, the table does more than suggest the wide

Litigation Rates

Country	Civil Cases per 100,000 Population
Australia	5,277 (1969)
Denmark	4,844 (1969)
New Zealand	4,423 (1969)
Great Britain	3,605 (1969)
West Germany	2,085 (1969)
Japan	1,257 (1970)
Sweden	683 (1970)
Finland	493 (1970)
Norway	307 (1970)
South Korea	172 (1963)

variation in litigation rates. Some of the variations shown may be accounted for by different reporting techniques and by the concealment, under alternative labels, of what are essentially judicial processes. There is considerable variance in the accuracy of litigation statistics. Similar types of cases are treated differently in different countries. The use of administrative and other special tribunals to settle disputes which elsewhere come to courts of general jurisdiction affects the reliability of this index. The table presented above uses the "raw" figures and does not attempt to correct for functional similarities reported under different category headings. The number of civil cases filed at the basic trial level was ascertained in all cases except Great Britain from each nation's statistical abstract. The British figures were obtained from the report of the Lord High Chancellor, *Civil Judicial Statistics* (Command Paper 4721, July, 1971). The United States has not been included because of the unavailability of adequate statistics from all 50 states at the time this paper was written. See Joel B. Grossman and Austin Sarat, "Litigation in the Federal Courts. A Comparative Perspective," *Law and Society Review*, 9 (Spring, 1975), 321-346.

<sup>54</sup> For a discussion of the strategies of litigation employed by various reform groups see Joel Handler, "Social Reform Groups and the Legal System: Enforcement Problems" (Institute for Research on Poverty, Madison, Wisconsin, 1974).

<sup>55</sup> This value is first defined by Banfield and Wilson in *City Politics*.

<sup>56</sup> See Herbert Jacob, *Debtors in Court* (Chicago: Rand McNally, 1969), p. 20.

ient theory." According to this theory, variation in the process of mobilization is a function of the level of complexity, differentiation, and scale of a social structure. The argument is that as the complexity, differentiation, and scale of a society increases, reliance on courts and other formal-public adjudicators also increases.<sup>58</sup>

Increased reliance on formal-public types of adjudication results from changes in the nature of social relationships which appear to accompany processes of social development. In less developed societies, individuals have relatively stable and enduring contacts and relationships with a limited range of others.<sup>59</sup> Conflict which occurs in such settings is easier to resolve informally. Generally there will be a framework of trust, a larger context of ongoing relationships between the disputing parties,<sup>60</sup> and a concomitant desire to resolve the conflict in a way which permits restoration of normal relations.<sup>61</sup> Galanter notes, in comparing common law and indigenous processes of dispute settlement in colonial India:

The common law proceeds on the basis of equality before the law, while indigenous dispute settlement finds it unthinkable to separate the parties from their statuses and relations. The common law gives a clear-cut "all or none" decision, while indigenous processes seek a face-saving solution agreeable to all parties; the common law deals with only a single isolated offense or transaction while the indigenous system sees this as arbitrarily leaving out the underlying dispute of which this may be one aspect.<sup>62</sup>

In such a setting, we would expect a much more frequent reliance on indigenous processes than on the formal-public common law system.

In more complex societies, individuals may have a wider range of contacts. Fleeting and

transitory interaction between strangers is more common, and disputes occur more often between parties who have nothing more in common than the dispute itself. Under such conditions informal adjudication is a less acceptable alternative. But even in more complex societies, where relationships are ongoing and stable, as in certain economic transactions or in marriage, the mobilization of formal-public adjudicative institutions will generally be avoided.<sup>63</sup> The social development model predicts that courts and other formal and public adjudicators will play a more important role in dispute settlement in developed as opposed to underdeveloped or developing societies because non face-to-face relationships proliferate in these complex societies. As Marc Galanter formulates the hypothesis, "... the more inclusive in life space and temporal span a relationship between parties, the less likely it is that those parties will resort to the official system and more likely that the relationship will be regulated by some independent private system."<sup>64</sup>

The social development approach has entered political science in the work of Herbert Jacob. In his attempt to explain intercity differences in the use of courts in bankruptcy and garnishment proceedings, Jacob notes that individuals and business in cities marked by a "modern political culture" are more prone to resort to the courts to resolve their disputes than are those in less developed areas. Among the most important indicators of the presence of this culture motif are a high rate of geographic mobility among the residents of an area and a high level of bureaucratization.<sup>65</sup> Both are symptoms of breakdown in the kind of close personal and social relationships found in more traditional and less litigious cultures.<sup>66</sup>

The social development perspective has occupied an important place in the literature on conflict and conflict resolution, but it is especially problematic in its tendency to give rise to ideal types. Explanations based on this theory often do

<sup>58</sup> For a discussion of this relationship, see David M. Trubek, "Max Weber on Law and the Rise of Capitalism," 1972 *Wisconsin Law Review*, 720-753.

<sup>59</sup> P. H. Gulliver, "Case Studies of Law in Non-Western Societies," in *Law in Culture and Society*, ed. Laura Nader (Chicago: Aldine, 1969), p. 15. Also see Richard D. Schwartz and James C. Miller, "Legal Evolution and Societal Complexity," *American Journal of Sociology*, 70 (September, 1964), 159-169; and Richard D. Schwartz, "Legal Evolution and the Durkheim Hypothesis: A Reply to Professor Baxi," *Law and Society Review*, 8 (Summer, 1974), 653-668.

<sup>60</sup> Max Gluckman, *The Judicial Process Among the Barotses of Northern Rhodesia*, 2nd edition (Manchester, England: Manchester University Press, 1967), p. 21.

<sup>61</sup> Marc Galanter, "Why the 'Haves' Come Out Ahead: Speculations on the Limits of Legal Change," *Law and Society Review*, 9 (Fall, 1974), 95-160. Daniel Lev, *Islamic Courts in Indonesia* (Berkeley: University of California Press, 1972).

<sup>62</sup> Marc Galanter, "Hindu Law and the Development of the Modern Indian Legal System" (paper presented at the 1964 annual meeting of the American Political Science Association), p. 25.

<sup>63</sup> This is true to the point at which parties wish to terminate these relationships. See Macaulay, "Non-Contractual Relations."

<sup>64</sup> Galanter, "Why the 'Haves' Come Out Ahead," p. 130. Also Vilhelm Aubert, "Law as a Way of Resolving Conflicts," in Nadar, p. 286.

<sup>65</sup> Jacob, *Debtors in Court*, chapter 6.

<sup>66</sup> According to the social development model, as social relationships become attenuated and economic relationships more complex and less personal, there should be greater reliance on official and more public means of dispute settlement. If we accept urbanization (percentage of the population in cities of 100,000 or more) as one indicator of social development it is possible to hypothesize a relationship between urbanization and the rate of litigation. For the countries listed in the Table in Note 57 there is a correlation of .617 (sig. level .05) between urbanization and litigation rate.

not account for what Leopold Pospisil has identified as the multiplicity of legal levels that exist in any society.

Any human society . . . does not possess a single consistent legal system, but as many such systems as there are functioning subgroups. Conversely, every functioning subgroup of a society regulates the relations of its members by its own legal system, which is of necessity different, at least in some respects, from those of other subgroups.<sup>67</sup>

The social development model posits a process of displacement in which one form of dispute settlement becomes dominant in a society. It would be more accurate to argue that as societies develop, various modes of conflict resolution are incorporated into, rather than displaced from, a society's conflict management system.

Furthermore, the social development model implies, if it does not directly present, a linear assumption that the rate of mobilization of courts and other public-formal adjudicators increases directly and continuously as societies develop. The relationship between development and the prominence of these adjudicative institutions in dispute settlement, however, is true only up to a point.<sup>68</sup> A look at the development of litigation in England<sup>69</sup> and Spain<sup>70</sup> suggests that there may be some break point in this relationship; the rise in the prominence and use of courts which accompanies development may occur only until a certain "critical mass" has been reached.

In the linear model social relationships which approach a specified level of impersonality and complexity can no longer be handled dyadically or informally; resort to some formal public adjudicative institution is necessary. After reaching a further threshold, however, social relationships may become so much more complex and specialized that generalist courts no longer have the expertise, and hence the legitimacy, to intervene effectively. At this point administrative remedies or less formal mechanisms such as arbitration are invoked. When relationships become as interdependent in the complex organization of postindustrialized societies as they were—or are—in primitive ones, the need for harmonious problem solving which preserves relationships is reasserted,

with some resulting movement away from litigation.<sup>71</sup>

In sum, the process of involving adjudicative bodies in defining, interpreting, and managing conflict varies with the level of development of society. Figure I depicts a *hypothetical* relationship between the level of development and the mobilization of courts. In the least developed societies, courts, if they exist at all, may be used little.<sup>72</sup> More highly developed societies, or those in a rapid state of change, are likely to emphasize the role of courts and show higher rates of litigation. Finally, in the most developed societies, the process of mobilization is likely to result in a more balanced utilization of courts and other adjudicative bodies.

The mobilization of adjudicative institutions cannot be analyzed solely in terms of a social development model. We must focus also on additional variables which specify the relationship of social context to the process of mobilization.

Chief among these variables are the nature of the dispute between the parties, the goals of the

<sup>67</sup> The movement away from litigation to arbitration is well known and documented. For example, Bonn reports that from 1961 to 1969, in the Southern District of New York, the number of contract actions filed in the federal court declined absolutely, while at the same time the commercial case load of the American Arbitration Association more than doubled. Robert L. Bonn, "Arbitration: An Alternative System for Handling Contract Related Disputes," *Administrative Science Quarterly*, 17 (June, 1972), 254-264. See also Leonard Downie, Jr., *Justice Denied* (Baltimore: Penguin, 1972), pp. 202-203 and Jerome Frank, *Courts on Trial: Myth and Reality in American Justice* (New York: Atheneum, 1963), chapter 27.

A similar movement from litigation to arbitration has been noted in Sweden and Denmark. See Britt-Mari Blegvad, P.O. Bolding and Ole Landon, *Arbitration as a Means of Solving Conflicts* (Copenhagen: New Social Science Monographs, 1973), pp. 103-109; and Blegvad, "The 'Choice' Between Litigation and Arbitration" (presented at a Conference on Sociology of Judicial Process, University of Bielefeld, German Federal Republic, 1973).

We would expect that those who seek alternatives to litigation either have a better chance at winning in those alternative forums or see advantages in the speed, lower costs, flexibility and expertise of arbiters. On the other hand, arbitration has transferred a considerable amount of political and economic power away from public agencies to private groups who can operate outside of the restraints of legal and constitutional rules. See also James L. Stern, "Alternative Dispute Settlement Procedures," *Wisconsin Law Review*, No. 4 (1968), 1100-1112. Yet, private arbitration remains dependent on the existence of courts, and arbitration decisions themselves may be the subject of litigation, usually in a suit to compel enforcement. See Thomas J. McDermott, "Arbitrability: The Courts Versus Arbitration," *Arbitration Journal*, 23, No. 1 (1968), 18-37.

<sup>72</sup> See Jane Collier, *Law and Social Change Among the Zinacanton* (Stanford: Stanford University Press, 1973).

<sup>67</sup> Leonard Pospisil, "Legal Levels and Multiplicity of Legal Systems in Human Societies," *Journal of Conflict Resolution*, 11 (March, 1967), 3.

<sup>68</sup> This relationship has been examined for the United States by Grossman and Sarat, "Litigation in the Federal Courts."

<sup>69</sup> For a discussion of the development of litigation in Great Britain see Lawrence Friedman, "Functions of Trial Courts in the Modern World," pp. 21, ff.

<sup>70</sup> See Jose J. Toharia, "Economic Development and Litigation: The Case of Spain" (presented at Conference on Sociology of Judicial Process, University of Bielefeld, German Federal Republic, 1973), pp. 14, ff.

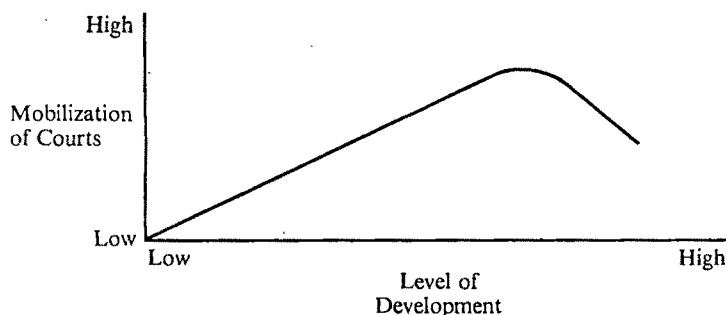


Figure 1. Social Development and the Mobilization of Courts

disputants, and perceptions of the costs and benefits of seeking different types of adjudicative relief. Of course, in every case the decision to involve courts or other adjudicators is predicated on a prior recognition that there is a more or less real conflict for which relief is possible, and that there is a remedy which can be sought. Studies of legal services programs have noted the importance of this threshold requirement in explaining the non-use of courts by poor people. And there is good reason to believe, though as yet little empirical evidence, that recognition of available legal remedies is generally low even in societies in which the frequency of litigation is comparatively high.<sup>73</sup>

**The Nature of the Dispute.** Conflicts may be distinguished on a variety of grounds.<sup>74</sup> Perhaps the most fruitful categorization involves the theoretical distinction between interests and values; in practice, of course there is considerable overlap. Conflicts of interest occur where two or more actors want the same thing. Normally, this type of situation is more conducive to compromise, either because the object in demand can be divided or because some equivalent can be substituted for it. Conflicts over values, however, tend to be less conducive to negotiation because they are less tangible and more symbolic. The currency of easy

compromise is often lacking and the psychic cost of compromising "principles" is much greater than that of a satisfactory mutual adjustment of other types of interests.

Of course, not all differences over values result in social conflict. People may agree to disagree, or simply ignore the contrary beliefs of others. Conflicts are frequently turned inward and resolved by processes of dissonance reduction. But where conflicts of value do occur, we expect that they are more likely to result in mobilization of formal-public adjudicators than are conflicts of interest. In practice, disagreements over principles may be resolvable only through the kind of zero-sum decisions which the judiciary provides; conflicts of interest may be more readily handled in other adjudicative or nonadjudicative settings.<sup>75</sup>

The involvement of any particular level of adjudicative institution also depends on the complexity of a dispute. One does not usually "make a federal case" out of a simple personal problem that could either be settled by the parties themselves because of a common interest in dispute avoidance or by quick reference to available third parties. So the process of mobilization clearly depends not only on the nature of a dispute—i.e., whether it involves interest or values—but also on its magnitude. A sense of right and wrong may condition the response of both parties to a dispute, but it may not be strong enough to merit taking

<sup>73</sup> On the recognition of legal needs see Gresham Sykes, "The Legal Needs of the Poor in the City of Denver," *Law and Society Review*, 4 (November, 1969), 255-277. Almond and Verba did report that only two per cent of their sample would try to change government policy by going to court. Jacob's application of the same questions produced comparable results. But what they were tapping was only a small component of a more generalized attitude about going to court to resolve disputes. See Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965), pp. 148-160; and Jacob, *Debtors in Court*, p. 233.

<sup>74</sup> Conflict, as used in this paper and generally in the literature, is a "state existing between two (or more) individuals characterized by some overt signs of antagonism." See Vilhelm Aubert, "Competition and Dissensus," *Journal of Conflict Resolution*, 7 (March, 1963), 25.

<sup>75</sup> Blegvad et al. report data which suggest that conflicts of interest may be more productive of arbitration and less productive of litigation than conflicts of value. *Arbitration as a Means of Solving Conflicts*, p. 139. However, this does not necessarily mean that conflicts of value will numerically predominate in the caseloads of courts. In fact, preliminary data from another study show without question that most litigation in American courts involves what would seem, by the above definition, to be conflicts of interest. What appears to be a conflict between these data and the hypothesis is easily resolved if one makes the entirely plausible assumption that conflicts of interest represent a much larger universe from which a relatively small sample of disputes is mobilized to the stage of judicial proceedings.

the risks and incurring the costs of bringing it to a public forum.<sup>76</sup> In the United States, it is first the cost of litigation, when weighed against the magnitude of the claim, that discourages most people from litigating automobile accident claims and encourages reliance on informal alternative means of settlement.<sup>77</sup>

The decision about how to deal with a dispute, no matter what its magnitude, is sometimes a matter of private choice and sometimes a matter of public policy; it may also reflect both factors. For instance, whether or not a dispute is taken to court is in some sense at the discretion of one of the litigants who may be forced by the nature of the dispute to take the more active hand in its settlement. This very discretion in deciding when to bring a dispute to court depends on the existence of alternatives to the courts, and it is increased the more that the presence of alternative dispute settlement mechanisms is accompanied by relatively open access to these forums. On the other hand, official policy and structure are important factors influencing the choice of sites in dispute settlement. Depending on the nature and magnitude of a dispute, government may act to prevent or penalize recourse to nonofficial arenas of dispute settlement; it may seek to structure disputes in such a way as to preclude private settlement. We know from the work of Edelman and others that what government does and says influences perceptions of who are adversaries, how they will behave toward each other, what is the extent of individual rights and how readily government may be called upon to protect such rights.<sup>78</sup> It suggests appropriate patterns of behavior and expectations about relationships in each type of dispute and it may stimulate and direct various forms of participation designed to influence how disputes are managed. Just as private norms may come to be reflected in public policy and law, so too law and policy may shape the substance and development of private norms. In sum, what government does influences the nature and existence of a dispute, the choice of how it is to be handled, and the rules for its resolution.

**The Goals of the Disputants.** The process of mobilization is importantly shaped by the goals of the disputants. Where goals are specific, as they often are in disputes between individuals, adjudication may satisfy the need for short-term, finite, and relatively immediate disposition of issues. Where goals are more diffuse and long term, as

they often are with organizations, the mobilization of adjudication will serve primarily instrumental purposes. Those pursuing longer-range, and more policy-oriented, goals are likely to seek an adjudicative framework in which a record can be accumulated, and where decisions will have precedent value. A high premium will also be placed on the attainment of rule or policy change.

In any conflict situation, the more active of the disputants may choose to bring the dispute to a arena where non-zero-sum resolutions can be obtained. In such a case the relationship among the parties in the dispute is not "strictly competitive." Alternatively, the nature of the goals may dictate a more competitive strategy in which negotiation or reconciliation is unacceptable. Additionally the goals of the disputants determine the extent to which they are willing to publicize their dispute. In some cases, recourse to courts will be a central element in a strategy which seeks to remedy or neutralize inequalities of status, bargaining power, and other resources.<sup>79</sup> Formal litigation may be strategically advantageous to less powerful individuals or groups since decisions derived from litigation are not made on the basis of power alone.<sup>80</sup> Finally, the process of mobilization will involve a decision about whether the dispute must be played out against certain formal rules or whether it can be handled in a less formal fashion. Such decisions about strategy are not exclusively the autonomous preferences of the disputants. Prevailing cultural norms about the legitimacy of private action (self-help) in one dispute but not another, or the preference for harmonious solutions in certain types of situations, will influence strategic decisions.

**Prior Experiences and Perceptions of Costs and Benefits.** The calculation of costs and benefits may be a strictly objective process for those organizations which repeatedly involve adjudicative bodies in conflict and conflict resolution. It may represent a more subjective and possibly more expressive decision on the part of individuals, particularly those with no prior experience in resolving disputes in this manner. Experience with

<sup>76</sup> H. Laurence Ross, *Settled Out of Court. The Social Process of Insurance Claims Adjustments* (Chicago: Aldine, 1970), p. 144.

<sup>77</sup> *Ibid.*

<sup>78</sup> Murray Edelman, *Politics as Symbolic Action* (Chicago: Markham, 1971), p. 10 and *passim*.

<sup>79</sup> As Dolbeare argues, people are more likely to use the courts when the balance of other political forces is against them, *Trial Courts in Urban Politics*.

<sup>80</sup> By involving the official third-party intervention of the courts, new rules and new actors, not always predictable, enter the dispute. Flexibility may be lost. When a dispute rises to a certain level of prominence the sphere of conflict may be enlarged, increasing the resources of one side to the disadvantage of the other. The political value of resources such as money, popular support, and interest group aggregation may be devalued in the judicial context, which places a great premium on individual effort, individual rights, and a more "objective" or abstract approach to conflict resolution.



Any single means of adjudication is a critical determinant of the actions of disputants at any time in the future. For example, as Galanter suggests, those with a long history of litigation activity will be more ready to litigate, and more successful in litigating, than those rarely faced with such a choice (although even such repetitive litigants as creditors and insurance companies only litigate in a fraction of the cases where it is possible to do so).<sup>81</sup> It should also be noted that choosing to litigate is not only a choice of plaintiffs. Insurance companies who appear primarily as defendants in personal injury cases may nonetheless "control" the decision to litigate by their prior bargaining tactics with individual tort claimants. The decision to take disputes to court is, thus, often a decision by one party who rejects the use of other alternatives.

The importance of repeated experience with courts and other formal-public bodies contributes, along with the cost factor, to the observed dominance of organizations and private groups as initiators of litigation. The major distinction among those who bring disputes to court, as opposed to bringing them to other adjudicative bodies, is based less on differences in attitudes or dispositions favorable to this particular form of participation than on differences in resources associated with varying levels of organization. As a general rule, the costs, both in time and money, of involving adjudicative institutions in conflict resolution rise as one moves from private and informal arenas to more public and/or formal bodies. The "costs" of mobilizing any particular adjudicator, however, may be as different among organizations with different goals and structures as they are between individuals and organizations who are experienced in this process. Losing a case involves only a bookkeeping debit to a large organization, with no demonstrable effect on the organization or its goals. To an individual disputant the costs of losing—but also possibly the benefits of winning—are significantly greater. Perhaps it is not surprising that organizations are the largest single users of the civil trial courts,<sup>82</sup> nor that the prototypical civil case is composed of an organization (most frequently creditor institution or banks) as plaintiff against an individual as defendant.

However it is calculated, we expect that disputants, whether individuals or groups, will

choose an adjudicative forum which they believe will help minimize their "conflict costs" and increase the likelihood of a desired outcome. Adjudicative forums generally provide an immediate payoff; someone wins and someone loses. As a result, before initiating a lawsuit an individual or group may try to calculate a "risk" factor and weigh this factor against the possible benefits to be derived either from doing nothing or from invoking a different, perhaps less formal, adjudicative process. This calculation involves an assessment of the nature of the dispute at issue, the relevant goals, and the relationship between the parties as well as the economic costs and benefits. This summary judgment goes far in establishing whether or not a court will be employed as a means of dealing with—i.e., defining, interpreting and managing—conflict.

In summary, the process of mobilizing adjudicative institutions is difficult and complex. It varies with the characteristics of the institutions themselves as well as with the nature of the social context in which they are located. The process of mobilization is also influenced by the nature and magnitude of the issues in dispute, the goals of the individual disputants, and, finally, a judgment of the costs to be incurred and benefits likely to be derived from using one or another alternative. The process of mobilization and the reasons for involving the courts in dispute settlement are important in determining how courts and other adjudicators operate and what they do. These factors provide an important focus for future research. Most important of all, the process of adjudicative mobilization has important consequences for the stability of government and the political system.

### The Impact of Adjudicative Mobilization

The various types of adjudicative processes we have discussed may exist separately or in combination; where they do exist in the same society their functioning is not clearly insulated or distinct. This is true both with respect to structure and methods of operation. Adjudication often is only a phase or tactic in ongoing processes of conflict resolution. Settlements reached through adjudication are not always definitive. Indeed, a common strategy of adjudication is to prolong and widen a dispute rather than settle it. Frequently, adjudication is only conclusive for the parties but not for the issue.

Even after "settlement," conflict and the need for conflict resolution often continue. Current solutions become part of future problems which, in turn, may involve a return to some of the adjudicative alternatives which we have discussed previously; or they may involve the displacement of conflict into a nonadjudicative setting. The

<sup>81</sup> Craig Wanner, "The Public Ordering of Private Relations," *Law and Society Review*, 8 (Spring, 1974), 421-440; and Galanter, "Why the 'Haves' Come Out Ahead." Also, at the individual level cost considerations seem to produce a bias in court usage in favor of higher SES people. See Robert Hunting and Gloria Neuwirth, *Who Sues in New York City* (New York: Columbia University Press, 1962), pp. 98-110.

<sup>82</sup> Wanner, p. 437 and *passim*.

processes by which disputes are resolved result in the fragmentation of some conflicts. This fragmentation is often required by the norms and procedures of the various agencies of dispute resolution. If an agency is equipped to handle only certain types of issues, conflicting parties are forced to sublimate or divert parts of their dispute which go beyond those issues. As a result disputes are often settled piecemeal. The phenomenon of fragmentation certainly is not unique to the judicial process, but it may help to account for the lack of finality often associated with court decisions.

In terms of the structure of dispute-settling alternatives, where a society possesses the full range of conflict-resolving devices, they can be depicted best in the form of a pyramid, with the more numerous private-informal mechanisms composing the foundation and the least numerous and more residual public and formal devices congregated at the top.<sup>83</sup> There is no strict hierarchical relationship intended here. The dimensions of publicness and formality, while they clearly increase as one approaches the top of the pyramid, do not always go together in the same proportion. Symmetry would be a poor guide to understanding this pyramid, yet it is not formless or without structure.

We suggested earlier that every government<sup>84</sup> has an important stake in the manner in which disputes arise and are resolved. It is better able to impose procedural requirements and influence the substantive results of dispute resolution the closer to the top of the pyramid that a dispute occurs. Adjudicative dispute resolution at the base may help to defuse and deflect social conflicts and to decrease the workload burdens on official dispute resolution mechanisms. But the procedures and,

more important, the results of low level adjudication may be noticeably at variance with those in the more public and formal processes. For example, plea bargaining often produces a more lenient pattern of sanctions than would be produced by formal adjudication of guilt. Consequently, it is not surprising to find that government has an immediate and direct interest in promoting the movement of some conflicts from the base of the structure of dispute resolution toward the top. This is certainly true where the government itself is a party to a dispute. But there are occasions where the government, as a surrogate third-party mediator, has a greater interest in "solving" the dispute than do the nominal adversaries.<sup>85</sup>

Alternatively, there are some conflicts whose very appearance in public arenas would be severe threats to stability, for example, conflicts about the form of government itself. One would expect government efforts to prevent or discourage such conflicts from surfacing or at least from reaching the top of the pyramid. Most often, government will follow a mixed policy. Statements and symbols of goals and values appropriate to a particular level of conflict resolution will be disseminated. At the same time protagonists will be encouraged to seek less visible means of redress, with the emphasis on conciliation rather than on formal judgment or sanctions. Formal government intervention will remain a last resort. Emergence of a conflict at the highest formal levels of dispute settlement may indicate a failure of lower level bargaining and accommodation; it may also represent the beginning of a new phase of conflict.

Accepting a particular mode of conflict resolution as legitimate, and participating in it, implies acceptance of a range of values which affects substantive results as well as the stability of the social order. Acceptance of a vague notion of "due process of law" or "fair procedure" by American courts has greater consequences than merely facilitating acceptance of certain rules and forms of adjudication. If this is correct, then not only the frequency with which different types of adjudicators are mobilized but the supporting values which make them viable mechanisms of conflict resolution have an important and positive impact on social and political stability. As de Tocqueville says in discussing the consequences of service on a jury,

The jury, and more especially the civil jury, serves to communicate the spirit of the judges to the minds of all the citizens. . . . It imbues all classes with a respect for the thing judged and with the notion of right. . . . It teaches men to practice equity; every man learns to judge his neighbor as he would himself be judged. . . .

<sup>83</sup> The location of the prototypical conflict resolution medium for a particular type of dispute would of course vary among societies. See Henry Hart and Albert Sacks, *The Legal Process: Basic Problems in the Making and Application of Law* (Cambridge, Tentative edition, 1958), pp. 312-313. The Hart and Sacks pyramid is often rightly criticized for over-emphasizing the importance and primacy of the upper layers. Our use of the same metaphor is descriptive only. Certainly it would be contrary to much that we say elsewhere in this paper to endorse a top down view of the legal system. Morton Grodzins's "Marble Cake" metaphor, by which he described the federal system, might almost be more appropriate. Grodzins, "Centralization and Decentralization in the American Federal System," in *A Nation of States*, ed. Robert Goldwin (Chicago: Rand McNally, 1963), pp. 1-4.

<sup>84</sup> In our discussion in this section of the paper we treat government as a single unified whole. We are aware, however, that this treatment does not do justice to reality. We recognize that one unit of government may encourage use of the courts while other units discourage use. This is particularly important where the courts themselves, as a part of government, encourage or discourage litigation while other government bodies may be doing the opposite.

<sup>85</sup> Cf. Eckhoff, "The Mediator and the Judge," p. 172 and *passim*.

The jury teaches every man not to recoil from the responsibility of his own actions. . . . By obliging men to turn their attention to other affairs than their own, it rubs off that private selfishness which is the rust of society.<sup>86</sup>

Initially, one might expect to find that in industrialized societies a high frequency of litigation would be positively related to stability since, as we suggested earlier, government may be better able to impose procedural and substantive constraints on conflict resolution in formal, public institutions.<sup>87</sup> Conversely, a high frequency of disputes unresolved, or unresolvable by recourse to litigation, would be inversely related to stability. It does not follow, however, that a low frequency of litigation in industrialized societies will contribute to or is symptomatic of instability. A low level of litigation may indicate a high degree of formal and informal consensus about basic values throughout the population, which serves to depress potential conflict. Or it may reflect the existence of alternative dispute-resolution techniques which perform the same function as litigation. As in Japan, it reflects an historical and culture antipathy toward formalization of dispute resolution. Indeed, any society which was so rule-oriented and formalistic as even to approach the conditions suggested by the paradigm of "perfectly litigious man"<sup>88</sup> would evince instability for several reasons, not the least important being rigidity and inability to respond to rapidly changing social conditions. Finally, as an inspection of the data reported in footnote 57 suggests, there is probably no *direct* relationship between the frequency of litigation and social stability.

Nevertheless, there are important ways in which litigation affects stability indirectly. We can identify at least three. First, we have already suggested that the output of litigation often serves as a guide to the nonlitigious settlement of many similar disputes, becoming an important factor in private and informal bargaining. Beyond that, however,

<sup>86</sup> Alexis de Tocqueville, *Democracy in America* (New York: Knopf, 1945), pp. 291-297.

<sup>87</sup> A high frequency of litigation in a particular geographic area, or about a particular set of problems, might indicate temporary instability but promise a transition into a more stable environment. There are numerous examples just in the United States in recent years—questions of the rights of juveniles, prisoners, students, and persons subjected to involuntary civil commitment—of relationships which had been handled more or less bureaucratically and one-sidedly but which have now become "judicialized" and at least temporarily less stable. One could take the position that schools or prisons were run more efficiently when all disputes were settled internally. Or it could be argued that recourse to the courts provided a safety valve to stabilize conditions which, because of other social forces, were no longer responsive to the old rules.

<sup>88</sup> See Jerome Frank, *Courts on Trial*, p. 92 and *passim*.

litigation also serves as a structural model. Just as one finds basic majoritarian principles and parliamentary rules of order in many private groups, so too it is likely that the procedures of litigation in the courts, procedures designed to bolster the legitimacy of both the courts and the regime of which they are a part,<sup>89</sup> are widely copied and utilized in private dispute settlements.<sup>90</sup> American society, as has been noted by many observers, is characterized by a strong and increasing tendency toward legalization, or governance by rules. Litigation appears to promote this tendency, and is itself in turn a product of it.<sup>91</sup>

Second, litigation plays a role in absorbing pressures in key issue areas that might otherwise threaten the capacity of the nonadjudicative conflict resolution system. Grievances, even those beginning in a dyadic and fairly private relationship, may, if not settled, escalate into more intense and often more organized and widespread demands on the political system. Politicians who criticize judicial decisions proclaiming a particular set of rights as threats to their own power and authority often fail to appreciate the contribution of such decisions to system maintenance. On occasion litigation promotes decisions which appear to threaten the sovereignty of legislative or executive branches. But often litigation actually translates a number of demands which really do threaten a system into decisions which appear as symbolic victories to those seeking change, while at the same time preserving the existing system with little measurable change. This kind of "conflict displacement," which characterized early responses to the civil rights movement in the United States,<sup>92</sup> is possible because participants in the judicial system are encouraged to take comfort in the "individual" treatment each will receive, apart from others of the same class or with similar grievances. These are precisely the circumstances which can most effectively depoliticize and absorb large numbers of grievances while minimizing aggregative threats to the system. Litigants who recognize the common ground they share with others may well be disposed to seek a different and more threatening kind of participation. Not

<sup>89</sup> For an interesting discussion of the relationship of court procedures and regime legitimacy see Isaac Balbus, *The Dialectics of Legal Repression* (New York: Russell Sage Foundation, 1973), chapter 1.

<sup>90</sup> See Theodore L. Becker, *Comparative Judicial Politics* (Chicago: Rand McNally, 1970), pp. 109-112. Public norms may also reflect emerging norms of private dispute resolution.

<sup>91</sup> See Stuart Scheingold, *The Politics of Rights* (New Haven: Yale University Press, 1974), p. 48 and *passim*. Also see *Goss v. Lopez*, 42 L.Ed.2d 725 (1975), where the Supreme Court extended some due process guarantees to high school students threatened with suspension.

<sup>92</sup> Duane Lockard, *Toward Equal Opportunity* (New York: Macmillan, 1968), p. 142.

only does litigation encourage particularistic problem-solving efforts but, as Scheingold has observed, its disaggregative tendencies may actually impede the implementation of hard-won policy goals.<sup>93</sup> In the United States, the growth of class actions, test cases, and generally the use of litigation as a tactic to mobilize political forces has tended somewhat to counter its otherwise isolative tendencies.

The third, and perhaps most important, stability-related function of litigation concerns its impact on individual attitudes toward, and support of, regime values. We have already suggested that litigation promotes acceptance of rules of fair procedure and acceptance of the legitimacy of the institutions which approve and profess to follow these rules. It also produces a feeling of confidence that the political system is concerned with fairness and justice, a feeling which encourages individuals to accept decisions with which they might not agree, but decisions which are made fairly. This is not to say that litigation, or the values which are embodied in formal-public adjudicative institutions, can legitimize the entire political system; in fact, there is some evidence that contact with the courts does not even produce favorable evaluation of the courts.<sup>94</sup> Litigation is just one process among many which contribute to, but cannot themselves insure, the maintenance of support for the political system of which it is a part.

Although litigation in some way contributes to system stability through its influence on individual attitudes, it may also encourage certain attitudes which are, at least in part, threatening to that stability. Specifically, litigation encourages claims of "rights," claims which, as Friedman has so perceptively noted, may be dysfunctional to social equilibrium. By encouraging people to think of themselves as possessors of "rights," litigation encourages the transformation of conflicts of interest into conflicts of principle, conflicts which are both more intense and more difficult to resolve. At the same time, litigation promises more than it can provide. The institutions of a society are usually not set up to handle all theoretically conceivable rights, all rights to which people may feel entitled. One basic though for obvious reasons unstated assumption of a complex system is that not all rights can or will be claimed.<sup>95</sup> Litigation, by encouraging claims of "right,"<sup>96</sup> makes it difficult for the entire political system to discourage, as it must, certain kinds of claims. Yet, para-

doxically, restrictive rules of access to the judicial system insure that the presentation of claims of "right" is a long, difficult, and discouraging process. Only those with persistence and resource can actually make an effective legal claim of "right." As Macaulay and Walster have noted, the legal system may also promote certain rights as theoretically absolute but in fact encourage bargaining which makes the full attainment of such rights impossible.<sup>97</sup>

At this point, we should recognize the existence of what would appear to be a theory of the role of courts and litigation counter to our own. Theodore Becker has argued that

the key determinant of the effect the court structure has in society may well be a *widely believed* myth that the court is an instrument of social order. The fact that most people in a social group believe that order is forthcoming because of the courts may be the sufficient condition to the maintenance of that very order. That this is held as likely (both consciously and subconsciously) by certain elites probably accounts for the vast amount of literature that supports this belief and helps to reinforce it over time.<sup>98</sup>

Indeed, Becker might well argue that our effort here is just another contribution to the myth.

While there is certainly not adequate evidence to support all the propositions we advance, even less evidence supports Becker's hypothesis. The evidence Becker presents shows only that in some societies social order is maintained without great reliance on litigation or the courts. Of course this is true. But it is doubtful that the people in *those* societies *believe* that the existence of courts is important to the social order. On the other hand, we would not deny that attitudes of efficacy, trust, and diffuse support of governmental institutions, including courts, may be based on citizen misconceptions about themselves and their relationship to government. There is little evidence that people can or do think abstractly enough about the courts to link them to the maintenance of social order. But they may have sufficiently positive feelings about judicial institutions to support a general sense of satisfaction, otherwise derived, which may result in quiescence.

In closing, it is important to note that stability is only one of several dimensions on which to measure the consequences of litigation and litigiousness. Justice and social change are two obvious and important alternatives. A judicial system unable to provide more than the mere symbols of justice is unlikely to be regarded as an attractive dispute-resolution forum by potential litigants. Indeed the importance of the courts as one type of adjudicative institution ultimately

<sup>93</sup> Scheingold, *The Politics of Rights*, pp. 8, 130 and *passim*.

<sup>94</sup> Jacob, *Debtors in Court*, pp. 117-124.

<sup>95</sup> Lawrence M. Friedman, "The Idea of Right as a Social and Legal Concept," *Journal of Social Issues*, 27, No. 2 (1971), 189-198.

<sup>96</sup> The importance of rights and their relation to litigation and litigation strategies is also discussed by Scheingold, *The Politics of Rights*.

<sup>97</sup> Stewart Macaulay and Elaine Walster, "Legal Structures and Restoring Equity," *Journal of Social Issues*, 27, No. 2 (1971), 173-188.

<sup>98</sup> Becker, p. 113.

rests on their ability to make just decisions. But, in addition, courts must provide a general example of *how* to reach just decisions and of how reasoning and sound judgment can help solve social problems. A system which does not set an example of how just decisions are reached, and of what is just, ought not to be regarded as successful even if political quiescence and social stability are achieved. The legal process will be effective in resolving disputes only if it promises and performs in accord with distinctively "legal" values.<sup>99</sup>

Very much related to the concern for justice is the achievement of progressive social change; often the two are equated. No one would argue with the proposition that to remain viable and dynamic, a legal system must reflect major changes in a society. But how much can one expect of the law, and litigation, in positively promoting such change—leading a society instead of following it? Litigation, often at the constitutional level, has played such an important role in promoting equality and due-process norms in the United States in recent years that inevitably a set of exaggerated expectations has developed. Resolution of conflict will always be part of the dynamic of change. But we must be wary, as Trubek and Galanter remind us, of the fallacy of defining legal reality to conform to our moral aspirations. The Liberal-Legalist paradigm, as they define it, manifests a

"pervasive belief in the ultimate efficacy of legal rules as instruments of social change."<sup>100</sup> Yet the observed discrepancy between the policy outputs of litigation—rules—and the problems of implementation suggest considerable caution in judging the role of litigation primarily by this standard.

### Conclusion

In this paper, we have tried to assess the role of courts and other adjudicative institutions in the definition, interpretation, and management of conflict. We have suggested that such an assessment can be carried out both by comparing courts with other governmental institutions and by comparing courts with institutions performing similar functions in other parts of society; and we have presented the second type of comparison. We have also suggested that to understand the operation of courts and other adjudicative institutions, we must understand the process through which they are mobilized and involved in conflict resolution. Finally, we have argued that this process of mobilization and the more specific process through which courts become involved in defining, interpreting and managing conflict will have important consequences for the political system. In most cases, we have not had the data necessary to test the relationships we suggest or the arguments we present. We hope that our observations will provide at least an initial guide to verification and further theory construction.

<sup>99</sup> This line of argument was suggested to us by Hadley Arkes. We are not advocating a return to "juridical authority" in the same sense in which Theodore Lowi uses the term. Lowi, *The End of Liberalism* (New York: Norton, 1969), chapter 10. For a good expression of the contrary view, that legalization is the sign of a collapsing social order and not the means of its rejuvenation, see Sanford Levinson, "The Rediscovery of Law," *Soundings* (Fall, 1974), pp. 318–337, esp. p. 333.

<sup>100</sup> David T. Trubek and Marc Galanter, "Scholars in Self-Estrangement: Some Reflection on the Crisis in Law and Development Studies in the United States," 1975 *Wisconsin Law Review*, 1062–1102. See also Robert Booth Fowler and Joel B. Grossman, "Law, Liberalism and Social Change: A Preface," *American Politics Quarterly*, 2 (July, 1974), 276–312; and Handler, "Social Reform Groups."

# Mass Political Attitudes and the Survey Response\*

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## Introduction

There can be little doubt that the sophisticated electorates postulated by some of the more enthusiastic democratic theorists do not exist, even in the best educated modern societies. Whatever else students of public opinion find unsettled, agreement is widespread that citizens have, at most, a general grasp of political issues without having well-developed opinions on every question of public policy. Indeed, no public opinion surveys are necessary to establish the point. The sheer volume of business in a large nation makes it impossible for even the most studious voter to follow more than a fraction of it.<sup>1</sup>

Of course, a theory of representative democracy need not require citizen opinions on every issue. Representatives usually are not expected to be mere administrative conveniences, meekly reflecting their constituents' preferences on every bill and thereby saving the polity the expense of continuing referenda. Rather, theories of representation typically attempt to treat politicians as specialists in governmental affairs, thereby saving the ordinary man for other tasks.<sup>2</sup> What is required of citizens is only enough sophistication to choose their representatives. In turn, this requires that the voters implicitly make use of at least some broad notions of public policy, lest they be unable to judge the various candidates for their vote.<sup>3</sup>

The general direction of public opinion research has been to assert that even these minimal orien-

tations toward the polity are lacking. In particular, the public opinion surveys reported by the University of Michigan Survey Research Center (SRC) have powerfully supported the bleakest views of voter sophistication.<sup>4</sup> While the SRC has amassed varying evidence about the conceptual skills of voters, the predominant impression these studies yield is that the average citizen has little understanding of political matters. Voters are said to be little influenced by "ideology,"<sup>5</sup> to cast their votes with far more regard to their party identification than to the issues in a campaign,<sup>6</sup> and often to be ignorant of even the names of the candidates for Congress in their district.<sup>7</sup> Needless to say, the impact of these conclusions on democratic theory is enormously destructive.

In fact, an even more fundamental charge has been lodged against the mass public. Criticisms like those just mentioned make the case that voters' preferences on public policy matters are unsophisticated or poorly organized, or that they are without influence in the voting decision. Serious as these allegations are, they stop short of claiming that voters have no policy views whatever. This last charge—that the typical citizen's political thoughts appear ill focused simply because he lacks an image of the world—is far more disturbing than the others. For it is one thing to argue that voters have difficulty connecting their preferences to particular candidates and particular schools of thought, quite another to claim that the preferences are absent from the beginning. Yet, just this claim has been made, notably in the

\* I would like to express my thanks to John Aldrich, Lloyd Etheredge, Richard Katz, Gerald Kramer, David Mayhew, Richard Niemi, Douglas Rae, and Charles Whitmore for helpful comments on earlier drafts. Remaining errors are, of course, the responsibility of the author. The data used were collected by the Survey Research Center at the University of Michigan and made available through the Inter-University Consortium for Political Research. Neither body is responsible for the analysis and interpretation presented here.

<sup>1</sup> For detailed arguments on this point, see Robert A. Dahl, *After the Revolution?* (New Haven: Yale University Press, 1970), pp. 40–56; and Bertrand de Jouvenal, "The Chairman's Problem," *American Political Science Review*, 55 (June, 1961), 368–72.

<sup>2</sup> A helpful discussion of the many versions of "representation" is Hanna Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1967).

<sup>3</sup> Even a constituent choosing a legislator in the Burkean tradition requires *some* political opinions, lest he have no means of evaluating the candidate's political judgment.

<sup>4</sup> See especially Angus Campbell et al., *The Voter Decides* (Evanston: Row, Peterson, 1954); Angus Campbell et al., *The American Voter* (New York: Wiley, 1960); Angus Campbell et al., *Elections and the Political Order* (New York: Wiley, 1966); Philip E. Converse et al., "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 321–36.

<sup>5</sup> Campbell et al., *The American Voter*, pp. 227–34.

<sup>6</sup> Campbell et al., *The American Voter*, ch. 5. A comprehensive review of related literature is Peter B. Natchez, "Images of Voting: The Social Psychologists," *Public Policy*, 18 (Summer, 1970), 553–88. Analyses of voting that give less weight to party identification include David E. RePass, "Issue Saliency and Party Choice," *American Political Science Review*, 65 (June, 1971), 389–400; and Richard W. Boyd, "Popular Control of Public Policy: A Normal Vote Analysis of the 1968 Election," *American Political Science Review*, 66 (June, 1972), 429–49.

<sup>7</sup> Donald Stokes and Warren Miller, "Party Government and Saliency of Congress," in Campbell et al., *Elections and the Political Order*, p. 204.

well-known treatment of belief systems in mass publics by Philip Converse.<sup>8</sup>

Converse presents data indicating that the rank-order correlation between what a typical American thinks about a political issue this year and what he will believe two years hence is quite low—.30 to .40 on questions of foreign aid, isolationism, and federally guaranteed employment, for example. This seems to mean that there is little continuity in most voters' thinking. The correlations improve somewhat for racial issues and, more dramatically, for party identification. Converse argues that the high correlations for party ID show that consistency is possible for mass man; it simply does not occur for broad policy issues, since most voters find them meaningless and give a random response.

Similarly, Converse showed that the correlation between pairs of *different* attitudes at the same time was low, usually lower than the correlation over time for either one of them. A voter who believed in federally guaranteed jobs, for example, was only a little more likely than a voter who opposed them to favor government intervention in the housing and electricity markets, in spite of the common intellectual issues seemingly involved. Similar results have appeared in several other studies.<sup>9</sup>

The implication of both these findings, Converse argues, is that even relatively central and permanent issues of American political life have no meaning for the voters, that the issues involved do not form a part of voters' conceptual framework. Hence, for a large part of the population, the answers are essentially random. No real attitudes exist.

Some resistance to these conclusions has developed. Unfortunately, little empirical evidence

has been developed to aid Converse's opponents, and most of the resistance has been confined to reinterpretation of his pessimistic results, with an attempt made to explain the findings on other grounds. Thus one occasionally hears the argument that Converse's low over-time correlation coefficients on political attitudes should not surprise us, since the phenomenon of attitude change is common enough. Those familiar, however, with just how weak a relation is represented by a correlation of .30 or .40 will not find the argument very persuasive. Citizens undergoing such erratic attitude change over a two-year interval will inevitably be suspected of having little real concern with the issues involved.

Among the few empirical re-examinations of Converse's findings, perhaps the best is Brown's attempt to demonstrate that voters exhibit considerable over-time attitude stability.<sup>10</sup> Thirty-six subjects, half designated "political articulates" and half "political inarticulates" (on the basis of subjective evaluations by the experimenters), were each given forty-eight general political statements (e.g., "Just because a man is a human being doesn't make him smart enough to vote"). The subjects were then asked to place these statements on a scale of -5 to +5 indicating their degree of agreement. Two to six weeks later, the operation was repeated. The placing of the statements typically correlated at .60 to .90 over the time interval, and there seemed to be no major differences between articulates and inarticulates in the strength of the correlations.

Brown's results are intriguing, but not entirely persuasive. One difficulty is that the brief interval between surveys in his study may have allowed respondents to remember their previous answers. If so, the correlations would be biased upward. Furthermore, even if no memory effect occurs, respondents might well exhibit only a little instability over a short interval. A six-week period, after all, is less than one-seventeenth of Converse's two-year interval, and it is perhaps not surprising that attitude stability is greater in the shorter period. This difference in the time interval be-

<sup>8</sup> Philip Converse, "The Nature of Belief Systems in Mass Publics," *Ideology and Discontent*, ed. David E. Apter (New York: Free Press, 1964), pp. 206-61. See also Philip E. Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue," *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte (Reading, Massachusetts: Addison-Wesley, 1970), pp. 168-89.

<sup>9</sup> Robert Axelrod, "The Structure of Public Opinion on Policy Issues," *Public Opinion Quarterly*, 31 (Spring, 1967), 51-60; Edward C. Dreyer, "Change and Stability in Party Identification," *Journal of Politics*, 35 (August, 1973), 712-22; and Donald D. Searing et al., "The Structuring Principle: Political Socialization and Belief Systems," *American Political Science Review*, 67 (June, 1973), 415-32. See also Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June, 1964), 361-82. For contrary views on related matters, see Norman R. Luttbeg, "The Structure of Beliefs among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 398-409; and Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956-1968," *American Political Science Review*, 66 (June, 1972), 415-28.

<sup>10</sup> Steven R. Brown, "Consistency and the Persistence of Ideology: Some Experimental Results," *Public Opinion Quarterly*, 34 (Spring, 1970), 60-68. Since this article was written, John C. Pierce and Douglas D. Rose have published "Nonattitudes and American Public Opinion: The Examination of a Thesis," *American Political Science Review*, 68 (June, 1974), 626-49, which is also an interesting re-examination of Converse's findings. Its idiosyncratic statistical methods and restrictive assumptions, however, make possible several different interpretations of some of its key results, not all of them favorable to the authors' thesis. Many of these difficulties are pointed out by Converse in his powerful reply, pp. 650-60, in the same issue. (See also the rejoinder by Rose and Pierce, pp. 661-66.)

tween surveys, along with other differences in methodology and sampling techniques, makes it difficult to compare Converse's and Brown's results.

In addition, Converse's survey questions concern national public policy issues, while Brown's deal with very general attitudes toward politics and political actors. Thus one might believe Brown's results and yet remain uncertain about citizens' abilities to cope with even the broadest dimensions of policy in a political campaign. Some such ability seems essential to informed voting decisions. It is difficult to choose among American politicians on the basis of the attitudes Brown measures—beliefs about universal suffrage or the fundamental equality of all men.

If, therefore, Converse's argument can be escaped neither by denying the necessity of public knowledge of policy questions nor by claiming that his respondents exhibit only moderate amounts of attitude change, then his conclusions seem to remain as disturbing as ever. Democratic theory loses its starting point—public opinion on policy matters.

#### Individual Choice Behavior

A possible escape route remains. Psychologists studying the theory of individual choice have formulated models of human decisions that allow further analysis of Converse's data. These models usually insist that human choice is stochastic, that is, probabilistic. Given a variety of decision situations, each with apparently the same evidence and circumstances, individuals will nevertheless not make identical choices in all of them. Rather, alternative choices are made with varying probabilities. One may regard this phenomenon as an inherent characteristic of human choice or as a failure of the experimenter to recognize all the factors that determine choice: the experimental outcome is the same. People do not have a single preference in most decision situations.<sup>11</sup>

Applied to the analysis of survey questions, this conclusion of the individual choice studies means that citizens will make responses probabilistically. This latter conclusion is not fruitful, however, until two sources of probabilistic choice are disentangled: the variation that is due to the objects of choice and the variation that is due to the chooser himself. In a series of experiments in

which subjects were asked to choose among color of grey, Coombs<sup>12</sup> demonstrated (1) that subject evaluated any given shade of grey differently at one time point than at another—variation owing to objects of choice—and (2) that their own most preferred shades also varied from one time period to another—variation owing to the subjects themselves. Subjects were thus somewhat unclear about the choices available to them and about their own preferences.

Schematically, a subject's most-preferred shade may be thought of as a point along a continuum of "greyness," and the other available shades (as perceived by him) may be arrayed as points to its left and right along the same line. The implication of Coombs's study is that both the preferred point and the others may vary somewhat around their mean position for the subject. In a given choice, a subject may be thought of as drawing his most preferred point from the distribution around his central preferred point and likewise drawing the perceived relative positions of the other points from their own distributions. A model of this sort explains choice much better than versions which take either preferences or objects of choice as fixed. (See Figure 1.)

If the results of this study were applied to survey questions, one might expect that the respondent's genuine opinion about, say, foreign aid would be, not a single point, but rather a distribution of points around some central position. Respondents would be assumed not always to "know their own mind." Similarly, the objects of choice, both the questions and the response categories ("strongly agree," "agree," and so on) would also be distributions around central points. That is, survey questions would be treated as vague: respondents will not always respond the same way to the same question even if their attitudes remain unchanged. A subject may say "strongly agree" one time and "agree" the next, simply because of the ambiguity of the question asked or because he is uncertain how strong is "strongly."

Hence, if one were to compare a citizen's responses at any two time periods, one would expect something less than perfect correlation. The size of the correlation would depend on how vague the questions and response categories are (variation owing to the objects of choice) and on how unstable the respondent's views are (variation owing to the subject).

This distinction is important because the normative implications of the two kinds of variability are rather different. The subject's changing posi-

<sup>11</sup> Two useful reviews of the individual choice literature that discuss stochastic choice and the evidence for it are Ward Edwards, "The Theory of Decision Making," *Psychological Bulletin*, 51 (1954), 380-417, and Ward Edwards, "Behavioral Decision Theory," *Annual Review of Psychology*, 12 (1961), 473-98. Both are reprinted in *Decision Making*, ed. Ward Edwards and Amos Tversky (Baltimore: Penguin, 1967), which also includes other discussions of the same topic.

<sup>12</sup> C. H. Coombs, *A Theory of Data* (New York: Wiley, 1964), pp. 106-18. Reprinted in Edwards and Tversky, pp. 319-333.



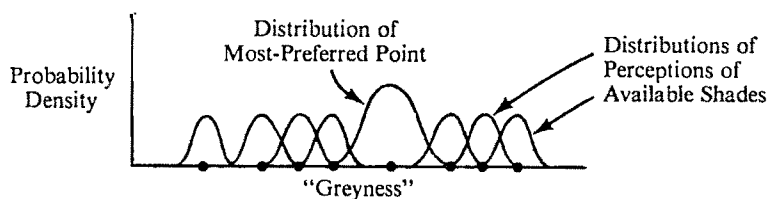


Figure 1. Probability distributions of subject's most-preferred shade of grey and of his perceptions of the shades available for choice

tion presumably represents the sort of variation Converse had in mind. It measures instability of viewpoint. On the other hand, the variation in the questions themselves might conveniently be called "measurement error," since it refers to the inaccuracy with which the underlying attitude is reflected by the survey instrument. If survey questions are vague, then even perfectly stable respondents will appear to be inconsistent. The variation in their responses, therefore, represents errors of observation by the researchers. It is clear that this latter sort of variability is much less serious for democratic theory: it reflects a flaw in the survey research method rather than in the responses of subjects.

Converse does not distinguish between these two sources of variability in voters' responses. In his analysis, both kinds of variability are attributed to subjects' changing positions. The result is an underestimate of voters' attitudinal stability.

The immediate question, of course, is just how much bias Converse's analysis introduces. A statistical model is now put forward which allows an estimate of the stability of voters' responses, uncontaminated by measurement error.

#### A Model of the Survey Response

Consider the case of a voter whose opinion never changes. This voter may appear to change his views because of the effect of measurement error, but in fact his views remain constant. Now if three interviews are taken with this voter at two-year intervals, as was done with Converse's respondents, we expect his views at time 1 and time 3 to correlate as well as those at time 2 and time 3. If measurement errors obey the usual assumptions (see below), then since all three time periods exhibit the same opinion, there is no reason to think that any two will correlate better than any other two.

By contrast, consider a respondent whose opinions do change but whose responses are observed without error. All observed variation in his views represents genuine opinion change. Suppose for a moment that these changes are essentially random shocks: knowing how a voter changed his views between times 1 and 2 provides no information about how he will change between times 2

and 3. He undergoes a series of random opinion changes. For such an individual, clearly, it will be easier to predict his views at time 3 from time 2 rather than from time 1, simply because one has more up-to-date information at time 2. The voter has had less time to move away. For such a respondent, opinions at the three time periods will *not* be equally correlated. Correlations between adjacent time periods should be higher than that between times 1 and 3. In fact, given the correlations between periods 1 and 2 and between 2 and 3, we can predict under these assumptions precisely what the correlation between periods 1 and 3 should be.

In summary, when a voter is stable in his views and *all* observed variability is measurement error, correlations will be equal across time periods. At the other extreme, when a voter is unstable and there is *no* measurement error, correlations should become smaller at a predictable rate as time periods become more distant from each other. By observing where the data fall between these polar cases, the model estimates the measurement error in each survey question.

The most important assumption in this model is that opinion changes are random. It is this assumption that makes possible the statistical analysis reported below. One would expect in general, however, that opinion change for most respondents is not random. Instead, if a voter becomes more conservative over the first time period, it seems more probable than not that he will continue to do so in the next period. In other language, we expect on the average a positive association between opinion changes in adjacent time intervals. It is shown below that ignoring this possible positive association biases the model in Converse's direction—toward estimates of less measurement error and more respondent variability. As a first approach, therefore, the simpler assumption of random opinion change was made.

By the nature of the assumptions, all sources of observation error are implicitly counted as "measurement error." For example, the model postulates interval-level data, though our measurements are only ordinal. Hence observation error includes not only that from the nature of the questions themselves, discussed in the text, but also the

information lost by having only ordinal categories instead of the (postulated) interval scale. Also included as measurement error are those errors that are due to clerical mistakes in coding responses and the biases due to differences in the personal styles of interviewers.<sup>13</sup> In short, any observational errors which meet the assumptions about "measurement error" set out in the model will be included. By far the largest part of the error, however, is expected to be due to the vagueness of the questions.

More formally, denote by  $p_t$  the voter's actual opinion at time  $t$  and by  $x_t$  the observed opinion at time  $t$ . Then define  $u_t$ , the change in the voter's actual opinion from time  $t$  to  $t+1$ , and  $e_t$ , the error of observation at time  $t$ , as follows:

$$\text{Defs. } u_t = p_{t+1} - p_t \quad (1)$$

$$e_t = x_t - p_t$$

In particular, since Converse's study employs three time periods, we have from (1) and (2) for each voter:

$$x_1 = p_1 + e_1 \quad (3)$$

$$x_2 = p_2 + e_2 = p_1 + u_1 + e_2 \quad (4)$$

$$x_3 = p_3 + e_3 = p_1 + u_1 + u_2 + e_3 \quad (5)$$

We make the usual assumptions about measurement errors, that they have an expectation of zero and are uncorrelated with each other and with the variable of interest, actual opinion. We also assume that error variance is constant across time periods, i.e., that the vagueness of each question is a constant.

#### Assumption Set 1:

$$E(e_t) = 0 \quad (\text{all } t) \quad (6)$$

$$E(e_t e_s) = 0 \quad (\text{all } t, s, t \neq s) \quad (7)$$

$$E(p_t e_s) = 0 \quad (\text{all } t, s) \quad (8)$$

$$E(e_t^2) = \sigma_e^2 \quad (\text{all } t) \quad (9)$$

Finally, it is assumed that an individual's opinion changes are independent of each other.

#### Assumption Set 2:

$$E(u_t u_s) = E(u_t)E(u_s) \quad (\text{all } t, s, t \neq s) \quad (10)$$

We now observe the following:  
from (1) and (8):

<sup>13</sup> For examples of the detection of interviewer bias, see Michael J. Shapiro, "Discovering Interviewer Bias in Open-Ended Survey Responses," and W. Andrew Collins, "Interviewers' Verbal Idiosyncrasies as a Source of Bias," both in *Public Opinion Quarterly*, 34 (Fall, 1970), 412-15 and 416-22, respectively.

$$E(e_t u_s) = 0 \quad (\text{all } t, s) \quad (11)$$

from (3)-(6):

$$E(x_2 - x_1) = E(u_1) \quad (12)$$

$$E(x_3 - x_2) = E(u_2) \quad (13)$$

from (3)-(5), (7), (9), (11):

$$\begin{aligned} E(x_2 - x_1)^2 &= E(u_1 + e_2 - e_1)^2 \\ &= E(u_1^2) + 2\sigma_e^2 \end{aligned} \quad (14)$$

$$\begin{aligned} E(x_3 - x_2)^2 &= E(u_2 + e_3 - e_2)^2 \\ &= E(u_2^2) + 2\sigma_e^2 \end{aligned} \quad (15)$$

$$\begin{aligned} E(x_3 - x_1)^2 &= E(u_2 + u_1 + e_3 - e_1)^2 \\ &= E(u_1^2) + E(u_2^2) \\ &\quad + 2E(u_1 u_2) + 2\sigma_e^2 \end{aligned} \quad (16)$$

Finally, from (12)-(16), and using (10):

$$\begin{aligned} &\frac{1}{2} \{ E(x_2 - x_1)^2 + E(x_3 - x_2)^2 - E(x_3 - x_1)^2 \\ &\quad + 2E(x_2 - x_1)E(x_3 - x_2) \} \\ &= \frac{1}{2} \{ E(u_1^2) + 2\sigma_e^2 + E(u_2^2) + 2\sigma_e^2 \\ &\quad - E(u_1^2) - E(u_2^2) \\ &\quad - 2E(u_1 u_2) - 2\sigma_e^2 + 2E(u_1)E(u_2) \} \\ &= \sigma_e^2. \end{aligned} \quad (17)$$

In (17), the expression on the left before the first equality sign involves only expected values of the observed responses and may be estimated from the data. The result will be a consistent<sup>14</sup> estimate of the quantity on the right after the last equality sign, namely, the amount of measurement error variance in the survey question,  $\sigma_e^2$ .

<sup>14</sup> Estimates obtained from the model, given the assumptions, meet the conditions required for consistency, i.e., asymptotic convergence in probability to the true values. See P. J. Dhrymes, *Econometrics* (New York: Harper and Row, 1970), pp. 112-13. Two violations of the assumptions seem probable. First, the fixed number of ordinal categories insures that the extreme opinions will always be observed with measurement error toward the center of the scale: a respondent with the true opinion, "strongly agree," can be observed erroneously only by recording him as lower in agreement than he is. The reverse is true of those with opinion, "strongly disagree." Hence a negative correlation between opinion and observation error exists. With five categories available for the policy questions, however, and seven for party ID, this error is likely to be slight. In fact, tests were run in which the response categories for these questions were collapsed to three, which should have exacerbated the problem. Instead, the final results of applying the model were virtually identical.

Second, because respondents with extreme opinions have measurement errors in only a single direction,

Note that if Assumption Set 2 is incorrect, and we omit it, the sole change will occur in the last line of (17), which will then read:

$$\sigma_e^2 = E(u_1 u_2) + E(u_1)E(u_2). \quad (18)$$

If, as argued above,  $u_1$  and  $u_2$  are probably positively correlated assuming they are correlated at all, then in (18),  $E(u_1 u_2) > E(u_1)E(u_2)$ , and therefore the expression in (18) will be less than  $\sigma_e^2$ . Hence if we mistakenly believe that Assumption Set 2 is true and take (18) as an estimate of  $\sigma_e^2$ , the result will be an underestimate of  $\sigma_e^2$ . As noted above, this is an error in Converse's favor: it estimates less measurement error than is actually the case (and hence more respondent variability).

It is possible to replace the postulate in Assumption Set 2 with one of several other assumptions. Two other particular postulates were tried in its place. The first stipulated that  $u_1$  and  $u_2$  were independent with  $p_1$  controlled:  $r_{u_1 u_2 \cdot p_1} = 0$ . Verbally, this postulate means that although opinion change between times 1 and 2 may be correlated with the change between times 2 and 3, the relation is due solely to differences in respondents' original positions at time 1. Liberals may change their views in one way and conservatives in another, but within each group, opinion change consists of random shocks.

The second postulate tried was that  $p_1$  and  $u_2$  were independent with  $u_1$  controlled:  $r_{p_1 u_2 \cdot u_1} = 0$ . This assumption means that liberals and conservatives may change their views differently in the first time interval, but if the direction of this first change is allowed for, then there will be no relationship between liberalism/conservatism and opinion change in the second time interval.<sup>15</sup>

the amount of possible error depends on the distribution of voters' opinions. Hence the assumption in the model of constant error variance implicitly assumes constant voter distributions. Distributions at the three different time periods, however, were so nearly similar for all questions that this difficulty was ignored. I am indebted to John Aldrich for bringing the latter point to my attention.

<sup>15</sup> It is not possible to assume that  $p_1$  and  $u_2$  are independent without  $u_1$  controlled, though that is a more obvious assumption. Results (3)–(9) may be used to show that

$$E(x_2 x_1) - E(x_2 x_1) = E(p_1 u_2) \quad \text{and} \\ E(x_1) E(x_3 - x_2) = E(p_1) E(u_2).$$

Strict independence of  $p_1$  and  $u_2$  requires  $E(p_1 u_2) = E(p_1)E(u_2)$  and therefore also  $E(x_2 x_1) - E(x_2 x_1) = E(x_1)E(x_3 - x_2)$ . Inspection of the data showed this to be consistently false, but even if it had been true, the fact that the value of  $E(p_1 u_2)$  can be derived from (3)–(9) means that the new assumption adds nothing to the model. Statistically speaking,  $E(p_1 u_2)$  is "identified" without Assumption Set 2 or any of its substitutes.

When the first of these substitute assumptions was tried in place of Assumption Set 2, the result was estimates of  $\sigma_e^2$  that were within 1 per cent of those obtained under Assumption Set 2 for seven of the ten attitudes under study, and within 2 per cent for nine of ten. The tenth, the issue of keeping American soldiers overseas, differed by 5.4 per cent, indicating more measurement error than under Assumption Set 2. Under the second of the substitute assumptions, it was often impossible to obtain any results: given the data, the model was self-contradictory in four of ten cases. When estimates of  $\sigma_e^2$  were obtained, they typically deviated from those obtained under Assumption Set 1 by 5–15 per cent and were not consistently higher or lower. In short, neither alternate assumption dramatically altered or improved upon the estimates from the simpler Assumption Set 2. All results reported here are therefore based upon the original assumption.

If Assumption Set 2 is provisionally accepted, then the conclusion in (17) allows us to correct Converse's over-time correlation coefficients for attenuation due to measurement error. The approach parallels that used in psychological test theory.<sup>16</sup> First, the reliability of a measurement is defined, as usual, as the squared correlation between the observed and true scores or, equivalently, as the correlation between two observed scores taken at different times when true scores are unchanged. In either case, reliability at time  $t$ , denoted  $r_{tt}$ , has the following interpretation:

$$r_{tt} = \frac{\text{variance of true scores}}{\text{total observed variance}} \\ = \frac{\text{variance of true scores}}{\text{variance of true scores} + \text{error variance}}$$

<sup>16</sup> See, for example, Frederick M. Lord and Melvin R. Novick, *Statistical Theories of Mental Test Scores* (Reading, Massachusetts: Addison-Wesley, 1968). The approach set out in the text also bears a resemblance to that developed in David E. Wiley and James A. Wiley, "The Estimation of Measurement Error in Panel Data," in *Causal Models in the Social Sciences*, ed. H. M. Blalock (Chicago: Aldine-Atherton, 1971), pp. 364–74. The models were developed independently, however, and for different purposes; the Wileys have ratio scale data and their results depend on the existence of a meaningful zero point, while we have ordinal or interval level information only. The assumptions and computations involved are therefore somewhat different. The Wileys' article is also important for its statistical critique of David R. Heise, "Separating Reliability and Stability in Test-Retest Correlation," *American Sociological Review*, 34 (February, 1969), 93–101, which is reprinted in the Blalock volume, pp. 348–63.

Thus the reliability of a measure is simply the percent of its variance that is *not* errors of observation.

In psychological testing, reliabilities are typically obtained by correlating the two halves of a test or by correlating the performance of subjects with themselves when they are retested a short while after taking the test the first time. Neither method is available here: we are dealing with single questions only, and with time periods of two years, during which some genuine opinion change may be expected to occur. Instead, reliabilities must be derived from the evidence at hand. Taking note of the definition of  $r_{it}$  as applied to the model set out above, we have:

$$r_{it} = \frac{\text{var}(p_i)}{\text{var}(x_i)} = \frac{\text{var}(x_i) - \sigma_e^2}{\text{var}(x_i)} \quad (19)$$

This last expression may be estimated from the data, using (17).

Given an observed correlation,  $r_{ts}$ , between two measures,  $t$  and  $s$ , along with their reliabilities,  $r_{tt}$  and  $r_{ss}$ , test theory may be used to find the value the correlation would take on in the absence of measurement error. Call this "corrected correlation"  $r_{ts}^*$ . Then as usual:

$$r_{ts}^{*2} = \frac{r_{ts}^2}{r_{tt}r_{ss}} \quad (20)$$

Thus, Converse's correlation coefficients can be corrected to allow for the presence of measurement error arising from the vagueness of the questions.

In addition to Converse's survey questions, one other item from the same panel study is included in the results presented below. The question concerned frequency of church attendance. We have strong expectations about the actual stability of the attitudes measured by such a question, and therefore it may be used as a test of the model. We expect that most people's church attendance is quite stable and that therefore true over-time correlation coefficients for church attendance are very high. In fact, however, the observed correlations are relatively modest—between .60 and .70. Such correlations mean that less than half the variance is "explained" by previous behavior, a result for this question that seems implausible.

One is led to suspect that the correlations for this question are depressed by measurement error, i.e., by the ambiguity of the question. Indeed, while the question about church attendance is straightforward enough, its response categories are far from precise—"regularly," "often," "seldom," and "never." If the model postulated

above is accurate, therefore, we expect it to encounter a good deal of measurement error on this question. Furthermore, corrected correlation coefficients should be quite high. Success with this test question should suggest that the results obtained for Converse's original survey question are accurate.

### Estimation

The outcome of the application of the model to Converse's items and the test question may be seen in Table 1. For reasons that will become apparent below, 382 individuals for whom certain items of background data were unavailable were dropped from the study. The remaining group defines the "total sample" used here, 1132 respondents. All correlations for each issue were based on that part of the sample having an opinion on the issue in all three time periods. The correlations are Pearson  $r$ 's based on coding responses on a scale of 1 to 5 for the five responses "strongly agree," "agree," "it depends, not sure," "disagree," "strongly disagree."

Similar actions were taken for the party identification and church attendance variables, which have seven and four response categories, respectively. The correlation coefficients are Pearson product-moment coefficients, rather than Converse's tau- $b$ 's since the tau- $b$  does not lend itself to the sort of analysis undertaken in the text. The Pearson  $r$ 's were computed by treating the variables as interval-level measures. Such an approach is not unreasonable in cases like these.<sup>17</sup>

The variables being correlated are not normally distributed, hence the distributions of the correlation coefficients in the text are unknown. Coefficients from different questions therefore cannot necessarily be meaningfully compared, since the underlying distributions of opinions may differ. Coefficients are best interpreted as referring strictly to the percentage of variance of one variable linearly explained by the other. That interpretation is sufficient to the substantive points being made.

It would be preferable to cast the entire debate over attitudinal stability in terms of regression coefficients rather than correlations, which would eliminate the problems of the preceding paragraph. Tradition is in the opposite direction, however, and in any case, the variance of each attitude is so stable across time periods that regression and correlation coefficients are virtually identical.

<sup>17</sup> See Robert P. Abelson and John W. Tukey, "Efficient Conversion of Non-Metric Information into Metric Information," in Tufté, ed., *The Quantitative Analysis of Social Problems*, pp. 407-17. For exact statements of the attitude questions, see Converse, "The Nature of Belief Systems in Mass Publics," footnote 21.

Table 1. Pearson Correlations (*r*) Between Attitudes in Converse's Panel Study, Time Periods 1, 2, 3<sup>a</sup>

Attitude	Observed <i>r</i> 's			Corrected <i>r</i> 's			Reliabilities		
	1-2	2-3	1-3	1-2	2-3	1-3	1	2	3
Federal Assistance to Negroes ( <i>N</i> =712, $\sigma_e^2=0.966$ )	.51	.51	.49	.95	.98	.93	.55	.52	.51
School Integration ( <i>N</i> =730, $\sigma_e^2=1.430$ )	.45	.55	.42	.80	.96	.76	.54	.57	.57
Isolationism ( <i>N</i> =761, $\sigma_e^2=1.160$ )	.37	.46	.35	.79	.96	.74	.46	.49	.48
Foreign Aid ( <i>N</i> =676, $\sigma_e^2=1.120$ )	.32	.44	.31	.72	.99	.71	.44	.44	.44
Maintain Army Overseas ( <i>N</i> =630, $\sigma_e^2=0.781$ )	.31	.37	.25	.63	.81	.49	.55	.44	.47
Federal Aid to Education ( <i>N</i> =784, $\sigma_e^2=1.050$ )	.48	.45	.43	1.00	.85	.88	.44	.52	.55
Guaranteed Jobs ( <i>N</i> =772, $\sigma_e^2=1.316$ )	.46	.47	.42	.94	.96	.89	.48	.51	.47
Government Intervention in Housing & Electricity ( <i>N</i> =546, $\sigma_e^2=1.567$ )	.37	.37	.37	.97	.98	.94	.39	.37	.39
Party Identification ( <i>N</i> =986, $\sigma_e^2=0.567$ )	.82	.86	.80	.94	.97	.91	.88	.88	.88
Church Attendance ( <i>N</i> =989, $\sigma_e^2=0.244$ )	.70	.64	.60	.94	.84	.79	.75	.74	.78

<sup>a</sup> Total *N*=1132. The total sample was defined as those individuals in the panel study for whom certain background data were available (see text). Correlations for each attitude were based on the percentage of the sample having an opinion in all three time periods.

(Note, however, that this identity will not hold for Table 5.)

In the case of the federal aid to education question, the initial estimate of  $\sigma_e^2$  resulted in a corrected correlation coefficient between time periods one and two of 1.057. Ordinary statistical variability seems the most likely cause of the misestimate, but the assumptions of the model may be inappropriate to this question as well. In any event, the estimate of  $\sigma_e^2$  was then constrained so that no corrected coefficient exceeded 1.00 and  $\sigma_e^2$  was adjusted downward by 4.7%. It is the results from the latter estimate which appear in the text. This adjustment, which has disturbed several readers, is the standard approach to a common problem: by sheer chance, some near-zero positive quantities may have negative estimates if the statistical procedure is not constrained to be positive. Such constraints are typically used, for example, when estimating multiple correlations corrected for the number of independent variables and in variance component estimation. Here the difficulty is with the variance of true opinion between times 1 and 2, which has a negative estimate. Forcing it to be zero produces the constrained estimate.

### Results

As may be readily seen, the model gives a rather different picture of the mass public than does Converse's analysis. Here the problem with

the weak original correlations is demonstrated to lie, not with the variability of respondents, but rather with the fuzziness of the questions and with other errors of measurement. Low reliability in the questions constitutes most of the variance.<sup>18</sup> Interestingly, party ID is seen to have higher initial correlations more because of its greater intelligibility as a question rather than because respondents are more stable with regard to it.

Results for the test question are precisely as expected. The reliability of the church attendance measure is found to be rather low. Its corrected correlation coefficients are high, in accord with prior expectations. Thus, some confidence about the results for Converse's questions is justified.

On each policy question, about two thirds of the respondents express an opinion at all three time periods, and only they were included in the sample for that question. Strictly speaking, the conclusions derived from the estimation proce-

<sup>18</sup> Interestingly, some recognition of the weakness of the questions appears in Converse himself: he notes in "The Nature of Belief Systems in Mass Publics," footnote 21, that his questions had to be revised when presented to congressmen, since their simplistic wording made responses so difficult. The difference in survey questions put to congressmen and to the mass public reduces the impact of another of Converse's findings, that correlations among attitudes are higher for congressmen than for ordinary citizens. It is well known that small changes in the wording of survey questions can make a considerable difference in response patterns.

Table 2. Percentage of the Total Sample with No Opinion in any Time Period

Federal Assistance to Negroes	1.6%
School Integration	1.4
Isolationism	2.0
Foreign Aid	2.4
Maintain Army Overseas	4.2
Federal Aid to Education	1.0
Guaranteed Jobs	1.1
Government Intervention in Housing and Electricity	7.1
Party Identification	1.5
Church Attendance	0.0

dures apply only to them. However, it should not be thought respondents in the omitted third of the sample are entirely without opinions. A check of half-a-dozen attitude questions showed no consistent tendency for correlations between attitudes at different time periods to drop when respondents without an opinion at the other time period were added to the rest of the sample. In short, a respondent could describe himself as having no opinion at one of the time points and still give at the other two time periods responses that were far from random. Furthermore, only a handful of respondents were without an opinion at all three time periods. (See Table 2.)

Nevertheless, at this point the question may be raised: How is it possible that respondents find the questions so vague? Granted that none is very precise, yet the analysis indicates that the amount of ambiguity in voters' minds is substantial. Might it not be that this "measurement error" is really a simple matter of not understanding the question? After all, the high over-time correlations above indicate that voters find the questions vague, nothing more. If simple policy questions remain terribly confusing to the respondents, then these citizens can hardly be said to be sophisticated.

This argument appears to be strengthened when we examine the reliabilities of each of the attitudes measured. The questions about party identification and church attendance are quite reliable, while the policy items are much less so. This might mean that voters do not understand the policy questions.

The difficulty with this argument is that it provides no evidence by which to choose between the competing explanations. One may observe individuals having difficulty with questions and blame either the individuals or the questions. The usual criterion, however, for distinguishing between complexity and vagueness is the judgment of someone acquainted with the material. Applying that notion to the survey questions presumably means inquiring whether more sophisticated members of the electorate had noticeably less measure-

ment error in their responses than did other members of the citizenry. The model set out above must therefore be modified to allow for differences in citizens' having differing amounts of measurement error in their responses. If these differences are large, then "measurement error" probably means "failure to understand"; if small, then everyone finds the questions difficult, and the fault lies with the questions themselves.

Converse himself discusses a Markov chain model to estimate the attitudinal stability of various parts of his sample population, but because he allows only for respondent variability and not for measurement error of any sort, he is soon reduced by the implications of the data from at least one survey question to the assertion that the entire population is divided into two groups—a small segment who are perfectly consistent in their responses and the remainder, who are perfectly random.

One way to avoid this rather unlikely conclusion would be to use the model set out above to estimate the measurement error for each of a number of subgroups in the population. Respondents could be divided on the basis of their education, their social class, or other background factors. Estimated measurement errors could then be compared, and a conclusion reached on that basis.

A difficulty with this approach is that respondents can be divided into no more than a few groups before sample sizes become so small as to make the model's statistical estimates useless. This means that no estimates can be made of the joint effect of all background factors or of their separate independent effects, with other factors controlled.

A better procedure is to use regression techniques. By making estimated measurement error the dependent variable and the background factors the independent variables, the impact of the background factors, taken individually and jointly, may be easily obtained. One may then examine the degree to which the better-off members of the electorate (the better-educated, more prosperous, etc.) have lower expected measurement errors.

For each survey question, the following simple additive regression equation was estimated:

#### Dependent Variable

Respondent measurement error variance:<sup>19</sup>

$$\sigma_{ei}^2 = \frac{1}{2} \{ E(x_{2i} - x_{1i})^2 + E(x_{3i} - x_{2i})^2 - E(x_{3i} - x_{1i})^2 + 2E(x_2 - x_1)E(x_3 - x_2) \}.$$

<sup>19</sup> This expression can be simplified a good deal for computational purposes, but it is listed here as it appeared in the derivations from the model, where its form eased the exposition.

where the subscript  $i$  refers to the respondent in question. (See (17).) Since we have just one observation on each  $x_{1i}$ ,  $x_{2i}$ , and  $x_{3i}$ , we must estimate the expected value of any expression involving them by the one observation available. Thus  $E(x_{2i} - x_{1i})^2 = (x_{2i} - x_{1i})^2$ , and so forth. Note, however, that we assume  $E(x_2 - x_1)$  and  $E(x_3 - x_2)$  to be the same for all respondents and estimate them from the entire sample. Without this assumption, the expression in brackets above requires subscripts in the last term, and in that case, the expression is identically zero.

**Independent Variables** (taken from the 1958 section of the SRC panel study):

1. City dweller (scored 1 if inhabitant of city with population exceeding 100,000 or its suburbs, 0, otherwise).
2. Southerner (scored 1 if living in South, 0, otherwise).
3. Rural (scored 1 if respondent lives in place of under 10,000 population, 0, otherwise).
4. Concern (a five-point scale recording the respondent's expressed concern about who wins the 1958 congressional election—recoded so that higher values indicate more concern).
5. Financial situation (a three-point scale for the respondent's evaluation of his current financial situation—higher values are pessimistic).
6. Interest in campaign (a three-point scale recording the respondent's stated interest in the 1958 congressional campaigns—recoded so that higher values indicate more interest).
7. Talked to others (Did the respondent talk to other people to influence their vote? Scored 3 if yes, 1 if no).
8. Male (scored 1 if male, 0 if female).
9. Age (age in years).
10. Education (the SRC's nine-point scale—higher values indicate more education).
11. Income (the SRC's ten-point scale—higher values indicate more income).
12. Occupation (the SRC's nine-point scale—recoded so that higher values indicate higher occupational status).

Since only a few respondents answered "don't know" or "no answer" on the variables "concern," "financial situation," and "talked to others," such responses were recoded to the midpoint of their scale, to which they seemed to correspond. Similar answers for "interest in campaign" were recoded to the category indicating least interest for the same reasons. For all other variables, including the dependent variable in each regression, respondents with missing data were eliminated from the sample.

This regression, when performed for each of the survey questions, yields quite modest multiple correlation coefficients. (See Table 2.) If we consider only the eight policy questions, for example, the multiple  $R$  ranges from 0.11 to 0.19. This

means that between 1 per cent and 4 per cent of the variance in respondents' estimated measurement errors is "accounted for" by differences in background factors. In addition, a total of seven regression coefficients of the ninety-six involved are significant at 0.05, which is just two more than would be expected by chance. If we simultaneously test the significance of the entire set of variables for each question, the result is significant at the 0.05 level in just three of eight cases. Results like these are the traditional indications that our independent variables do not predict measurement error well at all: the concerned, interested, and well-educated respondents perceive virtually as much ambiguity in the policy questions as do the less involved.

In fact, however, multiple  $R$ 's and significance levels provide very little statistical evidence for the hypothesis in question (or, indeed, for most social science hypotheses). We are concerned with whether the independent variables have a sizable impact on the dependent variables, that is, with whether the true regression coefficients are large enough to be consequential. Neither multiple correlations nor significance tests will answer that question.<sup>20</sup> The relevant statistics are instead the estimated regression coefficients and their standard errors.

Table 4 presents the full results of the regression for the Foreign Aid policy question, which was chosen for display because it falls approximately at the median in "predictability" using either multiple  $R$  or standard error of the residual as criterion. Two aspects of the results are apparent. First, those factors that one would expect to increase voter comprehension typically have coefficients in the expected direction: taking an interest in the campaign, caring who wins, talking to others about it, higher education, income, and occupational status all act to reduce the perceived vagueness of this policy question. Similarly, apparently irrelevant variables like Southerner, City

<sup>20</sup> The problem is particularly acute here because the dependent variable in the regression equation is itself a statistical estimate and incorporates a certain amount of measurement error. This will repress both the multiple correlations and the significance levels of the regression coefficients. Both are so low, however, that even very substantial upward revision would not alter the conclusions in the text. In addition, neither the regression coefficients nor the predicted values graphed in Figures 2 and 3 are biased by measurement error, and both of them indicate that the differences among the population in their understanding of the survey questions is slight. Unfortunately, the unknown distributional form of true and observed opinions and the single observation on measurement error variance per respondent make it impossible to estimate the size of the measurement error variance in the dependent variable without additional assumptions.

Table 3. Regression Results<sup>a</sup>

Issue	Multiple <i>R</i>	Significant (at 0.05) Coefficients (with Standard Error)		Is Overall Regression Significant (at 0.05)?
Federal Assistance to Negroes ( <i>N</i> =712)	0.155	Male	0.427 (0.215)	No
School Integration ( <i>N</i> =730)	0.106	None		No
Isolationism ( <i>N</i> =761)	0.132	None		No
Foreign Aid ( <i>N</i> =676)	0.153	Interest	-0.683 (0.330)	No
Maintain Army Overseas ( <i>N</i> =630)	0.191	Financial situation	0.800 (0.300)	Yes
		Education	1.115 (0.461)	
Federal Aid to Education ( <i>N</i> =784)	0.185	Concern	0.690 (0.342)	Yes
		Talked	-0.747 (0.284)	
Guaranteed Jobs ( <i>N</i> =772)	0.175	Education	1.337 (0.589)	Yes
Government Intervention In Housing and Electricity ( <i>N</i> =546)	0.123	None		No
Party Identification ( <i>N</i> =486)	0.133	Concern	-0.613 (0.270)	No
Church Attendance ( <i>N</i> =989)	0.147	Age	-0.244 (0.112)	Yes

<sup>a</sup> The dependent variable in each regression is the estimated measurement error variance for each respondent on the policy question. To facilitate comparison of coefficients, all independent variables in the regressions are standardized to a range of 0-1. The variable Age is standardized by setting 0=20 years and 1=70 years. Regressions are based solely on those respondents with an opinion on the policy question in all three time periods.

Dweller, and Financial Situation have little or no effect. Most of the standard errors are large (relative to the coefficients); but, and this is the second point, the usual 95 per cent confidence region for most of them falls in the range  $\pm 1.0$ , and none falls outside the range  $\pm 1.6$ . Thus, even if we are quite unlucky in our coefficient estimates, the impact of these independent variables remains relatively small.

For example, suppose that our coefficient for education is wrong by two full standard deviations, and suppose further that we change an otherwise average respondent from having graduate school training (education=1) to having no formal schooling (education=0) while holding all else constant. Now perhaps the best intuitive measure of the "average perceptual error" of a respondent is the estimated standard deviation of his measurement error, which may be regarded as the amount by which he typically errs in perceiving the question. This measure is just the square root of the predicted dependent variable in the regression. Using this measure, we find that our respondent's average perceptual error rises with his

absence of schooling from about 1.0 to about 1.5. In short, under the worst possible assumptions, the loss of twenty years of education results in an average vagueness of a unit and a half instead of a single unit on the five-point scale of the policy questions. Most of the other variables in the regression equation have smaller effects than this, and *at worst*, none results in an average perceptual error above 1.6 units in this example.

Even taken all together, the effect of the independent variables is quite modest. This is perhaps most easily seen in a graph of the expected values of the dependent variable (the square of the "average perceptual error") as predicted by the regression equation. The two attitudes graphed are (1) Maintain Army Overseas, whose measurement error is best predicted by the background factors, (highest *R* and lowest standard error of the residual) and (2) School Integration, whose measurement error is predicted worst (lowest *R* and highest standard error). Respondents are weighted by their SRC sampling weights in the graphs to obtain a more representative sample.

The graphs show that there are probably some



Table 4. The Complete Foreign Aid Regression<sup>a</sup>

Independent Variables	Regression Coefficients (Standard Error)
City dweller	-0.062 (0.328)
Southerner	-0.033 (0.293)
Rural	-0.011 (0.275)
Concern	-0.194 (0.368)
Financial situation	0.045 (0.330)
Interest in campaign	-0.683 (0.330)
Talked to others	-0.304 (0.296)
Male	-0.392 (0.229)
Age	0.678 (0.450)
Education	-0.184 (0.504)
Income	-0.280 (0.590)
Occupation	-0.361 (0.563)

Intercept = 2.119.  $R = 0.153$ .  $N = 676$ . Standard error of residual = 2.910.

<sup>a</sup> The dependent variable is the estimated measurement error variance for each respondent on the Foreign Aid policy question. All independent variables are standardized to a range of 0-1 to facilitate comparison. The variable Age is standardized by setting 0 = 20 years and 1 = 70 years.

differences among subgroups in the population in perceived vagueness of the questions, especially on the Army Overseas question. Measurement error is partly a matter of comprehension by the respondent. What is impressive about the graphs, however, is the extent to which this is not true, the extent to which the subgroups cluster around intermediate values of measurement error. There is no evidence here of Converse's select group of sophisticates divided from the mass of the population. Thus the measurement error in the questions is not just another form of the ignorance Converse originally claimed to have found. The well-informed and interested have nearly as much difficulty with the questions as does the ordinary man. *Measurement error is primarily a fault of the instruments, not of the respondents.*

From this perspective, it is clear that some of Converse's other arguments must be revised as well. He showed that attitudes on domestic policy, on racial matters, and on foreign policy were correlated with other attitudes in their respective

areas to form dimensions of policy thinking.<sup>21</sup> Taken at face value, the correlations appeared weak: Converse therefore argued that respondents show little ideological coherence. From the present perspective, with the knowledge that survey questions incorporate a great deal of measurement error, it is clear that intercorrelations among attitudes would rise dramatically if the attenuation due to measurement error were corrected. The expression in (20) applies.

Table 5 presents the Pearson intercorrelations among attitudes obtained before and after the appropriate correction for measurement error is made. Although the intercorrelations are not uniformly large (indeed, they probably should not *all* be large), the result is again a rather different picture of the coherence of voters' political thinking.<sup>22</sup>

### Summary and Conclusion

Two possible sources of weak correlation coefficients among citizens' political survey responses were distinguished: (1) the instability of a voter's political attitudes and (2) the low reliabilities of opinion survey questions. A statistical model designed to separate these two sources of response instability was put forward and applied to Converse's data. Rather weak reliabilities for the survey questions were obtained. When the correlations among attitudes were corrected for this unreliability, the result was a sharply increased estimate of the stability and coherence of voters' political thinking.

Care should be exercised in interpreting these results. The model implies that voters are stable in their thinking, but it says nothing about their wisdom. A devotee of exotic conspiracy theories of political life may have coherent and stable (indeed, rigid) political views, and yet not be a

<sup>21</sup> Converse, "The Nature of Belief Systems in Mass Publics," pp. 228-29.

<sup>22</sup> In order to maximize sample sizes, estimates of reliabilities are based on the per cent of the sample with attitudes on the variable in question at all three time periods, while intercorrelations are based on those 1958 respondents who had an opinion on both the variables correlated. A corrected correlation, which is a function of two reliabilities plus an intercorrelation, thus depends upon three different samples of respondents. If some of the parameters being estimated differ among these samples, errors will be introduced. As one would expect, however, the three samples always overlap heavily, and the errors introduced are almost surely negligible.

These relatively high intercorrelations make it unlikely that the large over-time correlations found earlier are due to response biases in favor of "agree" or "disagree" answers. To give liberal answers on both the civil rights questions, for example, respondents had to agree to one and disagree with the other. The same is true for the Foreign Aid and Isolationism questions. Yet these two pairs of issues had the highest corrected  $r$ 's.

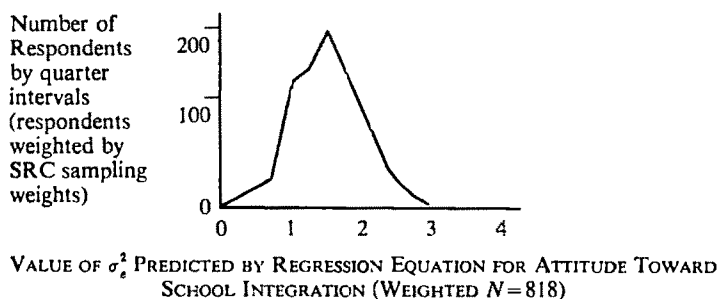
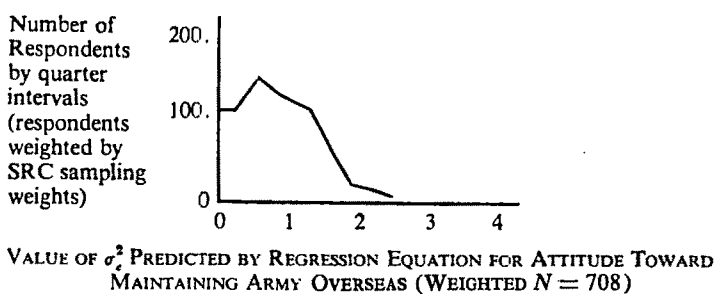


Figure 2a



$\sigma_e^2$  is the respondent's measurement error on a policy question. It may be interpreted as the square of the "average perceptual error" of a respondent, measured on the five-point scale used for the responses to the policy questions.

Figure 2b

Table 5. Observed and Corrected Intercorrelations among Attitudes in Converse's Panel Study, 1958

	Observed $r$ 's			Corrected $r$ 's	
	Federal Aid to Education	Government Intervention in Housing and Electricity		Federal Aid to Education	Government Intervention in Housing and Electricity
<i>Social Welfare Issues</i>					
Guaranteed Jobs	.37	.06	Guaranteed Jobs	.72	.14
Federal Aid to Education	—	.14	Federal Aid to Education	—	.33
	Foreign Aid	Maintain Army Overseas		Foreign Aid	Maintain Army Overseas
<i>Foreign Policy Issues</i>					
Isolationism	.34	.20	Isolationism	.73	.43
Foreign Aid	—	.12	Foreign Aid	—	.27
	School Integration			School Integration	
<i>Civil Rights Issues</i>					
Federal Assistance to Negroes	.45	Federal Assistance to Negroes		.82	

very well-informed or sophisticated citizen. A certain stability of viewpoint is a necessary, but hardly sufficient, condition for political understanding. If voters lack other, more subtle features of conceptual sophistication, the stability of their opinions will be cold comfort to the democratic theorist.<sup>23</sup>

The conclusions derived from the model are perhaps most important for what they recall to mind about survey methodology. Public opinion research depends heavily upon the survey response for evidence. While a great deal of progress has been made in its understanding, the nature of that response is by no means fully comprehended, and substantive work is bedeviled by doubts about aspects of the measuring instruments that remain ill-formulated theoretically—response set, scaling errors, interviewer bias, etc. If a researcher has no idea of the size of these errors, as is often

<sup>23</sup> See W. Lance Bennett, "Political Attitudes and Social Life" (Ph.D. dissertation, Yale University, 1974), who argues that uninformed citizens may develop stable opinions precisely because of their lack of information: they become subject to symbolic appeals by political leaders.

the case, he has little choice but to assume that they are small and then proceed, treating the observed responses as direct measures of the underlying attitudes. The present results remind us that the errors involved may be substantial.

As the research undertaken in public opinion grows in complexity, methodological uncertainty becomes increasingly corrosive of substantive results. The greater the distance from data to conclusions, the more opportunity for error. Major improvements in our understanding of political thinking may therefore come to depend upon a considerably more advanced theoretical knowledge of our measuring instruments than we have yet mustered.<sup>24</sup>

<sup>24</sup> Two interesting attempts along these lines, using Markov chains, are James S. Coleman, *Models of Change and Response Uncertainty* (Englewood Cliffs, N.J.: Prentice-Hall, 1964); and Ralph B. Ginsberg, "Critique of Probabilistic Models: Application of the Semi-Markov Model to Migration" and "Incorporating Causal Structure and Exogenous Information with Probabilistic Models: With Special Reference to Choice, Gravity, Migration, and Markov Chains," *Journal of Mathematical Sociology*, 2 (January, 1972), 63-82 and 83-103, respectively.

# The Effect of Aggregate Economic Variables on Congressional Elections\*

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Public officials, many academic experts, and the press consider aggregate economic conditions to be important determinants of election outcomes. Low unemployment and a low rate of inflation are sometimes said to favor incumbents, and high rates of inflation and unemployment to favor challengers. Others suggest that high or rising unemployment favors the Democrats while high or rising inflation favors the Republicans. Attempts to test these propositions, in one or another form, can be found in social science journals for the past forty years.<sup>1</sup>

The importance placed on aggregate economic variables by politicians and their advisers is well known. The chances of victory for Richard Nixon in 1972 are said to have increased greatly following the wage and price freeze and effective devaluation of the dollar in August 1971. At least one former chairman of the Council of Economic Advisers accepts the view that aggregate economic performance has an important influence on voting decisions.<sup>2</sup> There seems little reason to doubt that policy makers attempt to achieve particular levels or ranges for economic aggregates to win votes, even if the post-election consequences are unfavorable. Less certain is the effectiveness of these measures in congressional elections.

\* We are indebted to Jay Kadane and Timothy McGuire for helpful comments, to Gerald Kramer for finding errors in our data, and to the National Science Foundation for financial support. An earlier version was presented at the 1973 meeting of the American Political Science Association.

<sup>1</sup> A review of much of this literature can be found in Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964," *American Political Science Review*, 65 (March, 1971), 131-143; and George J. Stigler, "General Economic Conditions and National Elections," *American Economic Review*, 63 (May, 1973), 160-167.

<sup>2</sup> Arthur M. Okun, "Comments on Stigler's Paper," *American Economic Review*, 63 (May 1973), 172-177. In fact, our interest in the subject started in 1970 after a discussion one of us had with Herbert Stein, then a member of the Council of Economic Advisers. Stein argued that the outcome of the 1972 election would depend on the ability of the administration to reduce inflation and aggregate unemployment to a specific range. A different view is expressed by former Chairman Paul W. McCracken in "The Practice of Political Economy," *American Economic Review*, 63 (May, 1973), 168-171.

Several problems prevent acceptance of the argument and the evidence. Prior to Kramer's study, much of the work was done within a loose and rather ad hoc framework that leaves doubt about the proposition tested.<sup>3</sup> Kramer places his study within the context of a model of rational behavior, but as Stigler has shown, the evidence he presents depends very much on the choice of time periods.<sup>4</sup> Moreover, Kramer's study shows at most, that rising real income benefits incumbents. "With real income held constant, change in unemployment or in the rate of inflation have no significant independent effects."<sup>5</sup> A careful study of British opinion polls shows significant but variable influence of inflation and unemployment on voters' opinions of the popularity of the parties.<sup>6</sup> Less clear is whether parties are able to translate sentiment into votes.

There are two main problems with the argument that the choices made by rational voters are significantly influenced by the current position on aggregate economic variables. One is that a voter's opportunity set includes more than a choice between rival candidates. He can—and many voters do—choose not to vote. Even casual examination of voting statistics suggests that changes in the participation rate of eligible voters often is larger than changes in the share of votes received by the major parties. Is it less rational for a voter to abstain than to shift party loyalty? Two, any set of economic policies gives rise to expectations about future consequences. Recent experience reminds us that government economic policies have such long-term consequences. Do voters ignore the prospect that the policies that increase employment just before the election may increase inflation after the election? Are voters aware that action to reduce inflation before the election may reduce employment after the election? Do opposition candidates fail to make this point?

<sup>3</sup> This position is taken by Stigler, and by William H. Riker, "Discussion," *American Economic Review*, 63 (May, 1973), 178-179.

<sup>4</sup> Stigler.

<sup>5</sup> Kramer, "Short-term Fluctuations," p. 141.

<sup>6</sup> See C. A. E. Goodhart and R. J. Bhansali, "Political Economy," *Political Studies*, 18 (1970), 43-106.

This paper combines two approaches. One is the framework provided by rational decision making; the other is the classification of voters according to degree of partisanship found useful in the work of the Survey Research Center.<sup>7</sup> In the following section, we present a model of voting behavior in which members of the electorate decide whether to vote and for whom to vote. The model permits estimation of (1) the effect of changes in participation rate following the enfranchisement of women, (2) the effect of party realignment in the 1932 election, and (3) the independent effect of presidential elections on congressional elections. A discussion of the results and a conclusion complete the paper.

### A Model of Voting

Discussions of voting often emphasize the motivations that lead people to vote, to abstain, or to vote for the candidate of a particular party. Attitudinal and other psychological variables receive main emphasis, but family and group pressures are also considered important. Campbell and Flanigan offer excellent discussions of these relationships.<sup>8</sup> An implication of their work is that it is helpful to classify voters as strong or weak partisans or as independents.

Recent studies of political behavior have attempted to use the notion of rational decision making as an organizing device.<sup>9</sup> Applied to voting decisions, rational decision making is said to induce voting whenever expected utility is positive. Riker and Ordeshook summarized this position in a decision rule which can be written:<sup>10</sup>

$$(1) \quad R = PB - C + D.$$

<sup>7</sup> Angus Campbell, Philip S. Converse, Warren S. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), esp. chaps. 6 and 7.

<sup>8</sup> Campbell et al., chap. 4, and William H. Flanigan, *Political Behavior of the American Electorate*, 2nd ed. (Boston: Allyn and Bacon, 1972).

<sup>9</sup> Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957); James M. Buchanan and Gordon Tullock, *The Calculus of Consent: The Logical Foundations of Constitutional Democracy* (Ann Arbor: University of Michigan Press, 1962); Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: University of Michigan Press, 1967). William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25-42; Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), 1106-19; Richard D. McKelvey and Peter Ordeshook, "A General Theory of the Calculus of Voting," in *Mathematical Applications in Political Science*, Vol. VI, ed. James F. Herndon and Joseph L. Bernd (Charlottesville, Virginia: The University of Virginia Press, 1972).

<sup>10</sup> See Riker and Ordeshook.

$R$  is the expected utility of voting minus the expected utility of abstaining:  $PB$  is the difference in utility resulting from the victory of a particular candidate multiplied by the probability that the individual, by voting, causes a benefit,  $B$ .  $C$  is the cost of voting—the loss of utility from using resources to vote.<sup>11</sup>  $D$  includes other factors affecting utility, including the attitudinal and psychological variables emphasized by Lazarsfeld and Campbell, and the citizen's sense of duty.<sup>12</sup>

In this section, we consider the voter's decision as a two-step process, a decision to vote and a decision about the preferred candidate or party. The two decisions have common elements as well as differences, and they may be taken simultaneously or separately. We assume that both decisions depend on the costs and expected benefits summarized in equation (1) above.

**The Decision to Vote.** A citizen votes if the benefits exceed the costs. The principal cost of voting is a cost of acquiring information about parties, candidates and issues. The cost varies with the voter's commitment to a party or candidate. A strict partisan votes for the party candidates, votes regularly and gains utility from participation, from the affirmation of partisanship, and in some cases from a job or contract. Neither issues nor other factors affect a partisan voter's decision to vote or his choice of party. Perceived benefits—particularly  $B$  in equation (1)—dominate other factors, so benefits always exceed costs for members of this group. Included here are party officials, their friends and relatives, and any groups that tradition places firmly in one of the two major parties.

We assume that the fraction of the electorate that is "strict partisan" changes very little from one election to the next. Only major changes in cost or perceived benefits change the participation rate or party allegiance. The literature discusses two types of major changes during the years 1896 to 1970 included in our study.

One is the extension of suffrage to women in 1920. Prior to 1920, the calculation of net benefits is meaningless for women; after 1920, the same or similar factors affect women and men, although not always in the same way.

A second type of major change is a "realign-

<sup>11</sup> Tullock, p. 110-114, argues that in a typical election, there are many voters so  $P$  is small and  $R$  is negative. He concludes that voting is irrational. Riker and Ordeshook, p. 28-34 respond that  $P$  depends on the closeness of the election, not just the number of voters and also add  $D$  to the equation.

<sup>12</sup> See Campbell et al., also Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice*, 2nd ed. (New York: Columbia University Press, 1948). Riker and Ordeshook p. 28 discuss the "sense of citizen duty."

ment."<sup>13</sup> In our analysis, realignment occurs when there is a substantial change in costs or benefits. Since we distinguish between changes in participation and changes in party alignment, we can separate the effects of a realignment into (1) shifts from nonpartisan, or less partisan, to strict partisan behavior, and (2) shifts from one party to the other. Realignments are said to have occurred in 1896, at the start of our sample, and in 1932.

Let  $H_t$  be the proportion of habitual, partisan voters in an election held during a particular year  $t$ . Our discussion implies that

$$H_t = V_0 + \Delta V_{01} X20_t + \Delta V_{02} X32_t + e_{1t},$$

where

$$X20 = 1 \quad \text{if } t \geq 1920$$

$$X32 = 1 \quad \text{if } t \geq 1932$$

and both are zero elsewhere.  $V_0$  is the fraction of strictly partisan voters prior to 1920, and  $e_{1t}$  is a random variable representing the effect on partisans of weather, illness, and similar, non-systematic factors.

Voter participation in congressional elections increases in years of presidential elections but is never as high as the participation rate in the presidential election itself. A citizen's "sense of duty" may be greater when a president is elected. Also, the incremental cost of voting for a congressman is much lower once the voter decides to vote for president. The cost of forming an opinion about the merits of presidential candidates, however, is much lower than the cost of acquiring information about congressional candidates. The press, radio, television, the political parties and other groups are so much more active in the years of presidential elections that it is difficult to avoid exposure to information or partisan statements.

A second effect of presidential elections on voting for Congress is the effect of incumbency. Congressmen who are members of the President's party are more likely to support the President on partisan issues than members of the opposition. Presidential incumbency provides information about policies of congressional candidates, lowers the cost of voting and changes the expected net benefit of voting for Congress.

We use  $N_t$  to denote the percentage of voters induced to vote by the reduction in cost or increase in perceived benefits resulting from presi-

dential elections and incumbency.

$$N_{1t} = V_1 PR_t + \Delta V_1 RI_t PR_t + e_{2t},$$

where

$PR_t = 1$  if  $t$  is a presidential election year,

$RI_t = 1$  if the incumbent is a Republican,

and both are zero otherwise. An error term is included to allow for the effect of random factors affecting the proportion of the electorate in  $N_t$ .

None of the voters we have discussed is affected by aggregate economic variables or other specific issues. Some voters may be induced to vote because (1) they perceive a difference between candidates or parties on a particular issue, (2) they wish to protest against a particular policy or outcome, or (3) they wish to reward a party or candidate for a policy or outcome and encourage continuance. In each of these cases, the net benefit of voting and the probability of obtaining the benefit,  $PB$ , increase during the elections in which the issue arises.

The particular issue that concerns us is the effect of aggregate economic policies. To study the effect of these policies on an individual voter or a group, we have to distinguish between real and nominal income and to hold constant changes in real income. Nominal income is the value of income at current prices; changes in nominal income include all changes in prices. Measures of real income attempt to separate price changes from the quantity of goods and services received and to measure only the latter. Much of the reported unemployment in the United States is of short duration and arises because individuals voluntarily quit their jobs. It seems reasonable to expect voters to distinguish also between the prospect of being "laid off" as a result of government policies and changes in quit rates resulting from changes in the age composition of the population or other similar variables. It is reasonable also to expect voters to distinguish between "high" or "low" unemployment and rising or falling unemployment. Rising unemployment is likely to be accompanied by falling real income and reduced hours of work; high unemployment has no similar consequence for the bulk of the labor force or for voters. Quite apart from his social view, a voter is expected to be affected differently by the prospect of his own "lay-off" than by the unemployment of other members of the labor force.

Qualifications must also be made about the effects of inflation. Rational voters are concerned with their real income and real wealth, so their response to inflation depends on whether their incomes and purchasing power rise or fall with the price level. Inflation transfers wealth from

<sup>13</sup> Critical elections and the realignments produced by them have been discussed, among others, by Valdimer O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (1955), 3-18; Campbell et al.; and James L. Sundquist, *Dynamics of the Party System. Alignment and Realignment of Political Parties in the United States* (Washington, D.C.: The Brookings Institution, 1973).

reditors to debtors, and deflation induces the opposite flow. Within the private sector of the economy, private debtors gain what private creditors lose; there is redistribution but no net gain or loss. The principal net debtor in the U.S. is the U.S. government and its agencies, so the net effect of unanticipated inflation on the distribution of wealth is a transfer from the public to the government. Unanticipated inflation is a tax on creditors, in this case citizens or voters who are the government's creditors, for the benefit of debtors, including the government. The effect on voters should be similar to the effect of a tax with similar consequences for the distribution of income.<sup>14</sup>

To summarize, finding the effect of inflation, deflation or unemployment on voting requires, at a minimum, that we hold real income, or in an expanding economy the growth rate of real income, constant and that we analyze the effects of changes in the rate of inflation and the unemployment rate. Let  $N_2$  denote the proportion of voters for whom the net benefit of voting is a function of aggregate economic performance.

$$N_{2t} = a_1 \dot{p}_t + a_2 \dot{U}_t + a_3 \frac{\dot{C}}{p_t} + e_{3t},$$

where  $\dot{p}$ ,  $\dot{U}$ ,  $\dot{C}/p$  are percentage changes in consumer prices, the unemployment rate and real compensation per man-hour. The Appendix gives additional description of the variables and their measurement.

There are, finally, other election issues local as well as national. We postulate that such issues are randomly distributed over time, so we include them in the error term of equation (2) as  $e_{4t}$ .<sup>15</sup>

Our hypothesis is that the percentage of eligible voters participating in a congressional election,  $VP_t$ , is the sum of the components we have specified. Collecting terms in equation (2) gives the result

$$\begin{aligned} (2) \quad VP_t &= H_t + N_{1t} + N_{2t} + e_{4t} \\ &= V_0 + \Delta V_{01} X_{20t} + \Delta V_{02} X_{32t} \\ &\quad + V_1 PR_t + \Delta V_1 RI_t PR_t \\ &\quad + aE_t + e_t, \end{aligned}$$

<sup>14</sup> Further discussion of these points can be found in Martin J. Bailey, "The Welfare Cost of Inflationary Finance," *Journal of Political Economy*, 64 (April, 1956), 93-110.

<sup>15</sup> We made some efforts to see whether some other "national" issues had a significant effect on our results. We investigated the effect of war, the duration of war, the rate of change of farm prices and the rate of change of stock market prices, the latter as a possible indicator of future economic conditions. None of our preliminary results provided evidence that our measures of these events had a significant effect on congressional elections.

where

$$aE_t = a_1 \dot{p}_t + a_2 \dot{U}_t + a_3 \frac{\dot{C}}{p_t}$$

and

$$e_t = \sum_{i=1}^4 e_{it}.$$

**The Choice of Party Candidate.** The framework developed to analyze the decision to vote is applicable, with little change, to the choice of candidates in congressional elections. Denote by  $D_t$  and  $R_t$  the percentage of the total vote for candidates of the Democratic and Republican parties so that

$D_t \cdot VP_t = VD_t$  is the percentage of those eligible to register who vote for the Democratic candidate in election year  $t$ ,

and similarly for Republicans and third party voters ( $VT_t$ ). Summing, we have

$$VP_t \equiv VD_t + VR_t + VT_t$$

In this section, we obtain the share of the vote for the Democratic candidate by distributing each of the components of  $VP$  introduced in the previous section. The Republican share is the exact analogue. The third party vote is treated as a residual.<sup>16</sup>

The first two components of  $VP$  are strict party regulars,  $H$ , and participants in presidential (but not congressional) elections,  $N_1$ . The Democrats' share of these votes is the fraction  $\alpha_D H_t + \beta_D N_{1t}$ . Voters concerned with the performance of the economy,  $N_2$ , are permitted not only to respond to aggregate economic variables but to respond in a partisan way. We allow our measures of unemployment or inflation to be given a more or less favorable interpretation depending on the party holding power. Democrats obtain a fraction,  $\gamma_D$ , of  $N_2$  that varies with incumbency,

$$(\gamma_D + \Delta \gamma_D RI_t) N_2$$

If voters in  $N_2$  are nonpartisan,  $\Delta \gamma_D RI_t$  has no systematic effect on the Democrats' share. The fourth set of factors includes local issues and personalities and is assumed to be independent of

<sup>16</sup> Our definition differs from Kramer's, "Short-term Fluctuations," p. 136. He treats all minor party votes as votes against the incumbent on grounds that short-term fluctuations in voting express satisfaction and dissatisfaction with the performance of the incumbent. We believe this overstates the extent of change in public attitudes when there are changes in the party holding the presidency by shifting the average third party vote from one major party to the other.

aggregate economic variables and our measures of incumbency.

Combining terms yields an hypothesis for the Democratic vote

$$(3) \quad VD_t = d_0 + \Delta d_{01}X20_t + \Delta d_{02}X32_t \\ + d_1PR_t + \Delta d_1PR_tRI_t + d_2\dot{p}_t \\ + d_3\dot{U}_t + d_4\frac{\dot{C}}{p\dot{t}} + \Delta d_2\dot{p}_tRI_t \\ + \Delta d_3\dot{U}_tRI_t + \Delta d_4\frac{C}{p\dot{t}}RI_t \\ + e_{Dt},$$

and a similar equation for the Republican vote.

#### Estimates and Implications

Our model differs from most previous studies of the effect of aggregate economic variables by permitting the voter to abstain. It is a peculiar notion of rational behavior, in the sense of equation (1), that requires a voter to believe that the opposition candidates can manage the economy better than the incumbents. If our hypothesis is correct, a main effect of aggregate economic variables is to change the participation rate.

Table 1 shows least squares estimates of the parameters of *VP*, *VD* and *VR* using data for congressional elections from 1896 to 1970.<sup>17</sup> The 1912 election is omitted to avoid the effects of the Roosevelt-Taft split.

Three main implications of our findings deserve emphasis. (1) There is an identifiable, stable, partisan vote. This proposition, useful in analyzing survey data, is supported by our analysis of voting decisions. The magnitudes estimated from the election statistics, however, differ from the results shown in surveys. (2) There is a "coattails" effect. An incumbent president increases his party's vote. A Republican incumbent brings the two parties, on average, near to equality

<sup>17</sup> The disturbances in the *VD* and *VR* equations are linear combinations of the four components of the error term in *VP*. Use of ordinary least-squares to estimate the coefficients of the *VD* and *VR* equations yields unbiased estimates that are not as asymptotically efficient as the generalized least-squares estimators of Zellner.

See Arnold Zellner, "An Efficient Method of Estimating Seemingly Unrelated Regressions and Tests for Aggregation Bias," *Journal of the American Statistical Association*, 57 (1962), 348-68; Arnold Zellner and David S. Huang, "Further Properties of Efficient Estimators for Seemingly Unrelated Regression Equations," *International Economic Review*, 3 (1962), 300-313, and Arnold Zellner, "Estimators for Seemingly Unrelated Regression Equations: Some Exact Finite Sample Results," *Journal of The American Statistical Association*, 58 (December, 1963), 977-92.

**Table 1. Parameter Estimates of Voting Participation. (VP), Democratic (VD) and Republican (VR) Shares in Congressional Elections (t-statistics in parentheses)**

	<i>VP</i>	<i>VD</i>	<i>VR</i>
Constant	47.64 (23.72)*	21.42 (16.97)*	22.30 (22.38)*
<i>X20</i>	-16.39 (6.13)*	-7.05 (4.08)*	-4.51 (3.30)*
<i>X32</i>	11.39 (4.60)*	8.67 (5.37)*	1.58 (1.24)
<i>PR</i>	11.78 (5.48)*	6.81 (4.93)*	5.35 (4.90)*
<i>RIPR</i>	0.76 (0.26)	-2.12 (1.02)	2.68 (1.64)
$\dot{p}$	-0.23 (1.11)	-0.52 (3.05)*	-0.03 (0.21)
$\dot{U}$	0.01 (0.79)	-0.01 (0.56)	0.01 (0.46)
$\dot{C}/p$	0.11 (0.34)	-0.00 (0.18)	0.04 (0.19)
<i>RI</i> $\dot{p}$		0.62 (2.07)*	0.38 (1.63)
<i>RI</i> $\dot{U}$		0.02 (0.78)	-0.02 (0.85)
<i>RI</i> $\dot{C}/p$		-0.09 (0.19)	-0.07 (0.21)
$\bar{R}^2$	.71	.67	.68
Durbin-Watson	.68	1.35	1.11

\* Denotes significance at .05 level, one-tail test.

in presidential election years. (3) We find little evidence that incumbent presidents can increase support for candidates of their own party by reducing unemployment or increasing the growth of real income. In the remainder of this section, we discuss our findings in greater detail.

**Partisans.** Currently, in non-presidential years, 42.6 per cent of the eligible voters are partisans who vote in congressional elections. Presidential elections increase participation by 11.8 to 12.5 per cent on average, the increase depending on the party affiliation of the incumbent President. About 55 per cent of the voters participate in presidential years, and, on our hypothesis, choose candidates independently of current issues. The



Democrats have a slight advantage in nonpresidential years; 23 per cent of the strict partisans vote for the Democratic candidate, and 19 per cent now vote for the Republican candidate. Less than 1 per cent are third-party partisans.

Presidential elections with a Democratic incumbent add 11.7 per cent on average, and increase the Democrats' vote to 29.8 per cent compared to 24.7 per cent for the Republicans. A Republican incumbent appears to have little additional effect on the overall participation rate, but there is a redistribution of votes from Democrats to Republicans. The average Democrat percentage falls to approximate equality with the Republican percentage.

Our findings show that the Democrats generally have an advantage over the Republicans, but the advantage is less than is suggested by differences in the political identification of the electorate obtained from surveys. Surveys classify approximately 19 per cent of the electorate as "strong Democrats" and no more than 14 per cent as "strong Republicans" in presidential years.<sup>18</sup> If we ignore the effect of presidential elections (*PR*) or incumbency (*RIPR*) in Table 1, our estimate of the Democrats' share is four percentage points above the survey data, and our estimate of the Republican share is more than five percentage points higher than shown in the surveys. If we include the effect of presidential elections and incumbency, because the survey data are for presidential elections, both Democrats and Republicans can depend on even larger percentages than shown in the surveys. The Republican percentage rises above the sum of "strong" and "weak" Republicans shown by the surveys, but the Democrats percentage (28 per cent to 30 per cent) remains far below the total for "strong" and "weak" Democrats (40 per cent to 45 per cent) shown in the surveys. There appears to be no way to reconcile the survey data with our estimates of partisan voting.

The long-term partisan share is relatively stable. This is shown by the coefficients of variation of  $V_0$ ,  $d_0$  and  $r_0$ , all less than 6 per cent. Partisan behavior now appears to be less intense than in the past, however. Including women in the electorate in 1920 reduced the partisan vote for both parties and increased the variability of our estimates. "Realignment" in 1932 added to the partisan vote and increased variability further.

Our results suggest that "realignment" is an inappropriate term for the shift in voter sentiment in 1932. Although the Democrats gained more than the Republicans and became the dominant party, the gain did not come from dissatisfied Republicans switching parties. All of the Demo-

crats' net gain, as shown by the coefficient of  $X_{32}$ , is from nonaligned to Democratic and appears as an increase in the participation rate. The partisan Republican vote increased by 1.6 per cent. The increase is not statistically significant but is close to the reduction implied by  $VP - VD$ . We are, therefore, inclined to accept the estimate as meaningful.

**Incumbency.** Presidential elections increase the participation rate by about 12 per cent, on average, regardless of the party in power. Republican voters are slightly less affected than Democrats by a Democrat incumbent and their participation rate is less variable. An incumbent Democratic president increases the Democrats chances of controlling the House; an incumbent Republican president leaves the two parties with almost equal shares of the partisan vote for Congress.

The principal effect of a Democratic incumbent is to increase the number of weak partisans who vote. Both the Democratic and Republican vote increases. The Democratic share rises by more than the Republican share, so the absolute difference increases. However, the relative advantage of the Democrats declines. The Republicans benefit, slightly and in a relative way, from the increased participation in presidential elections, even if the incumbent is a Democrat. A Republican incumbent brings out approximately the same percentage of the total vote, but more Republicans and fewer Democrats vote, on the average. The coattails of Republicans appear to be longer; a Republican incumbent increases the Republican share of the vote by more than a Democrat incumbent increases the Democrat share. Several of these effects are not significant, however, at the 5 per cent level.

**The Effects of Aggregate Economic Variables.** The very small effect of the growth of real compensation is on participation, and not on the share of the vote received by either party. If voter dissatisfaction rises when real income or its growth rate decline, neither party gains or loses any significant percentage of the vote. Our attempts to find an effect of incumbency showed no evidence of a shift from Republicans to Democrats as real compensation changes. The estimated effects are tiny, and they are not significant by the usual test.

Changes in the unemployment percentage have little systematic effect on the participation rate or on party strength. Our results suggest that each party gains or loses from rising or falling unemployment rates when the other holds office, but the effects are small and far from significant. Some voters may believe familiar slogans suggesting that the Democrats are more "concerned"

<sup>18</sup> Flanigan, p. 42.

about employment and unemployment. The results suggest, however, that such voters are likely to be partisans, to have strong views about the two parties, and to have made their choice independently of the current unemployment rate or its rate of change.

Similar null results were obtained in several attempts to test for a significant effect of agricultural prices, stock prices, and other economic variables. These variables have no systematic effect on the participation rate or on the share of the vote going to Democrats and Republicans.

The inflation rate is the only exception to our findings that aggregate economic variables have very little effect on the outcome of congressional elections. High rates of inflation lower the participation rate, but the reduction is not significant. An increase in the inflation rate lowers the Democrats' share of the vote. The decline in the Democrats' share is estimated at half the inflation rate, and the effect is statistically significant. The Republicans are unaffected by the inflation rate.

Including the effect of incumbency changes the interpretation. If inflation rises in a presidential year with a Republican incumbent, our estimates suggest that the Democrats' share is unaffected or may rise slightly (0.62–0.52). The Republican share rises, but the change is not significant. Any effect of higher inflation is borne by the Democrats. The only effect of reducing inflation or increasing deflation, with fixed real income, is to increase the Democrats' share of the vote.

One likely, but untested explanation of this asymmetry is that most of the inflation in this century has occurred during the term of Democratic incumbents. The mean rate of inflation during the years used in our study is 2 per cent; during years of Republican presidents, the mean rate is close to zero. Rising inflation during several years of a Republican administration is a recent phenomenon that provides a test of the partisan effect. Our findings for real compensation and unemployment—where substantial differences in mean rates of change are also observed in Democratic and Republican years—suggest that neither party will benefit. Our findings for inflation suggest that the Republicans should have gained in the non-presidential year 1974 from their failure to control inflation. The relatively large loss of seats suggests that the asymmetric effect of inflation in the past may be attributed to the low rate of inflation in Republican years.

There are other plausible explanations. Unanticipated inflation redistributes wealth or income from creditors to debtors. The losses from unanticipated inflation fall most heavily on those who fail to anticipate inflation or fail to shift their position from net creditor to net debtor. We know that the relevant groups include those whose in-

come is fixed by contractual agreements. Landlords, unionized workers, social security recipients are likely to be heavily represented in the group of net creditors at least during the early stages of inflation. A second plausible explanation can be constructed if these groups include many issue-oriented voters who are inclined toward the Democrats in non-inflationary years. It is not difficult to construct still other plausible explanations of the asymmetry. Discriminating evidence is lacking.

Election statistics indicate that Republicans dominated the pre-1932 period while the Democrats controlled Congress in most of the years after 1932. This suggests the hypothesis that the economic events from 1929 to 1932 created a lasting change in the voting habits of nonpartisans. We have tested two additional hypotheses: One is that economic events were interpreted differently after 1932; the other is that the interpretation depends on the party of the incumbent President. We find no evidence to support either proposition.

### Conclusion

Our results suggest that, with the possible exception of inflation, aggregate economic variables affect neither the participation rate in congressional elections nor the relative strengths of the two major parties. There is very little evidence that an incumbent president can affect the composition of the Congress by measures that have short-term effects on unemployment or real income.

Taken together, the findings support the hypothesis that the principal fluctuations in the percentage of the votes received in congressional elections arise from changes in the participation rate and not from shifts between parties. The percentage of voters participating in presidential years rises markedly. The Democrats gain most when the incumbent is a Democrat; the Republicans gain most when the incumbent is a Republican. Fluctuations in the participation rate have a partisan effect. The Republican share rises to equality with the Democrats share in presidential years if the incumbent is Republican.

The principal changes in the composition of the House during this century appear to reflect four main factors. Two are long-term changes, and two have short-term effects.

From 1896 to 1920, the two parties had about an equal number of partisans. The participation rate fell, and the relative position of the Republicans increased after women were permitted to vote in 1920. From 1920 to 1932, the Republicans had an advantage of three percentage points at the start of the election. The 1932 "realignment" shifted voters from "nonpartisan" to Democratic, but on balance, relatively few voters appear

have changed party loyalty. The increase in the number of partisan Democrats shifted the balance toward the Democrats. One explanation of the shift is the growth in the number of federal employees in the 1930s and of employment in state and local government in the postwar years.

The advantage obtained from the strict partisan vote can be overcome in any election. Two factors that appear most important are presidential elections and local issues or other random factors.

One principal effect of presidential elections is to increase participation. The distribution of the increase depends on the party of the incumbent resident. Our findings suggest that Democrats have a 4 per cent advantage when there is an incumbent Democrat. The Republican share increases relative to the Democrats' in presidential elections, even if the incumbent is a Democrat.

A considerable part of the variance in participation and in party share is left unexplained or, on our interpretation, assigned to local and personal characteristics of the district and candidate. These factors are strong enough to dominate the results and to make our principal finding a null result. Neither our model of rational behavior nor the evidence implies that voters respond to short-term changes in employment or real income by voting for, or against, the party in power.

#### APPENDIX

##### List of Symbols, Definitions and Sources of Data

- $CPI$  Consumer Price Index (1929=100): 1896–1970 *Handbook of Labor Statistics*, 1972.
- $U$  Total Unemployment Rate: 1896–1947, Bureau of the Census: *Long-Term Economic Growth*; 1948–1970 Economic Report of the President, 1970.

$C$  Compensation per man-hour: 1896–1914, Albert Rees: *Real Wages in Manufacturing, 1890–1914*; 1914–1946: Albert Rees: *New Measures of Wage-Earner Compensation in Manufacturing, 1914–1957*; 1946–70 index of compensation per man-hour in non-agricultural establishments, adjusted to current dollars using 1966 benchmark. *Handbook of Labor Statistics*, 1972.

$VP$  Voting turnout: 1920–1970, *Statistical Abstract of the U.S.*, 1971; 1896–1918, computed as the fraction of the number of votes cast for Representative (Historical Statistics of the U.S.) divided by the voting age population. The voting age population, males over 21, for the years 1890, 1900, 1910 and 1920, from *Statistical Abstract of the U.S.* 1898, 1907, 1918 and 1928 respectively; other years by interpolation.

$VD$  Percentage of those eligible to register voting Democratic: 1918–1970 the product of the percentage vote cast for House Democratic candidates (*Statistical Abstract of the U.S.* 1971) and  $VP$ ; 1896–1920: number of votes cast for Democratic candidates (*Historical Statistics of the U.S.*) divided by the voting age population estimated as for  $VP$ .

$VR$  Percentage of those eligible to register voting Republican: Same sources as for  $VD$

$$\dot{p} = 1 - \frac{P_{t-1}}{P_t}$$

with  $t-1$  defined as the year prior to the election.

Similarly for  $\dot{U}$  and  $\dot{C}/p$ .

# Voter Response to Short-Run Economic Conditions: the Asymmetric Effect of Prosperity and Recession\*

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## Introduction

Economic conditions are presently receiving more attention from politicians, journalists, academicians and citizens than probably any other political issue. In light of the publicity they have received and their profound impact on the living standards of American families, it is hard to believe that these economic conditions do not influence voters' selection of candidates for public office.

Nevertheless this simple proposition, which is accepted as conventional wisdom by many politicians, has been the subject of debate among researchers during the past several years. This debate was touched off by Gerald Kramer's pioneering time-series analysis which appeared in the March, 1971 issue of this Review.<sup>1</sup> Using a multivariate regression model, Kramer derived results which appear to indicate that declining real income reduces the vote for the party of the incumbent President, and rising income increases it. Soon after this, George Stigler published findings which he claimed refuted Kramer's.<sup>2</sup> Most recently, Arcelus and Meltzer purport to demonstrate that economic conditions (with the possible exception of inflation) do not affect the vote.<sup>3</sup> How does one resolve this conflict?

To address this question properly, it is necessary to distinguish between the effect on the vote of economic downturns and that of economic upturns. Considerable evidence suggests that the magnitude of these two effects may be quite different. For example, in an article entitled "The Asymmetry of Liking and Disliking," Nehemiah

Jordan cites results from several different experiments which indicate that "a positive attitude (or positive affect) does not have an effect upon measured behavior oppositely equivalent to the effect of a negative attitude or negative affect."<sup>4</sup> In addition, Angus Campbell et al. in *The American Voter*, considering the trend of the "party division of the vote," state that "changes in the party balance are induced primarily by negative rather than positive attitudes toward the party controlling the executive branch of federal government . . . . A party already in power is rewarded much less for good times than it is punished for bad times."<sup>5</sup>

We believe that economic conditions play a small role in the determination of voting behavior in times of prosperity. During these times, their impact is dominated by such forces as the underlying balance of party identification and other more salient political issues. To this extent, our argument supports the views of Arcelus and Meltzer and of Stigler.

But what about years of economic distress? Here the accumulated evidence that economic conditions do have an effect is overwhelming. To interpret this evidence we must distinguish between the effects of long-run and short-run economic conditions. The effect of short-run economic conditions is reflected by greater voter concern over the economy and less favorable evaluation of the administration's performance relative to the economy. This rising chorus of criticism, however, is not uniformly distributed across the electorate. It is most intense among the large group of persons who are directly affected by the economic downturn. Data from *The American Voter* indicate that during the 1957-58 recession this group contained 4 out of every 10 American

\* The authors gratefully acknowledge the helpful suggestions of Henry J. Aaron, Richard E. Caves, John E. Jackson, John F. Kain, and William E. McAuliffe. Special thanks are due to Susan E. P. Bloom for her continual assistance throughout the development of the paper.

<sup>1</sup> Gerald H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964," *APSR*, 65 (March, 1971), 131-143.

<sup>2</sup> George J. Stigler, "Micropolitics and Macroeconomics: General Economic Conditions and National Elections," *American Economic Review*, 63 (May, 1973), 160-167.

<sup>3</sup> Francisco Arcelus and Allan H. Meltzer, "The Effect of Aggregate Economic Variables on Congressional Elections," *American Political Science Review*, 69 (December, 1975), 1232-1239.

<sup>4</sup> Nehemiah Jordan, "The Asymmetry of Liking and Disliking. A Phenomenon Meriting Further Reflection and Research," *Public Opinion Quarterly*, 29 (Summer, 1965), 315-22, at p. 315.

<sup>5</sup> Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), pp. 554-555. In addition, John Mueller presents evidence of this asymmetry with respect to the impact of an "economic slump" on presidential popularity. See John E. Mueller, *War, Presidents and Public Opinion* (New York: John Wiley and Sons, 1973), pp. 213-16.

families and that the effect of the recession on the attitudes of members of this group toward the then-current Eisenhower administration varied systematically with their party identification.<sup>6</sup> Basically, the impact on their attitudes was greatest for independents and weak party identifiers and least for respondents who strongly identified with either major party.<sup>7</sup> The crucial link between these attitudes and voting decisions is illustrated as the authors go on to show that evaluation of the administration's economic performance affected voters' intentions for the 1958 U.S. congressional election.<sup>8</sup>

To estimate the short-run impact of such economic downturns, it is necessary to measure the long-run underlying "expected" partisan balance, or "normal" vote.<sup>9</sup> Both the measurement of this partisan balance and the analysis of its determinants remain subjects of debate. On the basis of the 1930s' experience, however, it appears that this underlying balance can itself be affected by major, prolonged economic distress, such as that which followed the 1929 crash. For example *The American Voter* indicates that the age cohorts entering the electorate in the 1930s, following the crash, produced about 10 per cent more Democratic identifiers than the cohorts entering the electorate in the 1920s.<sup>10</sup>

In the context of the preceding discussion we developed and tested a model which focuses on the response of voters to *short-run* changes in the economy. To the extent that our model is specified as a multivariate regression, and is estimated from aggregate time series data, our study is similar to those of Kramer, of Stigler, and of Arcelus

and Meltzer. Our approach differs from theirs, however, in several important ways: (1) We explicitly recognize the asymmetry of the impact on the vote of short-run economic conditions and demonstrate that economic downturns reduce the vote for the party of the incumbent President, while economic upturns yield no corresponding benefits. (2) We estimate the lag structure of the effect of economic downturns and demonstrate that voters react far more strongly to economic conditions the year prior to an election than they do to economic conditions prior to that. (3) We compare the effect of economic conditions on the vote for the party of the incumbent President during Republican administrations with this effect during Democratic administrations and find no significant difference. (4) We examine the range of economic downturns which affect the vote and find it to be fairly broad.

Our paper is organized as follows: An explanation of the model is followed by a discussion of aggregate national time-series estimates, focusing on the asymmetry of the economic impact, its lag structure, and its impact on each party. Next we discuss a series of sensitivity tests of the model. We then draw upon state-level time-series data to provide further support for the model. In light of all this, we discuss several conceptual and methodological problems in the Arcelus and Meltzer analysis which we feel invalidate their conclusions.

### The Model

The model (Figure 1) postulates that voting behavior is determined by both long- and short-run forces. The structure of the model includes a dependent variable, two independent variables, and a disturbance term.

The dependent variable, *voting behavior*, is represented by the Republican share of the two-party vote in U.S. House of Representatives elections. Third-party votes are not included since it is unclear whether they represent a vote against the incumbent party or a protest against the two-party system. Fortunately, during the period under study, third-party candidates received a small proportion of the vote, and changing the measure to incorporate these votes does not affect our conclusions.<sup>11</sup>

One independent variable is the long-run influence of *party identification*. The balance of party identification represents a "normal" or "expected" baseline vote about which short-run forces, such as economic conditions, cause the ob-

<sup>6</sup> Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter*, p. 387.

<sup>7</sup> Similar evidence is provided by results of Gallup Polls taken during the 1937-38 recession. Approximately 63 per cent of the respondents indicated that they had noticed a "decline in business" in their community during the past two months. Of those who noticed a decline, 58 per cent indicated that they held the current Roosevelt Administration at least partly to blame for it. The variation in response by party, however, was quite striking: only 37 per cent of the Democratic respondents held the administration at least partly to blame, whereas 89 per cent of the Republican respondents did so. George Gallup, *The Gallup Poll: Public Opinion 1935-71* (New York: Random House, 1973), p. 78.

<sup>8</sup> Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter*, p. 391.

<sup>9</sup> Philip E. Converse, "The Concept of a Normal Vote" in *Elections and the Political Order*, Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes (New York: John Wiley and Sons, 1966), pp. 9-39.

<sup>10</sup> Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter*, Figure 7.1, p. 154.

<sup>11</sup> In 35 of the 37 elections studied, the major two parties obtained 94 per cent or more of the total vote. Their median percentage during this period was 97 per cent. Details of the effect of including third party votes are discussed later.

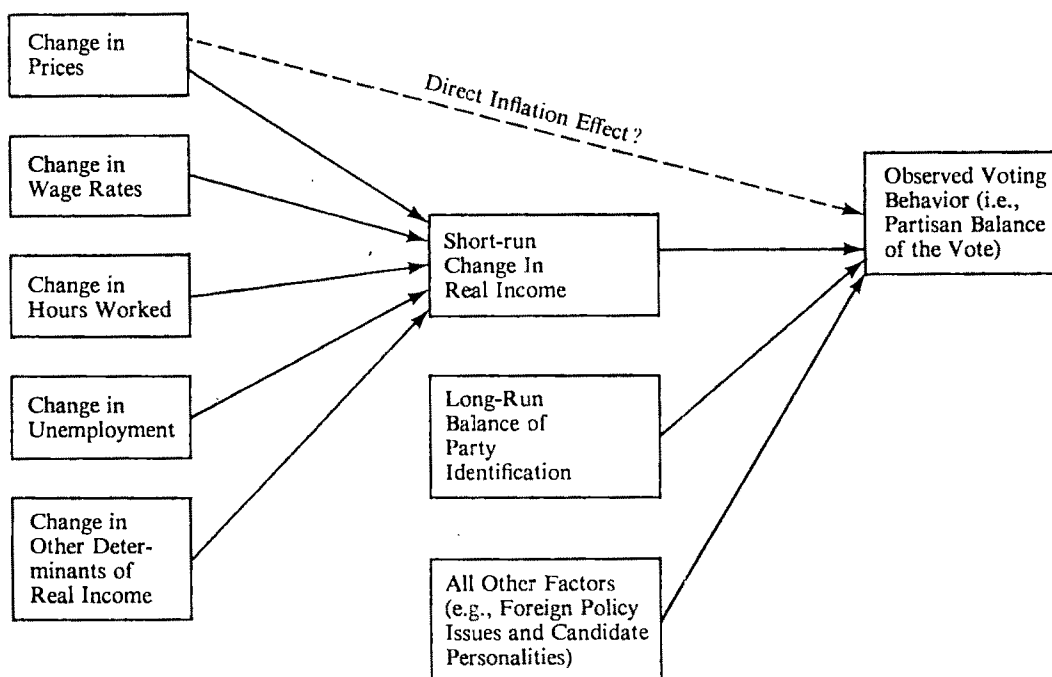


Figure 1. Model of the Impact of Income Changes and Party Identification on Voting Behavior

served vote to fluctuate.<sup>12</sup> Various measures of party ID have been devised by previous researchers.<sup>13</sup> Arcelus and Meltzer use dummy variables in their regression equations to represent different average party shares of the vote during different periods, a procedure which creates sharp breaks between periods and does not account for changes within periods. Kramer uses a linear time variable in some of his regressions which unfortunately assumes implicitly that party ID has followed a linear trend throughout the past century. In a recent paper, Tufte uses a moving average of results in past off-year House elections.<sup>14</sup> This approach enables him to account for gradual changes in partisanship but the weights used to calculate his moving average are not based on empirical evidence.

Voter registration by party, which has been recorded by several states since the early 1920s seemed to us to be the best available measure of

the balance of party ID over time.<sup>15</sup> We used these data to estimate our model based on election results in U.S. House races for three states during the period for which appropriate data are available (1930–70). Registration by party, however, is available only in a limited number of states, for very limited time periods. Since most of our analysis is based on aggregate national time-series, we needed to develop a proxy for registration. This proxy was defined as a weighted moving average of the Republican share of the two-party vote in U.S. House elections. Rather than arbitrarily selecting weights, we estimated them directly from the voting and registration data for the three states used in the state-level analysis. The selection of states and estimation of these weights is explained in detail in Appendix B. Basically, the weights were defined as the coefficients of the re-

<sup>12</sup> Philip E. Converse, "The Concept of a Normal Vote."

<sup>13</sup> One of the earliest attempts to measure party ID from aggregate data is: Donald E. Stokes, "Party Loyalty and the Likelihood of Deviating Elections," *Journal of Politics* (November, 1962), 689–702. Stokes estimates the division of party loyalty by taking the mean vote in all presidential elections from 1892–1960, and experiments with breaking this into subperiods for 1892–1928 and 1932–60.

<sup>14</sup> Edward Tufte, "Determinants of the Outcome of Midterm Congressional Elections," *American Political Science Review*, 69 (September, 1975), 812–826.

<sup>15</sup> We recognize that individuals, regardless of their true attitudes toward each party, may have incentives to register with the locally dominant party. We do not feel, however, that this poses a serious problem for our analysis because (1) we use aggregate statewide data, in which specific local biases are probably largely cancelled out; (2) remaining net systematic biases will be picked up in the intercepts of our regressions and thus will not affect our coefficient estimates; and (3) remaining net random biases will be picked up in the disturbance terms of our regressions and thus will not bias our coefficient estimates. For an extensive analysis of registration changes over time, see James Sundquist, *Dynamics of the Party System* (Washington: Brookings Institution, 1973), pp. 204–233.

ession of the Republican share of the two-party registration on the Republican share of the two-party vote in each of the two preceding elections. The weights were estimated to equal 0.69 and 0.36 for each preceding election respectively. Thus, the registration proxy was defined to equal the sum of 69 multiplied by the Republican share of the vote in the preceding election plus 0.36 multiplied by the Republican share of the vote in the election preceding it.

As can be seen, these weights decline rapidly. In fact, estimates presented in Appendix B indicate that only the results of the two immediately preceding elections are required to predict registration.

The other independent variable in the model, economic conditions, is most comprehensively measured by *changes in real income*. This change in spending power affects citizens' attitudes and thus influences their voting behavior. Figure 1 illustrates that prices, wage rates, hours worked, unemployment rates and other factors all contribute to the determination of real income. Arcelus and Meltzer claim that one of these factors (inflation), may have a direct effect on the vote. This direct effect represents an interesting refinement of the basic model, but since the determinants of income are highly intercorrelated, it is very difficult to distinguish their separate impacts on the vote. Fortunately, it is unnecessary to demonstrate such direct effects to show that overall economic conditions influence the vote.

The model requires an income measure which represents the change in the "average" voter's real spending power prior to each election. Because it was judged to be the best available measure of income as perceived by the voter, the percentage change in real per capita personal income during the year preceding each election was selected for this purpose. This measure includes before-tax income from all sources accruing to private individuals. One might argue that disposable, after-tax income better represents actual spending power. Since, however, the annual percentage change rates for these two variables have a correlation coefficient of 0.97, they contain virtually the same information (based on data available for 1920-70).

Personal income is available by state since 1929 and for the nation since 1919. Since the national analysis extends back to 1896, it was necessary to find an alternative measure for the earlier years. Percentage change in real per capita gross national product was selected. Of the available measures, it correlated most highly ( $r=0.91$ ) with the percentage change in personal income for the years in which these two series overlap (1920-1966).

Stigler points out that there is "no naturally correct period" before each election during which

income changes are most relevant to the determination of voting behavior.<sup>16</sup> Since personal income and gross national product are reported only on an annual basis back to 1896, the shortest possible time period is a year. Both Kramer and we use the change in real income during the year preceding each election under the plausible implicit assumption that voters react more strongly to more recent events. Stigler, on the other hand, chooses to average the percentage change in income during the two years preceding each election. According to him, his regressions were "surprisingly sensitive to this change of measure."<sup>17</sup> Unfortunately Stigler's procedure may just illustrate that if you average an important variable (income change the year directly preceding each election) with a relatively unimportant one (income change the second year prior to each election), you may dilute the apparent importance of the first variable. Estimates discussed later suggest that this is the case; hence instead of averaging the two annual changes, Stigler should have entered them as separate independent variables and estimated their relative impacts from his data.

The model does not assume that economic conditions are the sole determinants of the vote. *Other factors*, such as foreign policy and candidate personalities, may play an important role. In the regression equations which follow, these factors are represented by the disturbance terms as sources of "unexplained" variation in the vote. As long as they are not correlated with income changes, their omission does not bias resulting estimates of the impact of economic conditions on the vote.<sup>18</sup>

#### Estimation of the Model from Aggregate National Time-Series

**The Effect of Income Changes.** The model was specified as the following linear regression.

##### Equation 1. *Original Model Specification*

$$\text{VOTE} = a + B_1(\Delta \text{INC} \cdot I) + B_2(\text{ID}) + e,$$

where

VOTE = The Republican percentage of the two-party vote.

$\Delta \text{INC}$  = The percentage change in real per capita income during the year preceding each election.

I = An incumbency variable equal to 1 for

<sup>16</sup> George J. Stigler, "Micropolitics and Macroeconomics," p. 163.

<sup>17</sup> *Ibid.*

<sup>18</sup> For a general discussion of the effect of omitted variables on the estimates of regression coefficients, see Potluri Rao and Roger L. Miller, *Applied Econometrics* (Belmont, California: Wadsworth, 1971), pp. 60-67.

Republican presidential administrations and -1 for Democratic ones.

ID=The "expected" Republican percentage of the two-party vote due to party identification. For all national analyses, ID is measured by the registration proxy.

$a, e$ =The intercept and disturbance terms respectively.

This equation states that the Republican share of the vote is a linear additive function of changes in income,  $\Delta\text{INC}$ , and party identification, ID. To complete the specification, it is necessary to multiply  $\Delta\text{INC}$  by an incumbency dummy variable,  $I$ , to account for the party of the incumbent President at the time of each election. The rationale for this dummy variable is as follows: If income declines during a Republican administration, the Republican share of the congressional vote is expected to fall. If income declines during a Democratic administration, the Democratic share should decline and the Republican share will increase accordingly.<sup>19</sup> Thus, the coefficient,  $B$ , of the independent variable,  $(\Delta\text{INC} \cdot I)$ , should be positive for elections preceded by economic downturns. For reasons stated previously, however, we would not expect the coefficient to be significantly different from zero for elections preceded by increasing income. Thus the sign, magnitude, and statistical significance of this coefficient provide a basis for testing the model.

Equation 1 was estimated from aggregate national data for all congressional elections from 1896-1970 except 1912, basically the same time period used for previous studies. (The election of 1912 is deleted because of its ambiguous results arising from the large Progressive vote that year.<sup>20</sup>) Results are summarized in Table 1. Separate estimates are displayed for (1) elections preceded by declining real income, (2) elections preceded by rising income, and (3) all elections during the period. Each parameter estimate is listed in the table with its corresponding  $t$ -statistic directly beneath it in parentheses.

The most striking feature of these results is the difference between elections preceded by economic downturns and those during times of relative prosperity. For economic downturns, the coefficient of the income variable,  $(\Delta\text{INC} \cdot I)$ , is large, has the expected sign, and is statistically significant at the 0.01 level. On the other hand, during prosperous years, this coefficient is considerably

Table 1. Regression of the Congressional Vote on Income Change and Party ID  
(National, 1896 to 1970, Except 1912)

	Elections Preceded by		All Elec- tic ( $N=3$ )
	Declining Income ( $N=13$ )	Rising Income ( $N=24$ )	
Coefficients of			
Income Change [ $\Delta\text{INC} \cdot I$ ]	0.75** (3.5) <sup>a</sup>	-0.12 (-0.9)	0.21* (2.1)
Party Identifica- tion [ID]	1.19** (3.6)	0.86** (5.2)	0.61* (4.6)
Intercept	-0.12 (-0.7)	0.05 (0.5)	0.19** (2.7)
$R^2$	0.60	0.62	0.45
DW	1.5	2.0	1.9

\* Significant at .05 level, two-tail.

\*\* Significant at .01 level, two-tail.

<sup>a</sup> All  $t$ -statistics are in parentheses below parameter estimates.

smaller, has the wrong sign, and is not significant at the 0.05 level.<sup>21</sup>

These results provide compelling evidence of the fact that economic conditions have a strong asymmetric impact on the congressional vote. Political parties are "punished" by the voters for economic downturns but are not "rewarded" accordingly for prosperity. Apparently, in bad times the economy becomes a salient issue, whereas in good times it diminishes in importance relative to other determinants of voting behavior. It should be noted that all estimates show a significant positive correlation between the vote and party ID which explains why the overall  $R^2$  is virtually identical in the two separate estimates of the equation.<sup>22</sup>

The model was specified in the form of Equation 1 to provide maximum comparability with Kramer's original results and to provide a better

<sup>21</sup> The difference between the significance of the results for the two subsamples is even more striking if one considers that the increasing income estimates are based on almost twice as many observations as the declining income estimates.

<sup>22</sup> This point is demonstrated by results of the regression of VOTE on ID only. For economic upturns, ID "predicts" 61 per cent of the variation in VOTE. In other words, past trends "predict" the current vote fairly well. On the other hand, during economic downturns, ID "predicts" only 9 per cent of the variation in VOTE, since economic conditions cause voters to deviate from their past behavior.

<sup>19</sup> Because VOTE is defined in terms of the two-party vote, the percentage of the vote lost or gained by Republicans exactly equals the percentage gained or lost by Democrats.

<sup>20</sup> The two major parties received only 80 per cent of the congressional vote.



understanding of the following specification (Equation 2) which is used throughout the remainder of the paper.

Equation 2. *Preferred Model Specification*

$$\text{DEV} = a + B(\Delta\text{INC} \cdot \text{I}) + e,$$

where

DEV = The deviation of the Republican percentage of the two-party vote from its "expected" percentage due to party identification (i.e., VOTE minus ID).

$\Delta\text{INC}$  = The percentage change in real per capita income during the year preceding each election.

I = An incumbency dummy variable equal to 1 for Republican presidential administrations and -1 for Democratic ones.

$a, e$  = The intercept and disturbance terms respectively.

Equation 2 is preferable to the original specification because it focuses more directly on the principal subject of our analysis, the relationship between short-run economic and electoral fluctuations.<sup>23</sup>

This equation states that DEV, the deviation of the vote from its "expected" outcome, is a linear function of the change in income,  $\Delta\text{INC}$ , times the incumbency variable, I, discussed previously. Expectations for the income coefficient are the same as those for the income coefficient in Equation 1. For economic downturns, it should be significantly positive, while for upturns it should not be significantly different from zero. Estimates of Equation 2 from the aggregate national time-series are listed in Table 2. They are entirely consistent with our expectations.

The present specification facilitates direct comparison of the two subsamples in terms of  $R^2$  as well as the income coefficient. This comparison further illustrates the asymmetry of the process we have been describing: for economic downturns,  $R^2$  is quite high, while for upturns it is negligible.

**The Effect of Income Changes by Party.** After developing the basic conclusions of the model, we examined more closely how economic fluctuations

<sup>23</sup> Equation 2 is equivalent to Equation 1 with the value of the coefficient for ID constrained to equal one. This constraint is consistent with our inclusion of REG in the model to represent the "expected" vote, around which short-run economic conditions cause the observed vote to fluctuate. Empirical justification for this assumption is provided by the fact that the coefficients for ID in the appropriately specified asymmetric estimates of Equation 1 are not significantly different from one.

Table 2. Regression of Short-Run Deviations in the Congressional Vote on Income Change (National, 1896 to 1970, Except 1912)

	Elections Preceded by		All Elections (N = 37)
	Declining Income (N = 13)	Rising Income (N = 24)	
Coefficients of Income Change [ $\Delta\text{INC} \cdot \text{I}$ ]	0.66** (4.8) <sup>a</sup>	-0.19 (-1.8)	0.17 (1.6)
Intercept	-0.02 (-1.8)	-0.03** (-4.5)	-0.02** (-3.0)
$R^2$	0.67	0.12	0.07
DW	1.5	2.0	2.0

\*\* Coefficients and intercepts bearing "\*\*\*\*" are significant at the .01 level, two-tail; all others do not even attain significance at the .05 level, two-tail.

<sup>a</sup> All *t*-statistics are in parentheses below parameter estimates.

affect voting behavior by first attempting to determine whether these fluctuations affect the vote for the party of the incumbent President to the same extent for both Republican and Democratic administrations. If, for some reason, voters treat administrations of the two parties differently with respect to economic issues, this would have important implications for party strategies.

Up to this point, we have specified a single economic variable,  $(\Delta\text{INC} \cdot \text{I})$ , in the model. This implicitly assumes no difference between the parties' susceptibility to economic fluctuations. The coefficients of this variable in Equations 1 and 2 represent the response of voters to income changes during *both* Republican and Democratic administrations. A simple test for different responses under different party administrations is provided by respecifying the model as Equation 3.

Equation 3. *Income Effect by Party*

$$\text{DEV} = a + B_1(\Delta\text{INC} \cdot \text{R}) + B_2(\Delta\text{INC} \cdot \text{D}) + e,$$

where

DEV = The deviation of the Republican percentage of the vote from its "expected" percentage as defined in Equation 2.

$\Delta\text{INC}$  = The percentage change in real per capita income during the year preceding each election.

R, D=Republican and Democratic presidential dummy variables (R=1, D=0 during Republican administrations and R=0, D=1 during Democratic ones).

a, e=The intercept and disturbance terms respectively.

Equation 3 allows for different income coefficients during Republican administrations,  $B_1$ , and Democratic ones,  $B_2$ . For example, consider the income effect during Republican administrations. The Republican incumbency variable, R, equals 1 and the Democratic incumbency variable, D, equals 0. As a result, the model reduces to Equation 4 in which the income effect is measured only by the Republican coefficient,  $B_1$ .

Equation 4. *Income Effect During Republican Administrations*

$$DEV = a + B_1(\Delta INC) + e$$

According to the model, during economic downturns, the Republican vote will be lower than average and  $B_1$  should be positive. A similar logic applies during Democratic administrations and the model now simplifies to Equation 5.

Equation 5. *Income Effect During Democratic Administrations*

$$DEV = a + B_2(\Delta INC) + e$$

In this situation, the Republican vote should correlate negatively with economic changes and  $B_2$  should be negative. If both parties are equally susceptible to economic changes when they are in the White House,  $B_1$  and  $B_2$  should have opposite signs and equal absolute values during economic downturns. In addition, according to the present model, neither coefficient should be significantly different from zero during prosperous years.

Table 3 lists estimates of Equation 3 from the national data. Note that during economic downturns, coefficients for both parties have correct signs and are similar in magnitude. In addition, their *t*-statistics show that one is significant at better than 0.05 while the other is significant at 0.06. Significance in such a small sample indicates that these coefficients probably measure a robust phenomenon. On the other hand, estimates for prosperous times yield insignificant income effects regardless of the party in power. In conclusion, the data do not indicate appreciable differences between the effect of income on the vote during Republican or Democratic administrations.

**Lag Structure of the Effect of Income Changes.** Next we attempted to clarify the lag structure of the impact of changing income on voting behavior both to gain insights into the dynamics of

**Table 3. Regression of Short-Run Deviations in the Congressional Vote on Income Change by Party of the Incumbent President**

(National, 1896 to 1970, Except 1912)

	Elections Preceded by	
	Declining Income <sup>b</sup> (N=13)	Rising Income <sup>b</sup> (N=24)
Coefficient of Income Change, Republican President [ $\Delta INC \cdot R$ ]	0.59* (2.5) <sup>a</sup>	-0.09 (-0.3)
Income Change, Democratic President [ $\Delta INC \cdot D$ ]	-0.79 (-2.1)	0.23 (1.2)
Intercept	-0.02 (-1.3)	-0.03** (-3.3)
R <sup>2</sup>	0.68	0.13
DW	1.6	2.0

\* Significant at .05 level, two-tail.

\*\* Significant at .01 level, two-tail.

<sup>a</sup> All *t*-statistics are in parentheses below parameter estimates.

<sup>b</sup> Of the 13 elections preceded by declining income, 8 had Republican incumbent Presidents and 5 had Democratic ones. Of the 24 elections preceded by rising income, 10 had Republican incumbent Presidents and 14 had Democratic ones.

the process and to resolve the conflict between Kramer's and Stigler's results. One simple way to do this is to estimate Equation 6, which includes the annual percentage income change for each of the two years preceding every election as separate explanatory variables.

Equation 6. *Lag Structure of the Income Effect*

$$DEV = a + B_1(\Delta INC(-1) \cdot I) + B_2(\Delta INC(-2) \cdot I) + e,$$

where:

DEV=The deviation of the Republican percentage of the vote from its "expected" percentage as defined in Equation 3.

$\Delta INC(-1)$ =The percentage change in real per capita income during the first year preceding each election. (Note that this has been previously referred to as  $\Delta INC$ .)

$\Delta\text{INC}(-2)$  = The percentage change in real per capita income during the second year preceding each election.

$I$  = An incumbency dummy variable equal to 1 for Republican presidential administrations and  $-1$  for Democratic ones.

$a, e$  = The intercept and disturbance terms respectively.

Table 4 lists the results of estimating Equation 7 from the national data. For elections preceded by an economic downturn, the coefficient for the income change the year directly preceding each election is quite large and highly statistically significant, whereas the coefficient for the income change the second year prior to each election is indistinguishable from zero. Thus it is easy to see how by averaging  $\Delta\text{INC}(-1)$  and  $\Delta\text{INC}(-2)$  together, Stigler misspecified the model, thereby underestimating the income effect and concluding that it does not exist. Whereas Kramer, by including only the income change most relevant to voter decisions, was able to detect the income effect.

#### Sensitivity Tests of the Effect of Income Changes.

We performed a series of tests to determine the sensitivity of our results to the particular elections included in the sample and the definitions of variables that were used. Results of these tests indicate that our findings hold for a fairly broad range of economic conditions and variable definitions.

The first test shifts the emphasis from the existence of an income effect on the vote to a consideration of the conditions under which this effect occurs. To the best of our knowledge, few people would deny that a major depression, such as that following 1929, is "bad news" for the incumbent party. Indeed, the largest number of House seats lost since the Civil War occurred in 1874 (Republicans lost 96 seats after the depression of 1873), 1894 (Democrats lost 116 seats after the depression of 1893), and 1932 (Republicans lost 101 seats as the post-1929 depression worsened). In a nonpartisan sense, we would label this the Hoover-Cleveland effect. But such massive changes are only involved in three of the elections since the Civil War. Does the electorate also respond to less drastic economic downturns?

To study this question, elections were cumulatively deleted from estimates of Equation 2 in order of greatest to least decline in real per capita income. Results of this procedure (Table 5) indicate that the model survives deletion of half the sample. In fact, the coefficient of the income effect changes by less than 12 per cent. In addition,

**Table 4. Regression of Short-Run Deviations in the Congressional Vote on Income Changes Each of the Two Years Preceding Every Election**  
(National, 1896 to 1970, Except 1912)

	Elections Preceded by	
	Declining Income ( $N=13$ )	Rising Income ( $N=24$ )
Coefficient of Income Change, First Year Prior to Election [ $\Delta\text{INC}(-1) \cdot I$ ]	0.67** (4.6) <sup>a</sup>	-0.20 (-1.5)
Income Change, Second Year Prior to Election <sup>b</sup> [ $\Delta\text{INC}(-2) \cdot I$ ]	0.08 (0.4)	0.006 (0.1)
Intercept	-0.02* (-1.8)	-0.03** (-4.3)
$R^2$	0.68	0.12
$DW$	1.6	2.0

\* Significant at .05 level, two-tail.

\*\* Significant at .01 level, two-tail.

<sup>a</sup> All  $t$ -statistics are in parentheses below parameter estimates.

<sup>b</sup> The value of  $\Delta\text{INC}(-2)$  for the 1920 election (representing the income change from 1918-1919) was calculated from the per capita GNP data. See Appendix A for data sources.

although the  $t$ -statistic and  $R^2$  decline regularly with the deletion of each election, they consistently provide confirmation of the model. Further deletions leave too small a sample for clear interpretation of the results. Thus, the vote for the incumbent party appears to be measurably reduced by a fairly wide range of economic downturns. It should be reiterated that our sample contains only elections from 1896-1970, except 1912. Because of the absence of sufficient comparable time-series data, it was not possible to include the 1873 and 1893 depressions; their inclusion would have further reinforced our findings.

Next, a standard "jackknife" test was performed for years of declining income to determine whether any single observation had an effect on the regression coefficients that was drastically different from the effect of the others. This test consisted of systematically deleting a single election from the estimate of Equation 2 until each election had been deleted from the sample once. No election had an appreciable impact on estimates of the model. The coefficient,  $B$ , varied by less than 14

**Table 5. Sensitivity Test of Effect of Income Decline on Short-Run Deviations in Congressional Vote: Results with Cumulative Omission of Largest Income Declines<sup>a</sup>**

Elections Remaining in Sample	Coefficient of Income Change, All Presidents	<i>t</i> Statistics	Significance Level <sup>c</sup>	<i>R</i> <sup>2</sup>
All 13 <sup>b</sup>	0.66	4.8	0.0006	0.67
12	0.58	3.2	0.009	0.51
11	0.65	3.0	0.016	0.49
10	0.71	2.6	0.031	0.46
9	0.83	2.1	0.075	0.39
8	0.77	1.5	0.190	0.27
7	0.64	1.0	0.380	0.16

<sup>a</sup> Elections are deleted in order of largest to smallest income decline.

<sup>b</sup> Elections included (with corresponding annual percentage income change in parentheses): 1932 (-15.6), 1908 (-10.0), 1914 (-9.4), 1930 (-9.1), 1938 (-6.8), 1946 (-4.7), 1920 (-4.3), 1896 (-3.8), 1904 (-3.2), 1954 (-1.6), 1958 (-1.5), 1902 (-1.1), 1910 (-1.0).

<sup>c</sup> Two-tail.

per cent of its original value, and all estimates yielded high *R*<sup>2</sup> and large *t*-statistics.<sup>24</sup>

Next, we examined the sensitivity of our results to changes in the definitions of the variables. We first measured the impact of changing the definition of voting behavior from the Republican share of the two-party vote (which we prefer for reasons cited earlier) to the Republican share of the total vote. To do this, Equation 2 was re-estimated from the national data, using the new measure of voting behavior. The resulting coefficient, *B*, for the income term differed by less than 15 per cent of its original value and the *t*-statistic and *R*<sup>2</sup> remained stable. In addition, modifications to the registration proxy were incorporated into estimates of Equation 2 by changing the weights used to calculate this proxy. These weights were varied to extremes unlikely to occur given their estimated standard errors (see Appendix B). The coefficient, *B*, of the income term changed by less than 6 per cent of its original value and all estimates yielded high *t*-statistics and high *R*<sup>2</sup>.

<sup>24</sup> Results of the model (Equation 2) for elections preceded by rising income were subjected to the following sensitivity test: The rising income subsample was split in two, with one half containing the elections preceded by the largest income increases and the other half containing the elections preceded by the smallest income increases. Equation 2 was then estimated separately from each half of the rising income subsample. The results for both estimates were quite similar to those reported for the rising income subsample as a whole.

### Estimation of the Model from State-Level Time-Series

To test the preceding conclusions further, the model was estimated from data for the three states (California, Pennsylvania, and New York) that were used to construct the registration proxy. These estimates were based on personal income, registration and voting data for each U.S. House election from 1930-70. Separate estimates were made for elections preceded by economic downturns and those preceded by years of prosperity. In addition, two forms of the model were estimated, corresponding to Equations 2 and 3 in the national-level analysis. One assumed that income changes affect both parties to the same extent. The other allowed this effect to vary by party. Equations 7 and 8 illustrate these two specifications.

#### Equation 7. State-Level Replication of Equation 2

$$\text{DEV} = a + B_1(\text{CAL}) + B_2(\text{PENN}) + B_3(\Delta\text{INC} \cdot \text{I}) + e$$

#### Equation 8. State-Level Replication of Equation 3

$$\text{DEV} = a + B_1(\text{CAL}) + B_2(\text{PENN}) + B_3(\Delta\text{INC} \cdot \text{R}) + B_4(\Delta\text{INC} \cdot \text{D}) + e,$$

where

DEV = The deviation of the Republican percentage of the two-party vote from its "expected" percentage arising from party identification (i.e., VOTE minus ID, where in this case, ID is calculated directly as the Republican percentage of the two-party registration).

ΔINC = The percentage change in real per capita income during the year preceding each election.

I, R, D = Presidential dummy variables (I = 1, R = 1 and D = 0 during Republican administrations; I = -1, R = 0 and D = 1 during Democratic administrations).

CAL, PENN = State dummy variables (CAL = 1, PENN = 0 for California; CAL = 0, PENN = 1 for Pennsylvania; CAL = 0, PENN = 0 for New York).

*a*, *e* = The intercept and disturbance terms respectively.

Table 6. Regression of Short-Run Deviations in the Congressional Vote on Income Change  
(California, Pennsylvania, New York 1930-70)

	Equation 7 Elections Preceded by		Equation 8 Elections Preceded by	
	Declining Income (N=21)	Rising Income (N=42)	Declining Income (N=21)	Rising Income (N=42)
Coefficient of				
Income Change, All Presidents [ $\Delta\text{INC} \cdot \text{I}$ ]	0.76** (5.0) <sup>a</sup>	0.16 (1.2)		
Income Change, Republican Pres. [ $\Delta\text{INC} \cdot \text{R}$ ]			0.73** (3.0)	-0.27 (-0.4)
Income Change, Democratic Pres. [ $\Delta\text{INC} \cdot \text{D}$ ]			-0.85 (-1.7)	-0.23 (-1.3)
California [CAL] <sup>b</sup>	0.08** (-3.0)	0.08** (4.8)	0.08** (2.9)	0.08** (4.8)
Pennsylvania [PENN] <sup>b</sup>	-0.08** (-3.1)	-0.07** (-4.1)	-0.09** (-3.0)	-0.06** (-3.9)
Intercept	0.02 (0.9)	0.03* (2.2)	0.01 (0.5)	0.03* (2.3)
R <sup>2</sup>	0.79	0.68	0.79	0.68
DW	2.0	1.2	2.0	1.3

\* Significant at .05 level, two-tail.

\*\* Significant at .01 level, two-tail.

<sup>a</sup> All *t*-statistics are in parentheses below parameter estimates.

<sup>b</sup> CAL and PENN identify the state for each observation (CAL=1, PENN=0 for California; CAL=0, PENN=1 for Pennsylvania; and CAL=0, PENN=0 for New York).

These equations are identical to those estimated from the national data, with two exceptions. First, since party ID can be measured by state-level registration, it is unnecessary to calculate a registration proxy. Instead, registration can be used directly in defining the dependent variable (i.e.,  $\text{DEV} = \text{VOTE} - \text{ID}$ ). Second, it is necessary to include two dummy variables, CAL and PENN, to identify each observation by state. The coefficients,  $B_1$  and  $B_2$ , of these two variables indicate differences between New York's intercept,  $a$ , and those for California and Pennsylvania respectively. Interpretation of the estimated income coefficients is identical to that of estimates from the national data.

The state-level results are listed in Table 6. As can be seen, the estimated income coefficients are consistent with those obtained from the national data. They are large and significant for economic downturns but small and not significantly different from zero for prosperous times. In addition, the income coefficients for economic downturns

during Republican and Democratic administrations are similar to each other and remarkably similar to those obtained from the national data.<sup>25</sup>

#### Implications for the Arcelus and Meltzer Study

Having presented our results, we now turn to the Arcelus and Meltzer study. Their findings con-

<sup>25</sup> The state estimates do not provide a completely independent replication of the national analysis. The two analyses are linked to the extent that the characteristics of the three states are imbedded in the national data. If the model had been estimated from data for the nation minus the three states, the results would have been completely independent from the state-level analysis. This was not desirable, however, since the state time-series are shorter than the national ones. Nevertheless, the interdependency between the two levels of analysis is probably not serious since the three states represent only 25 per cent and 28 per cent of the total national vote in the 1930 and 1970 congressional elections, respectively. Also, state estimates cover little over half the time period included in the national analysis.

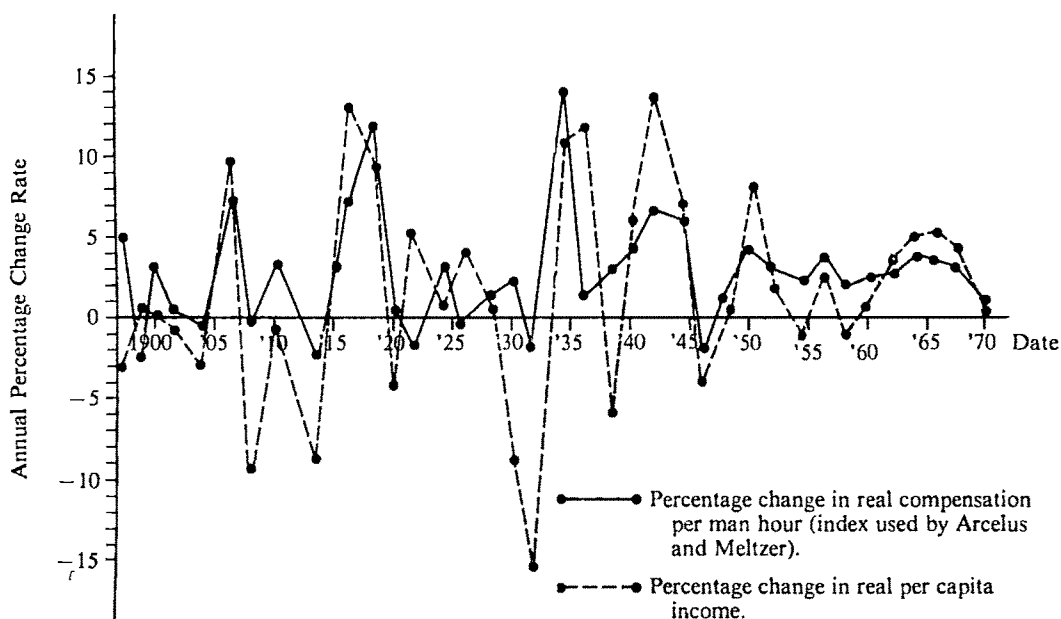


Figure 2. Comparison of the Annual Percentage Change Rates for Real Per Capita Income and Real Compensation Per Man Hour from 1896 to 1970<sup>a</sup>

<sup>a</sup> Each point represents the annual income or compensation change rate preceding the election year indicated.

flict with ours in several important ways, because of conceptual and methodological problems in their analysis.

One of their most serious problems is that they overlook the difference between the impact of economic downturns and the impact of economic upturns. Their specification of the model implicitly assumes that the impacts of upturns and downturns are opposite in sign and equal in magnitude. As previously discussed, our results demonstrate the inappropriateness of this assumption.

A second important problem is Arcelus and Meltzer's use of real compensation per man hour as a measure of real income. For years, economists have recognized that wages may rise during prosperous times when income is rising but that they do not fall in a corresponding manner during economic downturns when income is declining. This downward rigidity of wages is often explained by such factors as union contracts and minimum wage laws.<sup>26</sup> Figure 2 illustrates the difference between the percentage change in real compensation per man hour used by Arcelus and Meltzer and the percentage change in real per capita income (measured by personal income and GNP), for the time period used in their analysis. For example, during the Great Depression from 1931-32, real compensation per man hour declined by only 2 per cent, while real per capita

income fell 16 per cent. For the set of elections in their sample, real compensation per man hour "predicted" only 41 per cent of the variation in real per capita income. Thus we feel that compensation per man hour seriously misrepresents income changes.

A third problem with the Arcelus and Meltzer study is that their model includes three separate determinants of real income changes; [ $p$  (price changes),  $U$  (unemployment rate changes and  $C/p$  (changes in real compensation per man hour)] as explanatory variables. Arcelus and Meltzer find that no single factor (except possibly prices) exhibits a separate impact on the vote, and they conclude from this that overall short-run economic conditions have no effect. This conclusion does not follow from their results for the following reasons.

- (1) It is possible that no single real income determinant is powerful enough to influence the vote but that the entire group as a whole (or real income itself) has a pronounced effect.
- (2) The linear combination of prices, unemployment, and compensation per man hour used by Arcelus and Meltzer accounts for only part of the changes in real income because (a) additional factors enter into its determination and (b) the functional form relating income changes to its determinants is non-linear. Thus even as a group, these variables do not represent a complete test of the impact of *real income* on the vote. To illustrate this point the percentage change in per capita real income was

<sup>26</sup> See Gardner Ackley, *Macroeconomic Theory* (New York: Macmillan, 1961), pp. 196-198.

regressed on Arcelus and Meltzer's three variables ( $\dot{p}$ ,  $\dot{U}$  and  $\dot{C}/p$ ). As a group, these variables "predicted" only 61 per cent of the variation in real income changes.

- (3) Arcelus and Meltzer's economic variables are inter-correlated with each other and with the additional explanatory variables in their model (see their Table 1). This collinearity makes it difficult (if not impossible) to ascertain the separate impact on voting behavior of each economic variable. Their model further compounds this problem by including 10 explanatory variables in regressions which are estimated from only 37 observations. To assess the extent of this problem, it is useful to determine the multiple correlation,  $R$ , between each of their economic variables ( $\dot{p}$ ,  $\dot{U}$  and  $\dot{C}/p$ ) and the remaining nine explanatory variables in their model. To do this, each economic variable was separately regressed on all nine remaining explanatory variables. The resulting multiple correlation coefficient for  $\dot{p}$  as a linear function of the nine variables was 0.77; that for  $\dot{U}$  was 0.86; and that for  $\dot{C}/p$  was 0.77. This collinearity causes large standard errors for their regression coefficients, which make it very difficult to obtain estimates of the separate impact of each economic variable.

### Conclusions and Suggested Further Research

State and national time-series and selected cross-sectional survey data indicate that economic downturns have a pronounced effect on the vote in U.S. House elections. They support a modified, asymmetric version of Kramer's original model which postulates a "throw the rascals out" phenomenon, in which economic downturns reduce the vote for the party of the incumbent President, but economic upturns have no corresponding effect. In addition, the data show that (1) the effect of economic downturns on the party of the incumbent President is the same for either Republican or Democratic administrations, (2) voters primarily consider economic conditions which occur the year immediately preceding each election, (3) the model holds for a fairly broad range of economic conditions, and (4) the contradictory findings of Arcelus and Meltzer and of Stigler can be explained by problems in their methodologies.

Studies to date, however, represent only a first step toward a full understanding of the role of economic issues in the political process. The following is a sampling of the many possibilities for future research in this area.

**Assessment of the Relative Political Impacts of Inflation and Unemployment.** Strong inflation is historically infrequent and largely associated with the occurrence of wars. High peacetime inflation rates are a recent phenomenon. Unemployment rates correlate with real income changes. For these reasons, it is unlikely that aggregate time-series can provide meaningful estimates of the

relative impacts on voting behavior of inflation and unemployment. Such information, however, would be quite useful to public officials who must decide upon policies which make trade-offs between these problems. In our opinion, survey research techniques are the most fruitful potential source of this information.

**Replication of the Analysis From More Disaggregate Data.** Annual state per capita income data for 1919-21 and 1929 to the present exists in published form.<sup>27</sup> For more recent years, the Office of Business Economics is compiling income estimates for all counties and most major metropolitan areas.<sup>28</sup> This information, together with voting and registration data for those areas where it exists, could be used to replicate estimates of the model.

**Application to Other Elective Offices.** The structural relationship between economic conditions and the vote for the incumbent party may be common to a variety of elective offices. We have relied on the U.S. House vote, partly for reasons of comparability with previous studies, but also because the Senate and presidential time-series provide smaller samples. The Senate series begins in 1914 and the presidential series includes only half as many elections as the House series. In addition, we expect the effect of candidate personalities to reduce the impact of economic conditions in senatorial and presidential elections. These hypotheses, however, remain to be tested.

Finally, it should be noted that economic conditions are only one of many issues that enter into the political process and that in a broader context, a better understanding of their role is important for improving our knowledge of the role of issues in general.

## APPENDIX A

### Data Sources and Definitions of Variables

#### Aggregate National Analysis

1. *Percentage of the total vote by party, U.S. House of Representatives Elections, 1896-1970:* Obtained from unpublished tables provided by Prof. W. D. Burnham of MIT, which are considered to be the most recently updated figures

<sup>27</sup> See Appendix A for sources of state income data published by the Department of Commerce for 1929 to the present. For 1919-21 see: Maurice Leven, *Income In the Various States, Its Sources and Distribution, 1919, 1920 and 1921* (New York: National Bureau of Economic Research, 1925). Note that the Leven data are not strictly comparable to the Department of Commerce data.

<sup>28</sup> "Personal Income in Metropolitan and Non-metropolitan Areas," *Survey of Current Business*, 50 (May, 1970), 22-36.

available and are quite similar to those used in the original Kramer analysis. The Republican percentage of the two-party vote was calculated as the Republican percentage of the total vote divided by the sum of the Republican and Democratic percentages of the total vote, the quotient of which was multiplied by 100.

2. *Consumer Price Index 1919-70 (1967=100)*: Table 121, p. 276, *Handbook of Labor Statistics 1972*, U.S. Bureau of Labor Statistics, Washington, D.C. 1972.
3. *Total Population 1919-70*: Series A114, p. 200, *Long Term Economic Growth, 1860-1970*, U.S. Bureau of Economic Analysis, Washington, D.C. 1973.
4. *Income*:

A. 1895-1918—Per capita Gross National Product in constant 1958 dollars. Obtained from Series All, p. 182, *Long Term Economic Growth, 1860-1970, (LTEG)*, U.S. Bureau of Economic Analysis, Washington, D.C., 1973.

B. 1919-1928—Total personal income in current (nondeflated) dollars. Obtained from Series A37, p. 188, *LTEG*.

C. 1929-1970—Total personal income in current (nondeflated) dollars. Obtained from the *Survey of Current Business*, 53 (July, 1973), Table B, p. 52.

D. Real per capita annual income (INC) was estimated as  
For 1895-1918:

INC = per capita Gross National Product in 1958 dollars.

For 1919-1970:

$$INC = (TPINC \cdot 100) / (POP \cdot CPI),$$

where

INC = Real per capita income in 1967 dollars.

TPINC = Total current personal income.

POP = Total population.

CPI = The consumer price index (1967=100).

E. Annual percentage change in real per capita income the year preceding each election was then obtained as

$$\Delta INC = (INC - INC(-1)) / INC(-1),$$

where

$\Delta INC$  = Annual percentage change in real per capita income.

INC = Real per capita income the year of each election.

INC(-1) = Real per capita income the year prior to each election.

#### State-Level Analysis

1. *Percentage of the total vote, by party, U.S. House of Representatives Elections, 1930-70*: Paul T. David, *Party Strength in the United States 1872-1970*, University Press of Virginia, Charlottesville, Va., 1972. (Cal. data on p. 103, N.Y. data on p. 213, Penn data on p. 237). The Republican percentage of the two party vote was calculated as the Republican percentage of the total vote divided by the sum of the Republican and Democratic percentages, the quotient of which was multiplied by 100.

2. *Voter Registration by Party*:

A. California (for 1922-1960) Eugene E. Lee, *California Votes 1928-60*, Table 2-1, p. 29, (Berkeley, Calif.: Institute of Governmental Studies, 1963); (for 1962-70) *Report of Registration*, published for each general election by the California Secretary of State.

B. Pennsylvania—*The Pennsylvania Manual* (Harrisburgh: Commonwealth of Pennsylvania), 1925-26 through 1970-71 volumes. Prior to 1936, registration was not required in some rural areas. Since 1936, the data are statewide totals.

C. New York—*Legislative Manual of New York* (Albany: Secretary of State), 1921 through 1971-72 volumes.

3. *Income*:

A. 1929-47—Per capita personal income in current (nondeflated) dollars. Table 2, pp. 142-45, *Personal Income by States Since 1929*, Office of Business Economics, U.S. Dept. of Commerce, Washington, D.C. 1956.

B. 1948-58—Per capita personal income in current (nondeflated) dollars. Table F, pp. 33-4, *Sensitivity of State and Regional Income to National Business Cycles* (supplemental material in a reprint of the April, 1973 *Survey of Current Business*).

C. 1959-70—Per capita personal income in current (nondeflated) dollars. Table 2, p. 43, *Survey of Current Business*, 53 (Aug. 1973).

D. Real per capita income (INC) was estimated as

$$INC = (PINC \cdot 100) / CPI,$$

where

INC = Real per capita income in constant 1967 dollars.

PINC = Per capita current income.

CPI = The aggregate national consumer price index (1967=100). Separate price indices for each



state were not available for the entire period.

E. Annual percentage change in real per capita income the year preceding each election was then obtained as

$$\Delta \text{INC} = (\text{INC} - \text{INC}(-1))/\text{INC}(-1),$$

where

$\Delta \text{INC}$  = Annual percentage change in real per capita income.

$\text{INC}$  = Real per capita income the year of each election.

$\text{INC}(-1)$  = Real per capita income the year prior to each election.

## APPENDIX B

### Estimation of the Weights for the Registration Proxy

The registration proxy is a weighted moving average of the Republican share of the two-party vote. Its weights are defined as the coefficients ( $B_1$  and  $B_2$ ) of the linear regression (Equation B1) of the Republican share of the two-party registration (REG) on the Republican share of the two-party vote in each of the two preceding U.S. House elections [ $\text{VOTE}(-1)$  and  $\text{VOTE}(-2)$ ].

Equation B1. *Predicting Registration by Past Election Results*

$$\text{REG} = a + B_1(\text{VOTE}(-1)) + B_2(\text{VOTE}(-2)) + e,$$

where

REG = The Republican percentage of the two-party registration.

$\text{VOTE}(-1)$ ,  $\text{VOTE}(-2)$  = The Republican percentage of the two-party vote in the first and second preceding U.S. House elections respectively.

$B_1$ ,  $B_2$  = The regression coefficients or weights of  $\text{VOTE}(-1)$  and  $\text{VOTE}(-2)$  respectively.

$a$ ,  $e$  = The intercept and disturbance terms respectively.

This regression was estimated for California, Pennsylvania, and New York from time-series data on election results and voter registration by party maintained by each state. States were chosen according to the following criteria:

*Size*—Large states were needed to provide enough House districts to average out idiosyncracies arising

from such factors as specific personalities and dominant local party organizations.

*Length of Time Series*—Registration time-series by party vary by state from zero to over twenty elections. California, Pennsylvania, and New York have the longest series available, with the exception of Oregon, which has too few House districts for use in the analysis (California begins in 1922, Pennsylvania begins in 1925, New York data for consecutive elections begin in 1920, and Oregon begins in 1908).<sup>B1</sup>

*Variation Across States and Time*—The greater the variation in registration and voting behavior, the more generalizable are the estimates of the weights.

The analysis is summarized in Table B1. Each column in the table represents an estimate of Equation B1. The first three columns list results for each state. The coefficients, for  $\text{VOTE}(-1)$  and  $\text{VOTE}(-2)$  are the relative weights used to combine the variables to calculate the registration proxy. This linear combination “predicts” a large proportion of the variation in observed registration. For example, a new variable defined as  $[0.71 \cdot \text{VOTE}(-1) + 0.23 \cdot \text{VOTE}(-2)]$  “predicts” 75 per cent of the variation in California registration.<sup>B2</sup>

Coefficient estimates are remarkably consistent with each other. The appropriate  $t$ -test indicates that they do not significantly differ by state. Since we had no a priori reason for expecting them to differ, we pooled all observations into a joint sample from which Equation B1 was estimated.<sup>B3</sup> These pooled estimates (column 4 in Table B1) yield weights of 0.69 and 0.36 for  $\text{VOTE}(-1)$  and  $\text{VOTE}(-2)$  respectively. Thus the registration proxy used throughout the analysis of the national data was defined as  $[0.69 \cdot \text{VOTE}(-1) + 0.36 \cdot \text{VOTE}(-2)]$ .

Table B1 also demonstrates that only the results of the past two elections are required to predict voter registration at any given point in time.<sup>B4</sup>

<sup>B1</sup> Oregon increased from two to four House districts during this period.

<sup>B2</sup> The unadjusted  $R^2$  for the California equation is 0.75. Because they incorporate degrees of freedom, the adjusted  $R^2$ 's shown in the table are underestimates of the actual proportion of the registration variation “predicted” by each regression. For further discussion of adjusted  $R^2$ , see J. Johnston, *Econometric Methods*, 2nd edition (New York: McGraw-Hill, 1972), pp. 129–130.

<sup>B3</sup> The standard errors of estimate of the regressions for each state were statistically significantly different from each other, according to the appropriate  $F$ -test. Thus to obtain the most efficient estimators, observations for each state in the pooled sample were weighted in inverse proportion to the standard error of the separate regression for that state.

<sup>B4</sup> Estimates also indicate that the weights attached to additional elections decrease rapidly as the time between the election and registration figures increases.

Table B1. Republiation Share of Registration Estimated from Republican Share of Congressional Vote in Preceding Elections

(California 1922-70, Pennsylvania 1926-70, New York 1920-70)

	California ( <i>N</i> =25)	Pennsylvania ( <i>N</i> =23)	New York ( <i>N</i> =26)	All Three States Pooled <sup>c</sup> ( <i>N</i> =74)
Coefficients of:				
Vote, 1st Election Prior to Registration [VOTE(-1)]	0.71** (4.5) <sup>a</sup>	0.74** (5.8)	0.58** (3.6)	0.69** (8.1)
Vote, 2nd Election Prior to Registration [VOTE(-2)]	0.23 (1.5)	0.44** (3.5)	0.49** (3.1)	0.36** (4.2)
California [CAL] <sup>b</sup>				-0.09** (-5.5)
Pennsylvania [PENN] <sup>b</sup>				0.05** (4.4)
Intercept	-0.06 (-0.9)	-0.05 (-1.0)	-0.05 (-0.6)	-0.03 (-1.0)
<i>DW</i>	1.1	0.7	2.3	1.3
Adjusted <i>R</i> <sup>2</sup>	0.73	0.87	0.63	0.82
Adjusted <i>R</i> <sup>2</sup> , Past 6 Elections <sup>d</sup>	0.70	0.87	0.68	0.81

\*\* Coefficients and intercepts bearing "\*\*\*\*" are significant at the .01 level, two-tail; all others do not even attain significance at the .05 level, two-tail.

<sup>a</sup> All *t*-statistics are in parentheses below parameter estimates.

<sup>b</sup> CAL and PENN identify the state for each observation (CAL=1, PENN=0 for California; CAL=0, PENN=1 for Pennsylvania and CAL=0, PENN=0 for New York).

<sup>c</sup> Observations for each state are weighted in inverse proportion to the standard error of the separate regression for that state.

<sup>d</sup> Obtained from regressing current registration on the results of each of the past 6 congressional elections.

Additional past elections increase "predictive" ability very little. Comparison of the adjusted *R*<sup>2</sup> for the equations containing two prior elections with those containing six elections clearly illustrates this point. Adjusted *R*<sup>2</sup> explicitly trades off the gain in "predictive" ability against the loss of

degrees of freedom caused by adding explanatory variables. For some of the estimates in the table, the loss of degrees of freedom outweighs the increase in "predictive" ability. In these cases, adjusted *R*<sup>2</sup> decreases when going from two to six elections.

# Comment on Arcelus and Meltzer, The Effect of Aggregate Economic Conditions on Congressional Elections\*

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Arcelus and Meltzer begin with a quite plausible point, that short-term economic fluctuations may affect voter participation as well as partisan preference. Their empirical findings differ markedly from those of several earlier studies,<sup>1</sup> and if correct would considerably change our current understanding of the effects of economic variables on elections. It seems to us, however, that upon closer examination, their major inferences are not supported by the evidence they present. There are serious problems with their model, the data they apply it to, the statistical method they use to estimate it, and the way they interpret their results. We shall review some of these difficulties and show that their principal conclusions are not substantiated by their evidence, and that their results in fact show very little, one way or the other, about the effects of economic conditions on congressional elections. When some of the problems inherent in their research design are remedied, reanalysis of the model leads to quite different conclusions, which substantially (though not en-

tirely) confirm those of the earlier studies cited above. Before presenting these results, however, we first consider some more basic matters.

## Long-Term Changes in Participation

Figure 1 below shows the aggregate participation rate in congressional elections over the period 1896-1970. This is essentially the *VP* series used by Arcelus and Meltzer in their paper.<sup>2</sup> (The *VR* and *VD* series are similar.) Several broad trends are apparent. Participation declined rapidly from 1896 to 1912, dropped abruptly in 1920, then rose steadily until World War II. It dropped again during the War, then in the postwar period increased steadily until the 'sixties, and has declined somewhat since then. Though the precise reasons for these secular tendencies are not completely known, it seems clear that major historical and demographic forces, rather than short-term economic or other factors, are largely responsible.<sup>3</sup> In order to properly measure the impact of short-term forces, it is necessary to control and compensate for these longer-term historical forces, which would otherwise contribute most of the variation in the series and confound the estimates of short-term effects.

## The Arcelus-Meltzer model in effect controls

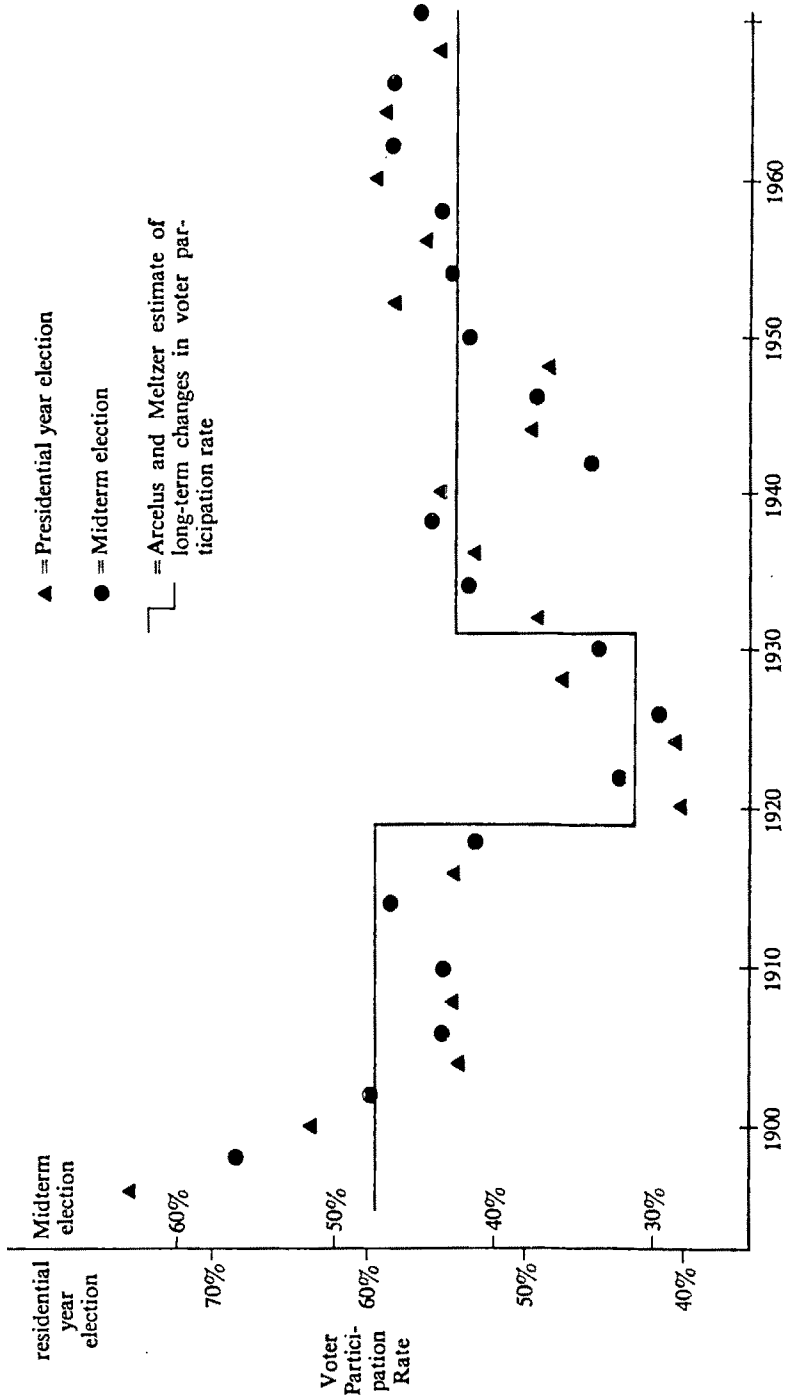
<sup>2</sup> Arcelus and Meltzer draw their participation data for 1920-1970 from the *Statistical Abstract of the United States* (1971). We used a later, revised series for 1930-1972, which is evidently based on new census data or analysis. We have also accounted for the gradual extension of women's suffrage before 1920. See Appendix.

<sup>3</sup> Among the forces at work were expansions of the electorate, the effects of immigration, changes in ballot forms and election laws, and the growth of literacy and the mass media. See for example, W. D. Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 54 (March, 1965), 7-28; Phillip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Phillip E. Converse (New York: Russell Sage Foundation, 1972); W. D. Burnham, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate,'" *American Political Science Review*, 68 (September, 1974), 1002-1023, and comments by Phillip E. Converse and J. G. Rusk.

\* The research underlying this paper was done at the Cowles Foundation for Research in Economics at Yale University; and supported by grants from the National Science Foundation and Ford Foundation. We are indebted to Susan J. Lepper and David R. Mayhew for helpful comments, and to Allan Meltzer for generously providing us with their data.

<sup>1</sup> Including G. H. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896-1964," *American Political Science Review*, 65 (March, 1971), 131-143; G. H. Kramer and S. J. Lepper, "Congressional Elections," in *The Dimensions of Quantitative Research in History*, ed. W. O. Aydelotte, A. G. Bogue and R. W. Fogel (Princeton, N.J.: Princeton University Press, 1972), pp. 256-281; S. J. Lepper, "Voting Behavior and Aggregate Policy Targets," *Public Choice*, 18 (1974), 67-81; E. R. Tufte, "The Determinants of the Outcome of Midterm Congressional Elections," *American Political Science Review*, 69 (September, 1975). In his original study Kramer found that real income fluctuations affected the incumbent party's vote, but that unemployment and inflation did not. There was a data error in the real income series used in the original article (discovered by George Stigler), however; the revised data indicate that both income and inflation are important electoral determinants. A corrected set of results is reported in Bobbs Merrill reprint (PS-498) of the original Kramer article.

Figure 1. Voter Participation Rate (House Vote/Total Eligible Vote), 1896-1970<sup>a</sup>.



<sup>a</sup> Omitting 1912. Scales of midterm and presidential year elections adjusted to equate the means of the two series.

for long-term trends by incorporating two dummy variables, for the post-1920 and -1932 periods. This specification implicitly assumes that voter participation has fluctuated around relatively stable levels, with abrupt shifts to new levels in 1920 and again in 1932, as shown in Figure 1. Yet inspection of the actual participation series indicates that most of the major changes have been gradual trends, persisting over a series of elections, rather than the abrupt shifts they posit. The residuals generated by their various equations are not random (this is clear from visual inspection of the residuals, and also from the values of the Durbin-Watson statistic [a measure of the correlation between adjacent residuals] they report), as they should be if the model were correctly specified, but display systematic patterns. It seems clear that the long-term model is misspecified, and hence that the Arcelus-Meltzer estimates of the long-term partisan shares are incorrect. It is not surprising that they are unable to reconcile their estimates with survey data findings on partisanship (p. 1237).

The misspecification of the long-term part of the model may well also affect the estimates of short-term factors. Since it introduces systematic discrepancies between real and estimated long-term trends in participation, whatever genuine short-term fluctuations are present will be confounded with these spurious discrepancies. Misspecification of one part of the model can, in general, lead to biased estimates of all coefficients.<sup>4</sup> It is no simple task to model explicitly the complex and changing mix of social, demographic and historical forces which underlie long-term changes in participation, and there may be no easy way to correct for these biases; it may be, in fact, that longitudinal participation data are simply too "noisy" to permit accurate measurement of short-term effects. In any event, there is a potentially

serious specification problem with the Arcelus-Meltzer model, and their results (including their estimates of short-run effects) should be regarded with caution.

### Why, and How, Should Economic Variables Affect Participation?

The starting point of the Arcelus-Meltzer analysis of short-term effects is their model of "rational" abstention: thus, "Our model differs from most previous studies . . . by permitting voters to abstain. . . . If our hypothesis is correct, a main effect of aggregate economic variables comes by changing the participation rate." In developing these hypotheses, they suggest three ways in which economic (or other) factors may induce voters to vote: "(1) because they perceive a difference between . . . parties on a particular issue, (2) they wish to protest against a particular . . . outcome, or (3) they wish to reward a party . . . for a policy or outcome. . . . In each of these cases, the net benefit of voting . . . increase[s] during elections in which the issue arises." Presumably the idea behind (1) is that some potential voters consider one of the parties—the Republicans, for example—best qualified to deal with a particular issue, such as inflation. Then in an election in which inflation "arises" as an issue—presumably because it is too high—these individuals will be more likely to vote. By the same reasoning this "perceived differences" hypothesis would also predict increased turnout when unemployment is high, and when the growth rate falls. These predictions are summarized in Figure 2 below. The second hypothesis, (2), leads to the same predictions: "protesters" are more likely to vote when conditions are bad, i.e., when inflation and unemployment are high, and the growth rate low. The third hypothesis, however, would predict precisely *opposite* effects, since "rewarders" will be more likely to vote when conditions are good. Arcelus and Meltzer offer no hypotheses about the relative strengths of these three motivations in the electo-

<sup>4</sup> See J. Johnston, *Econometric Methods* (New York: McGraw-Hill, 1972), pp. 168–169.

Figure 2

Variable:	Type of Motivation:		
	(1) "Perceived Differences"	(2) "Protesters"	(3) "Rewarders"
Growth Rate of Real Income	—	—	+
Inflation Rate	+	+	—
Unemployment	+	+	—

Direction of the effects of economic variables on voter participation predicted by Arcelus and Meltzer's hypotheses; +, — indicate that increases in the economic variable in question will increase or decrease turnout, respectively.

rate as a whole, or within any subgroup, such as partisans of either party. In the absence of such information, their theoretical hypotheses do not lead to any clear predictions about the net effects of economic conditions on participation. If "rewarders" are the most numerous group, the effects of the economic variables should be of the sign in column (3); if "rewarders" and others approximately offset each other, economic variables will have *no* net effect; while if protest and/or perceived differences are the prevalent motivations, the effects should be as in columns (1) and (2). Thus any possible set of empirical findings can be reconciled with their hypotheses: *there is no conceivable way in which their evidence could possibly falsify their model of the voting process.*

#### Where Are the Shift Voters?

Arcelus and Meltzer draw the surprising conclusion that "fluctuations in . . . congressional elections arise . . . not from shifts between parties." It seems to us, however, that their results do not support this conclusion, and in fact that there is no possible way of inferring the magnitude of shifting from the type of evidence they use. Their model proceeds by partitioning the eligible electorate into three groups: (1) "Habitual, partisan voters," who constitute  $H_t$  of the electorate in election  $t$ ; (2) a group  $N_{1t}$  of "voters induced to vote by the reduction in cost or increase in perceived benefits resulting from presidential elections and incumbency"; and finally a block  $N_{2t}$  of voters "for whom the net benefit of voting is a function of aggregate economic performance." The participation rate of this last block is assumed to be a linear function of certain macroeconomic variables. In extending this framework to a model of partisan choice, they take the partisanship of the first two groups as fixed (the Democratic shares of each block of voters being  $\alpha_D$  and  $\beta_D$ , respectively), while that of the third group of voters, those motivated to vote because of concern for economic issues, "varies with incumbency," presumably because of differential turnout effects. Nowhere in this framework is there any provision for a block of "shift" voters, who regularly vote but switch from one party to the other in response to economic or other issues. Ignoring this possibility seems dubious empirically, for evidence from other sources indicates that such voters do exist and that they constitute a sizable portion of the active electorate.<sup>5</sup> In any event, since their model makes no provision for the possibility of shift voters, it is not entirely surprising that they do not find any.

To explore this issue further, let us add to the

Arcelus-Meltzer model a fourth block of voters, who always vote but shift from party to party. Let  $S$  be the size of this block (as a fraction of the eligible electorate), and  $vr_t$  and  $vd_t$  be the Republican and Democratic shares, respectively, of these votes in election  $t$ . Since these voters always vote they will not affect total participation ( $S$  will be incorporated into the intercept of the  $VP$  equation), but they will affect the relative sizes of the Democratic and Republican shares. To simplify, let us suppose there is only a single relevant economic variable,  $\dot{r}$  (the growth in per capita real income), and that the voters in  $S$  vote for or against the incumbent party according to its economic performance. Then  $vd_t$  will be given by<sup>6</sup>

$$vd_t = c_1 - c_2\delta_t - c_3\delta_t\dot{r}_t + e_t,$$

where  $\delta_t$  is +1 or -1 according to whether the incumbent president is a Republican or Democrat, and the coefficient  $c_3$  is positive if voters do indeed shift in the manner described above. Since  $\delta_t = 2RI_t - 1$ , we can rewrite this expression in terms of the  $RI$  variable, after some elementary manipulation, as

$$vd_t = (c_1 + c_2) - 2c_2RI_t + c_3\dot{r}_t - 2c_3RI_t\dot{r}_t + e_t.$$

If we now add this block of voters to Arcelus-Meltzer's equation (3) (still assuming only one economic variable), the Democratic vote becomes

$$\begin{aligned} VD_t &= [\dots] + d_4\dot{r}_t + \Delta d_4RI_t\dot{r}_t \\ &\quad + vd_t \cdot S + e_t \\ &= \{[\dots] + (c_1 + c_2)S\} \\ &\quad - (2c_2S)RI_t + (d_4 + c_3S)\dot{r}_t \\ &\quad + (\Delta d_4 - 2c_3S)RI_t\dot{r}_t + e_t, \end{aligned}$$

(where  $[\dots]$  is the block of terms in their expression not involving economic variables). This expression involves nearly the same set of explanatory variables as their expression (3), the sole difference being the incumbency variable  $RI_t$ . The coefficient of this variable is a measure of the net advantage of incumbency, and both *a priori* considerations and other empirical evidence suggest this effect should be small, so that the inclusion or deletion of the  $RI_t$  term should have little effect on the remaining estimates.<sup>7</sup> Hence the results in their Table 1 can be interpreted in terms of this expanded model which includes switch voters.

A fundamental difficulty, however, is immediately apparent: the regression coefficient of the  $\dot{r}_t$

<sup>5</sup> For example, V. O. Key, *The Responsible Electorate* (Cambridge: Belknap Harvard, 1966).

<sup>6</sup> This is the specification used in Kramer, "Short-Term Fluctuations," but others would do as well.

<sup>7</sup> *Ibid.*, p. 139.

term, for example, is an estimator of  $(d_4 + c_3S)$ , where  $d_4$  is the true (but unknown) "participation" coefficient, and  $c_3S$  is the true "shift" coefficient. To test the hypothesis of a shift effect we must determine whether  $c_3S$  is positive. Yet the regression coefficient is an estimate of  $(d_4 + c_3S)$ , not  $c_3S$  alone; and as noted earlier, the participation coefficient can be positive, negative, or zero, since the Arcelus-Meltzer hypotheses place no constraints on it. Thus even if there were substantial switching, the estimated regression coefficient could be negative (or zero) if the participation effect were negative. Or conversely, it could be positive even if there were no shifting. The "shift" effect is not *identified*<sup>8</sup> in the Arcelus-Meltzer model; in the absence of other data or *a priori* information, there is no possible way of inferring its magnitude from their evidence.

#### Do Economic Factors Affect the Parties' Strengths?

On this broader question, Arcelus and Meltzer conclude that "with the possible exception of inflation, aggregate economic variables [do not] affect . . . the relative strengths of the two major parties. There is very little evidence that an incumbent president can affect the composition of the Congress by measures that have short-term effects on unemployment or real income." "Neither our model . . . nor the evidence implies that voters respond to short-term changes in employment or real income by voting for, or against, the party in power." This "null result" is based on the fact that the estimated coefficients of most of the economic variables are not significant and display no significant differential effects on the Democratic and Republican share of the two-party vote.

In several respects the evidence seems inadequate to support these conclusions. There are serious problems with the Arcelus-Meltzer data, and in particular with the unemployment and income variables (which we will examine below). This alone could account for their nonfindings on those variables. Quite apart from that, however, Arcelus and Meltzer's analysis of their results does not take adequate account of several rather basic matters of statistical inference. For one thing, multicollinearity among the economic variables makes it difficult to disentangle their individual effects, and tends to produce large standard errors for the individual estimates. Thus even if economic variables as a block had a highly significant

effect, it is possible for all of the estimated coefficients to be individually insignificant.<sup>9</sup> Arcelus and Meltzer make no attempt to examine this possibility.

There is another important problem. Since the net effect of any economic variable on the parties' vote shares depends on the *difference* between the coefficients associated with that variable in the *VD* and *VR* equations, the relevant question is whether this *difference* is significantly different from zero, not whether the individual coefficients are. Since the disturbances in the two equations are positively correlated,<sup>10</sup> the corresponding estimated coefficients of any particular variable will also be positively correlated.<sup>11</sup> Under these conditions the standard error of the difference between the two coefficients can be smaller than that of either coefficient alone,<sup>12</sup> and the difference itself could be statistically significant even if the individual estimates were not. Arcelus and Meltzer do not consider this possibility, either.

Finally, there is a more fundamental point of basic statistical inference. The fact that a certain estimate is not significantly different from zero by no means shows that the variable has no effect. Lack of significance means only that the range of possible error, or confidence interval, associated with the estimate is large enough to include zero. When this is so, the data are indeed consistent with the possibility that the true effect is small (or zero), but they may be equally consistent with the possibility of very large effects. If a confidence interval is small and centered about zero, so that the possible values of the coefficient consistent with the data all correspond to small effects, then one might indeed conclude from the evidence that the effect in question is absent, or negligible; but if the interval is very large (and happens to include zero), the appropriate inference is not that the variable has no effect, but rather, that the results leave a great deal of uncertainty about its possible effects.

The range of uncertainty in the Arcelus-Meltzer estimates is considerable. For example, if we consider the two estimates of the *RIC/p* terms in their Table 1, the usual .95 confidence intervals would run from about -1.02 to .84 for the coefficient in the *VD* equation, and from -.72 to .58 for the *VR* equation. Thus (ignoring the nonindependence of the estimates), while these results are consistent with the null hypothesis that there is no income effect (i.e., that both coefficients are approximately zero), they are equally consistent

<sup>8</sup> For a discussion of the identification problem in a slightly different context, see T. C. Koopmans, "Identification Problems in Economic Model Construction," in *Studies in Econometric Method*, ed. W. C. Hood and T. C. Koopmans (New York: Wiley, 1953).

<sup>9</sup> See Johnston, *Econometric Methods*, p. 159ff.

<sup>10</sup> The positive correlation is noted by Arcelus and Meltzer (footnote 17, p. 11).

<sup>11</sup> This follows from Johnston, pp. 239-241.

<sup>12</sup> Since  $\text{Var}(b - b') = \text{Var}(b) + \text{Var}(b') - 2\text{Cov}(b, b')$ , for any two estimators  $b$  and  $b'$ .

with the hypothesis of very sizable effects. For example, if the true values were  $-.4$  and  $+.4$  respectively (both lying well within their respective confidence intervals), a 10 per cent increase in real compensation per man-hour would decrease the democratic vote by some 4 per cent of the eligible electorate, and increase the Republican vote by the same amount. In a midterm election with total participation of around 40 per cent, this represents the difference between a 50-50 election and a Republican landslide of 60 per cent—a very major effect indeed. It seems to us that the main inference to be drawn from the results in the Arcelus-Meltzer Table 1 is that they leave a great deal of uncertainty about the nature and magnitude of the possible effects of economic variables on election outcomes—they simply do not shed much light on the question one way or the other.

This inconclusiveness, however, is in part the product of several serious problems with their data and estimation method, which we shall now review.

#### Data and the Specification of Variables

While Arcelus and Meltzer have corrected a data error in the  $\dot{C}/p$  series that we discovered in an earlier draft of their paper, we are still unable fully to replicate their results. We have carefully attempted to reconstruct their series using the sources and definitions given in their appendix, and have also used their own original data (with corrections in their  $\dot{C}/p$  series). With either data set, however, we find a number of discrepancies, in some cases substantial, between our estimates and those reported in their Table 1.<sup>13</sup> Some of these discrepancies may reflect errors which remain in their data.

Possible errors aside, moreover, the entire Arcelus-Meltzer analysis of the effects of economic variables is seriously weakened by their peculiar specifications of the economic variables, especially their choice of the "income" variable. Arcelus and Meltzer themselves comment that "rational voters are concerned with their real income," and argue for the importance of including "real income, or in an expanding economy the

growth rate of real income" as a variable. This is certainly plausible, since a variable like (per capita) real personal income is a measure of the average level of goods and services available for consumption, and hence reflects the average state of material well-being or prosperity in the electorate. Previous studies of the effects of macroeconomic conditions on voting have used such a variable, and have found it to be empirically important. Yet in passing from their general theoretical discussion to the particular equations they actually estimate, Arcelus and Meltzer inexplicably abandon real income, and change to quite a different variable, "real compensation per man-hour" (in nonagricultural establishments). The reasoning behind this change is obscure, and the compensation variable seems to us suspect in several ways. It is a measure of the average hourly wage (compensated for inflation) of those who work. Hence it ignores the unemployed: if in a recession low-wage workers are the first to be laid off, then compensation per man-hour would rise, even as the average level of prosperity in the country declined. It also takes no account of the number of hours worked: if the country were obliged to change over temporarily to a four-day work week, as Britain recently was, compensation per man-hour would be unchanged, though obviously incomes would fall. It takes no account of the agricultural sector, which was sizable in the earlier part of the sample period. All in all, compensation per man-hour seems to us a very poor measure of overall prosperity. A variable like per capita real personal income would have been a better choice.

The treatment of unemployment is likewise puzzling. They use, again without explanation, the *rate* of change in the unemployment rate. The rationale for this is unclear, since unemployment is already a rate (being corrected for changes in the size of the labor force), and does not show any long-term tendency to grow (unlike national income or the price level). On the Arcelus-Meltzer specification, the electorate should be equally concerned with unemployment if it increased from 5 to 6 per cent in the year of the election, or from .5 to .6 per cent, or from 50 to 60 per cent of the labor force—which seems most implausible. The unemployment rate itself (or perhaps the [absolute] change in that rate) would seem to be a more sensible variable.

<sup>13</sup> Using the original Arcelus-Meltzer data (with a corrected  $C/P$  series), we reestimated the  $VP$ ,  $VD$  and  $VR$  equations. The salient discrepancies between the two sets of estimates are summarized in the table below.

	Equation	$VP$		$VR$			
	Coefficient	$RIPR$	$\dot{p}$	$\dot{p}$	$\dot{C}/p$	$R\dot{p}$	$R\dot{C}/p$
Our Parameter Estimate		$-3.50$	$-0.39$	$-0.11$	$0.12$	$0.50$	$-0.39$
( <i>t</i> -statistic in parentheses)		(1.34)	(1.90)	(0.83)	(0.61)	(1.89)	(0.98)
Arcelus-Meltzer Parameter Estimate		$0.76$	$-0.23$	$-0.03$	$0.04$	$0.38$	$-0.07$
( <i>t</i> -statistic in parentheses)		(0.26)	(1.11)	(0.21)	(0.19)	(1.63)	(0.21)



The Arcelus-Meltzer definition of the rate of inflation is also a bit odd. If the consumer price index doubled during the year preceding the election (e.g., rose from 125 to 250), the inflation rate would be 50 per cent of their definition.<sup>14</sup> According to the usual definition, however, this should be counted as a 100 per cent inflation. Arcelus and Meltzer offer no reason for their convention, and it seems to us the usual definition is preferable.

It is not clear why the  $RI_tPR_t$  variable is included in the regression at all. Arcelus and Meltzer offer no explicit justification for including it, and their hypotheses about voting costs being lower during presidential than in midterm elections in no way suggest that this reduction should depend on which party holds the White House. Since this variable will be correlated with the  $RI_t\dot{p}_t$ ,  $RI_t\dot{u}_t$ ,  $RI_t\dot{C}/p_t$  terms, the effect of including it is to introduce more multicollinearity and decrease the accuracy of the estimates.

Finally, it is arguable that wartime elections should not have been included (or at least should have been handled differently to allow for the possibility of structural shifts in the relations being estimated), since the dislocations of a major war affect the meanings of the economic series and reduce the political importance of domestic economic (and other) issues.

### Methods

The Arcelus-Meltzer estimates of the effects of economic variables on the parties' vote shares are obtained by separately estimating the  $VD$  and  $VR$  equations, using ordinary least-squares regression. In a footnote they comment that these estimates, though unbiased, "are not as asymptotically efficient as . . . the [Zellner 'seemingly unrelated regressions'] estimators." An inefficient estimator yields estimates which are unnecessarily imprecise, and thus makes it less likely that accurate and significant estimates of the effects in question will be obtained.

The Arcelus-Meltzer estimates are indeed inefficient, though not for the reason they mention.<sup>15</sup> Their procedure ignores certain *a priori* information and constraints on the coefficients, which are implied by their theoretical hypotheses and model. In particular, their model takes the form of a system of equations:

<sup>14</sup> Arcelus and Meltzer define the rate as  $(p_t - p_{t-1})/p_t$ , rather than the conventional specification  $(p_t - p_{t-1})/p_{t-1}$ .

<sup>15</sup> The  $VD$  and  $VR$  relations involve the same set of regressors. Under these conditions the Zellner procedure reduces to the one used by Arcelus and Meltzer, of separate OLS estimation of each individual equation. Cf. Johnston, *Econometric Methods*, p. 240.

$$VP_t = H_t + N_{1t} + N_{2t} + e_t$$

$$VD_t = \alpha_D H_t + \beta_D N_{1t} \\ + [\gamma_D + \Delta\gamma_D RI_t] N_{2t} + e'_t$$

$$VR_t = \alpha_R H_t + \beta_R N_{1t} \\ + [\gamma_R + \Delta\gamma_R RI_t] N_{2t} + e''_t,$$

where  $H_t$ ,  $N_{1t}$  and  $N_{2t}$  are the sizes of the three blocks of voters (habitual partisans, voters induced to vote by the reduction in voting costs in presidential elections, and voters induced to vote by economic issues, respectively) in election  $t$ , expressed as shares of the eligible electorate. The coefficients  $\alpha_D$  and  $\alpha_R$ ,  $\beta_D$  and  $\beta_R$ , etc. are the Democratic and Republican shares of the votes cast by voters in the various blocks. Since they are shares, or fractions, each must lie between zero and one, and they must sum to one, minus the third-party vote (i.e.  $\alpha_D + \alpha_R = 1 - \alpha_T$ ,  $\beta_D + \beta_R = 1 - \beta_T$ , etc., where  $\alpha_T$ ,  $\beta_T$ , etc. are the third-party votes cast in each block). For most of the period under consideration, the aggregate congressional third-party vote has been quite small, and it is reasonable to assume that the sum of major-party coefficients is approximately unity. Thus the coefficients of the "economically motivated" block, in particular, must satisfy  $\gamma_D + \gamma_R = 1$  and  $\gamma_D + \Delta\gamma_D + \gamma_R + \Delta\gamma_R = 1$ , implying  $\Delta\gamma_D = -\Delta\gamma_R$ . If these facts are substituted into the  $VD$  and  $VR$  equations (still assuming only one economic variable,  $\dot{r}$ ), we get

$$VP_t = [\dots] + a\dot{r}_t + e_t \\ VD_t = [\dots] + (\gamma_D + \Delta\gamma_D RI_t)a\dot{r}_t + e'_t \\ = [\dots] + (\gamma_D a)\dot{r}_t + (\Delta\gamma_D a)RI_t\dot{r}_t + e'_t \\ VR_t = [\dots] + (\gamma_R a)\dot{r}_t + (\Delta\gamma_R a)RI_t\dot{r}_t + e''_t \\ = [\dots] + ([1 - \gamma_D]a)\dot{r}_t \\ + (-\Delta\gamma_D a)RI_t\dot{r}_t + e''_t$$

where  $[\dots]$  is the block of terms not involving economic variables. The model implies that the coefficients of the  $\dot{r}$  terms in the  $VD$  and  $VR$  equations must be of the *same* signs, and their sum must (approximately) equal the coefficient of the  $\dot{r}$  term in the  $VP$  equation. Moreover the coefficients of the  $RI\dot{r}$  terms must be of *opposite* signs, and approximately equal in magnitude. Yet the results reported by Arcelus and Meltzer violate all three conditions: the signs of the  $\dot{U}$  and  $\dot{C}/p$  estimates in the  $VD$  and  $VR$  equations, which should agree, are opposite, while the  $RI\dot{p}$  and  $RI\dot{C}/p$  estimates, which should be opposite (and equal), are of the same signs. Moreover the  $\dot{p}$  estimates in the  $VD$  and  $VR$  equations sum to  $-.55$ , rather

than  $-.23$ , as they should, and the sum of the  $\hat{C}/p$  estimates is also off.

Arcelus and Meltzer implicitly recognize some of these constraints in interpreting their results, and in their estimation of the  $VP$  relation. (Since the total participation relation is the sum of  $VD$  and  $VR$  equations [plus an unspecified relation for the negligible third-party vote], the coefficients in (1) must be the sum of the corresponding coefficients in (2) and (3). Arcelus and Meltzer do not include terms like  $RI\hat{r}_t$  in (1), presumably in recognition of the fact that these coefficients must sum to zero.) Their procedure for estimating the  $VD$  and  $VR$  equations, however, takes no account of the constraints. By ignoring these constraints, they get inefficient (and indeed mutually inconsistent) estimates.<sup>16</sup> Since several of the constraints are rather badly violated, it is clear that incorporating them would substantially affect their results. The "equal and opposite" constraint on the  $RI\hat{r}$ -like terms, in particular, directly involves the differences between corresponding coefficients in the  $VD$  and  $VR$  equations, so that their estimates of the net effect of these variables on the parties' respective vote shares would be particularly affected.

#### Revised Estimates

To get some idea of the consequences of the various statistical and data problems described above, we present below some revised estimates of the Arcelus-Meltzer model. Our variables and sources are identical to theirs, with the following exceptions:

- (1) Our income variable is the growth rate in per capita real personal income,  $\hat{r}_t = (r_t - r_{t-1})/r_{t-1}$
- (2) The rate of inflation is in the usual fashion as  $\hat{p}_t = (P_t - P_{t-1})/P_{t-1}$ .
- (3) In place of their unemployment variable we have used the unemployment rate (as percentage of the labor force) during the election year,  $u_t$ . We also have experimented with another variable, the (absolute) change during the year preceding the election  $\Delta u_t = u_t - u_{t-1}$ .
- (4) The unjustified  $RI_t PR_t$  term is omitted from all regressions.
- (5) The 1972 election has been added to the sample. Each regression has been estimated twice, first omitting, and then including, the war years 1918, 1942, 1944.

(Precise definitions and sources, where they differ from those of Arcelus and Meltzer, are given in the Appendix.)

To simplify interpretation of the estimates, their model has been rewritten in terms of a

second, Democratic incumbency dummy variable  $DI = 1 - RI$ . Then the Arcelus-Meltzer formulation of the form

$$VR = [\dots] + b_1 \hat{r} + b_2 RI \hat{r} + e,$$

is rewritten as

$$VR = [\dots] + b_1^* DI \hat{r} + b_2^* RI \hat{r} + e.$$

These equations are completely equivalent ( $b_1^* = b_1$  and  $b_2^* = b_1 - b_2$ ), but  $b_1^*$ ,  $b_2^*$  are direct estimates of the impact of the  $\hat{r}$  variable during Republican and Democratic incumbencies respectively, so the interpretation is more straightforward.

To incorporate the linear constraints on the various coefficients discussed earlier, we subtract the  $VR$  and  $VD$  equations, obtaining a relation of the form

$$VR - VD = [\dots] + c_1 RI \hat{r} + c_2 DI \hat{r} + e',$$

where  $c_1$  and  $c_2$  are now estimates of the net effects of  $\hat{r}$  on the parties' vote shares (i.e., of  $\hat{r}$ 's effect on the Republican vote, minus its effect on the Democratic vote), during Republican and Democratic incumbencies, respectively. Estimating this relation and the total participation ( $VP$ ) equation by ordinary (unconstrained) least squares is essentially equivalent<sup>17</sup> to estimating the  $VR$ ,  $VD$ ,  $VP$  system subject to the coefficient constraints. (We also obtained unconstrained estimates using the Arcelus-Meltzer procedure, but will not report these results in detail.) Each equation was estimated four times, with the two different versions of the unemployment term, and first including, and then excluding the war years.

In the interest of brevity, we will not report all results in detail. Since we are primarily interested in the effects of economic variables on the parties' vote shares, we shall concentrate on the  $VR - VD$  equations, and report only the coefficients of the economic variables. These are shown in Table 1.

Since we retain the Arcelus-Meltzer specification of the long-term part of the model, our estimates, like theirs, are probably subject to specification bias. The Durbin-Watson statistic values may be indicative of specification problems (or alternatively of [negative] autocorrelation; in that case our estimates are inefficient, though the estimated standard errors may be too large, and the true significance levels higher than those reported

<sup>17</sup> In principle the  $VR - VD$ ,  $VP$  system are "seemingly unrelated regressions" to which the Zellner GLS procedure should be applied. The covariance between the disturbances, however, is small; if we let  $e_1$ ,  $e_2$ ,  $e_3$ ,  $e^*$  be the disturbances in the  $VR$ ,  $VD$ ,  $VP$ ,  $VR - VD$  relations respectively, then  $e_3 = e_1 + e_2$  and  $e^* = e_1 - e_2$ . Hence

$$\text{Cov}(e^*, e_3) = E[(e_1 + e_2)(e_1 - e_2)] = \sigma_1^2 - \sigma_2^2,$$

where  $\sigma_1^2$  and  $\sigma_2^2$  are the variances of  $e_1$  and  $e_2$  re-

<sup>16</sup> Henri Theil, *Principles of Econometrics* (New York: Wiley, 1971), p. 282ff.

Table 1. Regression Estimates of Effects of Economic Variables on  $VR-VD$ 

War Years	(1) Omitted	(2) Included	(3) Omitted	(4) Included
$RI\hat{r}$ (+)	.015 (.131)	.020 (.130)	.403* (.242)	.413** (.241)
$RI\hat{p}$ (-)	-.136 (.215)	-.145 (.212)	-.008 (.197)	-.019 (.196)
$RIu$ (-)	-.278* (.163)	-.280** (.157)	X	X
$RI\Delta u$ (-)	X	X	.581* (.443)	.587* (.439)
$DI\hat{r}$ (-)	-.185** (.090)	-.146** (.084)	-.0015 (.209)	.011 (.187)
$DI\hat{p}$ (+)	.200* (.148)	.196** (.114)	.286** (.156)	.312*** (.105)
$DIu$ (+)	-.147* (.097)	-.153* (.093)	X	X
$DI\Delta u$ (+)	X	X	.555 (.633)	.431 (.519)
$R^2$	.75	.73	.74	.71
$d$	2.72	2.43	2.46	2.01

Estimated standard errors in parentheses. Intercepts and coefficients of  $X_{20}$ ,  $X_{32}$  and  $PY$  not reported.

(+), (-): Predicted sign of coefficient from "incumbency" hypothesis.

\*\*\*: Significant at .05 (or better) on two-tailed test.

\*\*: Significant at .10 on two-tailed, or .05 on one-tailed, test.

\*: Significant at .10 on one-tailed test.

X: Variable not included in equation.

In Table 1.<sup>18</sup> In any event, the results as reported, though hardly conclusive, generally indicate that economic variables do affect the parties' vote shares. As a block, the economic variables account for from 17 per cent (in equation [1]) to 13 per cent (in equation [4]) of the total variance of the  $VR-VD$  series. This increment to explained variance is significant at the .10 level or better in every case, using the usual F-test. Multicollinearity among the variables tends to produce large standard errors for the individual coefficients, but even so, three or four of the coefficients in each equation approach or reach conventional significance levels. Moreover, the signs of most of the estimates are consistent with the "incumbency" hypothesis, i.e., that voters vote for or against the

incumbent party according to its economic performance. In terms of magnitudes, several of the effects are respectable, though not overwhelming: for example, a 10 per cent inflation rate during a Democratic administration would decrease its plurality by two to three per cent of the eligible electorate, or (in a midterm election with turnout about 40 per cent) decrease its vote share by around 3 per cent of the votes cast. An unemployment level of 10 per cent during a Republican administration would decrease the Republican vote share by a comparable amount.

With respect to individual variables, consider inflation first. Three of the four  $DI\hat{p}$  estimates are significant, and the fourth is close. The  $RI\hat{p}$  estimates are not significant, and their standard errors are so large that they are not very informative, one way or the other, about the effects of inflation during Republican administrations. Nevertheless, the signs of all the estimates suggest that inflation hurts the incumbent party, and in equations (1) and (2) (which, for reasons to be given below, we

spectively. Unconstrained OLS estimation of the  $VR$ ,  $VD$  relations indicates  $\sigma_1^2$  and  $\sigma_2^2$  are nearly equal, so  $\text{Cov}(e^*, e_a)$  will be quite small, and the GLS estimates will be very close to our ordinary least squares estimates. See Johnston, *Econometric Methods*, pp. 238-241.

<sup>18</sup> See for example, Theil, pp. 255-256.

regard as the preferable specification), the magnitudes of the estimated effects are roughly the same, under either administration. (The unconstrained  $VD$  and  $VR$  estimates form a generally similar, though less sharp, pattern: the four significant estimates are in the "incumbency" direction, and while some eight estimates [of 16] were of the "wrong" sign, only one of these even approached significance [its  $t$ -ratio was 1.33].) All things considered, our evidence indicates that inflation does have an effect on congressional elections, with high inflation hurting the incumbent President's party.

Arcelus and Meltzer also found inflation to have some effect, but in a rather different direction. They found that the main effect of inflation is to lower the Democratic vote, and hence to increase the Republican vote share. Writing before the election, they comment that their "findings for inflation suggest that the Republicans will benefit in the nonpresidential year 1974 from their failure to control inflation." We doubt that many Republicans would interpret the election results that way.

Our results also suggest that the income variable affects the parties' vote shares. Two of the  $DI\dot{r}$  estimates and one of the  $RI\dot{r}$  estimates are significant (another is close), and all but one of the estimates are in the "incumbency" direction (the sole exception, the  $DI\dot{r}$  term in equation [4], is very close to zero and insignificant). The estimated effects are not symmetrical (in [1] and [2] the  $RI\dot{p}$  estimates are close to zero, and considerably smaller than the  $DI\dot{p}$  effects), though the estimates are too imprecise for detailed comparisons. In the unreported estimates of the unconstrained  $CD$  and  $VR$  equations, 13 of the 16 income estimates are in the "incumbency" direction. Four of the estimates are significant, and two others nearly so; all but one of these are in the "incumbency" direction. (The one significant estimate which goes in the other direction is in an equation involving the  $\Delta u$  unemployment variable. We will argue below that  $\Delta u$  is the wrong variable to use, so this result should be discounted accordingly.) In any event our results show that income does have an effect and they are generally consistent with the incumbency hypothesis.

The unemployment effects form a rather different pattern. One of the estimates is significant, and five others are close. The estimates are quite sensitive to the form of the unemployment variable; substitution of the change-in-level variable  $\Delta u$  for the level variable  $u$  reverses the signs of the unemployment estimates, and affects several of the other estimates as well. No matter which variable is used, however, the estimates do not conform to the "incumbency" hypothesis. On *a priori* grounds alone, there is a strong case for the

level variable  $u$ ; thus we would expect jobs to be a live political issue whenever the unemployment rate is high, irrespective of whether it is stable, increasing, or decreasing. The results in Table 1 reinforce this view. With the change-in-level variable  $\Delta u$ , the estimated effects are all positive which would imply that increasing unemployment always benefits the Republicans. We are inclined to discount this anomalous result. With the level variable  $u$ , however, the estimated effects are negative, suggesting that the electorate turns to the Democrats in times of high unemployment, which is not implausible. (The unconstrained  $VD$  and  $VR$  estimates with the level variable  $u$  all conform to this pattern also, though none are significant.) We are thus inclined to reject the  $\Delta u$  variable, and accept the results in (1) and (2) as the meaningful ones. They suggest that unemployment does have an effect, which always works in favor of the Democrats. The estimated magnitude of the effect is greater if unemployment occurs under a Republican administration (though once again, the estimates are too imprecise for detailed comparisons).

In sum, our reading of the evidence suggests that all three economic variables do influence congressional elections, and that Arcelus and Meltzer's nonfindings on income and employment can be attributed to the problems with their data and methods. The specific results reported in our Table 1 should be regarded as tentative, since as noted earlier, they may well be subject to bias because of the misspecification of the long-term model. Indeed, we are not convinced that short-term participation effects can be accurately estimated from aggregate participation data, because of the difficulties in controlling for long-term trends and variations in eligibility requirements, registration laws, polling hours, and the like.<sup>19</sup> It may well prove necessary to use disaggregated data at the state or county level, in order to control adequately for such factors. But at a minimum, our reanalysis of the Arcelus-Meltzer model does, we think, confirm the basic finding of earlier studies by Kramer and others, that economic conditions do affect the outcomes of congressional elections, and do so in ways which are broadly (though not completely) consistent with the notion that the electorate rewards or punishes the party in power according to its economic performance. To be sure, there is still substantial uncertainty about the detailed nature and magnitudes of these effects. Thus while most studies are in general agreement about the role of inflation and income, the evidence on unemployment is mixed. And there may yet turn out to be

<sup>19</sup> See, for example, Stanley Kelly, Jr., R. E. Ayres and W. G. Brown, "Registration and Voting: Putting First Things First," *The American Political Science Review*, 61 (June, 1967), 359-377.

important differences in the roles of different economic variables during Republican and Democratic incumbencies, or (as emphasized by Bloom and Price in their commentary) as between "rewarding" or "punishing" an incumbent.<sup>20</sup> But on the basic question of whether such effects exist, it seems to us the evidence is clear: they do.

## APPENDIX

### Data and Sources

The main data sources were *Long Term Economic Growth, 1860-1970* (Washington, D.C.: U.S. Bureau of the Census, 1970), the *Handbook of Labor Statistics* (Washington, D.C.: U.S. Department of Labor, 1973), the *Historical Abstract of the United States* (Washington, D.C.: U.S. Bureau of the Census, 1957), and the *Statistical Abstract of the United States* (Washington, D.C.: U.S. Bureau of the Census, various issues). These sources are referred to below as LTEG, HLS, HAUS and SAUS, respectively.

*Prices.* Consumer price index (1967=100): 1895-1972, HLS (1973), p. 287.

*Unemployment.* Total unemployment rate: 1895-1947, LTEG, pp. 212-213; 1948, 1972, *Economic Report of the President* (GPO, 1974), p. 279.

*Compensation per man-hour.* 1895-1914, Albert Rees, *Real Wages in Manufacturing, 1890-1914* (Princeton: Princeton University Press, 1961), p. 4; 1914-1946, Albert Rees, *New Measures of Wage-Earner Compensation in Manufacturing, 1914-1957* (Washington, D.C.: National Bureau of Economic Research, 1960), p. 3; 1946-1972,

index of compensation per man-hour for all persons in non-agricultural establishments, adjusted to current dollars using 1967 benchmark, HLS (1973), p. 175.

*Personal Income.* Department of Commerce concept: 1895-1919, based on GNP figures in John W. Kendrick, *Productivity Trends in the United States* (Princeton: Princeton University Press, 1961), pp. 298-299, then translated to personal income by a partially interpolated ratio of personal income to GNP, and adjusted to current dollars with implicit price deflator from M. Friedman, and A. J. Schwartz, *A Monetary History of the United States, 1867-1960* (Princeton: Princeton University Press, 1964), Table 62, facing p. 618; 1920-1968, LTEG, p. 189; 1969, *Survey of Current Business* (Washington, D.C.: U.S. Department of Commerce, January, 1973), p. S-2; 1970-1972, *Survey of Current Business* (Washington, D.C.: U.S. Department of Commerce, December, 1973), p. S-2.

*Population.* Total population residing in United States: 1895-1940, HAUS, p. 7; 1941-1972, SAUS (1973), p. 5.

*Voter Turnout.* Ratio of total vote to total eligible vote. Eligible vote: 1896-1898, enfranchised male and female voters estimated by interpolation, SAUS (1899), p. 19, SAUS (1901), p. 19, and the *Abstract of the Twelfth Census* (1900), p. 39, 1900-1918, males over twenty-one from Bureau of the Census, *Current Population Reports* (P-25, No. 311), and enfranchised women by interpolation from SAUS (1901), p. 19, SAUS (1919), p. 42, *Abstract of the Twelfth Census* (1900), p. 39, and *Abstract of the Fourteenth Census* (1920), p. 123; 1920-1928, SAUS (1971), p. 364; 1930-1972, SAUS (1973), p. 379.

*Election Returns.* Total, Democratic and Republican votes cast in congressional elections: 1896-1928, HAUS, p. 692; 1930-1972, SAUS (1973), p. 364.

<sup>20</sup> The evidence on this presented by Bloom and Price is impressive, though they consider only the income variable. Some very preliminary analysis we have done suggests that the asymmetry they found becomes much smaller if the effect of inflation is controlled for.

# Aggregate Economic Variables and Votes for Congress: A Rejoinder\*

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AND

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Our interest in the effect of aggregate economic variables on election results began in 1970 following a conversation with an administration official that we have reported elsewhere.<sup>1</sup> We doubted both the implicit theory of voting behavior and the ability of the administration to achieve rates of inflation and unemployment even close to the ranges mentioned. We take this opportunity to note that the unemployment rate was higher and the inflation rate substantially higher than the adviser's estimate, but President Nixon was re-elected.

At the time, the principal econometric evidence of the effects of aggregate economic variables was a study by Kramer. Kramer found evidence of an effect of real income, but despite (or perhaps because of) the flaws in his procedure, he found no evidence of an effect of inflation or unemployment.<sup>2</sup> Furthermore, then and now, most of the reported evidence pertains to congressional not presidential elections and to votes for congressmen, not seats in the Congress.

We concluded our study by failing to reject a null hypothesis—that there was no evidence of an effect of real income or unemployment on votes for congressional candidates. We were less certain about the effect of inflation. Our evidence suggests some effect, and we have continued work on the problem by analyzing presidential voting and by much more detailed analysis of congressional votes and seats.

That more work remains to be done is evident from our current work and from the lengthy replies that our paper stimulated. Each pair of authors wrote a comment longer than our original article. They agree neither with us nor with each other on the proposition that evidence supports. Each raises some points that the other ignores.

The similarity ends there. Bloom and Price offer

a scholarly criticism based mainly on their original and interesting work. Their comments are based on their assessment of evidence. We discuss their work first. Goodman and Kramer, on the other hand, offer a seemingly endless number of criticisms supported by little more than prior belief, innuendo, and conjecture. Answering each of the charges would take more space and time than the criticisms are worth. We are content to support our claim by discussing a few of their charges and by presenting evidence that most of their claims are empty.

## Bloom and Price

Bloom and Price devote most of their comment to testing an alternative hypothesis of the effect of economic variables on congressional elections. They find evidence to support their hypothesis. If we had developed their evidence, we would have rejected the null hypothesis, as they do.

The hypothesis that Bloom and Price accept is different from Kramer's and, we will argue, much closer to our contention than to his. Bloom and Price show that a *decline* in real, per capita income hurts the party of the incumbent president in congressional elections. They do not show that small changes in the growth rate of real per capita income hurt the incumbent's party. Voters are not shown to be sensitive to small fluctuations in the growth rate of real income. In fact, they are relatively insensitive; a 1 per cent fall in real per capita income costs the incumbent's party from 0.6 per cent to 0.8 per cent of its vote, according to their estimates.

From 1948 to 1974, the maximum decline in real per capita income in an election year was 1.6 per cent in 1954. The largest shift of votes implied by the hypothesis is 1 per cent, so the maximum effect on the difference between the parties is about 2 per cent. The effects of inflation, unemployment, and small changes in the growth rate of output are not shown.

Per capita real output has grown at an average rate of 3 per cent. Nothing is shown about the range from zero to three per cent. It is entirely consistent with the results presented by Bloom and Price that small changes in employment and output have small effects, or no effect at all, on voting. Recessions shift votes, and major recessions

\* We remain indebted to the National Science Foundation for support of our work.

<sup>1</sup> See footnote 2 of our paper, "The Effect of Aggregate Economic Variables on Congressional Elections" elsewhere in this issue of the *Review*.

<sup>2</sup> See G. H. Kramer, "Short-Term Fluctuations in U. S. Voting Behavior, 1896-1964," this *Review*, 65 (March, 1971), 131-143. Our discussion of the flaw in Kramer's treatment of minor party votes is in footnote 16 of our paper. Once an error in the data was corrected, the effect of inflation was found to be significant.

ions shift many votes. Marginal adjustments of economic conditions before an election have not yet been shown to be important. On the contrary, Bloom and Price show little or no evidence that stimulating the economy helps the incumbent's party. The short-term effect of short-term changes in economic variables is not established by the results Bloom and Price present.<sup>3</sup>

The asymmetry of the results raises questions. Why do voters respond to negative changes of 3 per cent in the average growth rate but not to positive changes or to reductions in the growth rate to 1 per cent? One reason may be that the new voters include new entrants to the labor force and workers with low seniority. These individuals bear a disproportionate share of the private cost of unemployment and recession. If they become weak or strong partisans of the party out of power, and remain loyal, we would have an explanation of the asymmetry and the effect found by Bloom and Price. An effect of this kind would not be inconsistent with our hypothesis.

All in all, we find the reformulation and the evidence presented by Bloom and Price intriguing. We hope that either they or others will investigate the asymmetry in the response to changes in real income.

#### Goodman and Kramer

There is, for us, a considerable difference between the proposition consistent with available evidence and the conclusion reached by Goodman and Kramer. They conclude that "on the basic question of whether such effects exist, it seems to us the evidence is clear: they do."<sup>4</sup>

What are these "effects"? Do voters reward and punish? Or, do they punish only, as Bloom and Price find? Do voters respond only to recession, measured by the negative growth of real income, or to inflation and recession, as Kramer concluded? Or do they respond more to inflation, than to income as we found? Do regular voters respond or is the main effect on new voters?

Goodman and Kramer do little to advance the discussion beyond the *a priori* position from which they start. They offer almost no evidence to support the strong, and in our view, overstated conclusions they reach.

A typical example of overstatement is the discussion of the evidence they present in Table 2. The table shows estimates of the effects of real income, inflation, and two measures of unemployment in four separate regressions. Only one co-

<sup>3</sup> Bloom and Price accept our argument that voters can abstain, but their work neglects the influence of economic conditions on participation.

<sup>4</sup> Saul Goodman and Gerald H. Kramer, "Commentary on Arcelus and Meltzer," *APSR* 69 (December, 1975), p. 1255-1265.

efficient—the effect of inflation—is significantly different from zero by the usual two-tailed test at the .05 level.

These results, unlike the results of Bloom and Price, do not cause us to reconsider our main conclusion. Inflation appears to affect the outcome of congressional elections; the various measures of unemployment have not been shown to have any significant effect; the current growth rate of real income has not been shown to have a reliable effect, and the work of Bloom and Price suggests that there is an asymmetry. Large negative deviations are important; other deviations are either much less important or unimportant.

The discussion of unemployment in Goodman and Kramer is an example of their *a priori* approach.<sup>5</sup> One result shows that changes in unemployment benefit Republicans. This result is rejected as "anomalous." The level of unemployment benefits Democrats, and the result is accepted as plausible. In fact, the sign of the level of unemployment is negative for the Democrats, and the results show that the Democrats gain only because the Republicans are hurt more. The differences are not significant.

If this were the only example of a cavalier treatment of evidence, we would dismiss the example as an oversight. Similar examples reoccur in the discussion of evidence and estimation, as we show in the following sections.

**Participation.** Both pairs of critics accept our hypothesis that voters can abstain instead of shifting party preference. Bloom and Price use the percentage of the two-party vote in their work and ignore the issue. Goodman and Kramer challenge our interpretation. They assert that "participation declined rapidly from 1896 to 1912" (p. 1255). A reasonable interpretation of their Figure 1 is that participation declined from 1896 to 1902 or 1904, so that the "historical trend" of which they speak is based on two or three observations.

Goodman and Kramer claim that our equation is misspecified (p. 1257). We are, frankly, puzzled at this overstatement. Their Figure 1 seems to us to show (1) a permanent shift in the participation rate in 1920 and (2) a second permanent shift about 1932. The first is negative but larger (in absolute value) than the shift in 1932. The coefficients for these shifts, in our participation (*VP*) equation, are entirely consistent with the evidence.

Although the word "misspecification" is used repeatedly, there is no explicit statement of the misspecification. The only evidence Goodman and Kramer offer is from our regression equation, and this evidence is misinterpreted. They claim, incorrectly, that the residuals from our *VP* equa-

<sup>5</sup> *Ibid.*, p. 1264.

tion are not random. The most that can be said, correctly, is that we cannot reject the hypothesis that the residuals are not randomly distributed. If the residuals are not randomly distributed, it does not follow that the model is misspecified in the sense that the estimates are inconsistent.

In short, there is no basis for the statement (p. 1257) "the Arcelus-Meltzer estimates of the *long-term* partisan shares are incorrect." A plausible interpretation is that the serial correlation shows our inability fully to explain *short-term* fluctuations in the voting percentage by introducing aggregate economic variables into the *VP* equation. More remains to be done.

**The Shift Voters.** Goodman and Kramer introduce a long, excessively formal discussion of a simple question. Where are the shift voters? To indicate the importance of the question, they cite a previous study by V. O. Key. That study, however, discusses presidential, not congressional, elections. Our recent work suggests that shifting is much more important in presidential elections.

To bolster their position, they quote selectively and inappropriately. We have italicized the words included in our proposition and omitted from their quotation. With the omitted words included, the quotation is (our p. 1238, their p. 1258): "*the principal fluctuations in the percentage of votes received in congressional elections arise from changes in the participation rate and not from shifts between parties.*"

No lengthy, formal analysis is required to support our proposition. All that is required is computation of the change in voting percentage in presidential and nonpresidential years. The mean difference is nearly twelve percentage points, according to the estimate in our paper. This difference is a 25 per cent change in average voting participation in congressional elections between presidential and non-presidential election years. The relevance of the comparison for the proposition becomes clear once the omitted words are restored.<sup>6</sup>

**Basic Statistical Inference.** Goodman and Kramer raise what they call a "fundamental point of basic statistical inference" (p. 1259). Their point is that the "fact that a certain estimate is not significantly different from zero by no means shows that the variable has no effect. . . . The data . . . may be equally consistent with the possibility of very large effects."

This is nonsense, pure and simple. Regardless

<sup>6</sup> The rest of the paragraph from which the quotation is drawn leaves little doubt about the meaning of the proposition. The paragraph states that changes in participation have a partisan (shift) effect.

of the size of the coefficient, relatively low *t*-statistics or large standard errors imply failure to reject the hypothesis that the variable in question has no effect.

**Measurement of Economic Variables.** A number of criticisms of our work can be discussed briefly. Some are raised by both critics.

- (1) In using compensation per man hour, we ignore the unemployed. This comment is puzzling. We included measures of unemployment separately. Our procedure holds a measure of real income constant when estimating the effect of unemployment.
- (2) Real compensation is an inappropriate measure of real income. Moreover, it is "suspect" (p. 1260 of Goodman and Kramer because real compensation per man-hour rises in recession. This comment and similar comments by Bloom and Price miss the point. One of the questions that we want to answer is whether employed and unemployed workers respond in the same or in different ways to recessions. To separate the two groups we estimate the response to earnings, holding unemployment constant and the response to unemployment, holding earnings constant. Only from estimates of this kind can we hope to learn whether the voters' response to unemployment or recession extends beyond the particular voters affected by loss of employment. The comment that we should not have deflated by man-hours is correct. We miss the effect of reductions in the work week.
- (3) We take no account of the agricultural sector. This is false. We note (footnote 15) that we tried a number of other measures of economic and other issues including agricultural prices.
- (4) Many additional criticisms reveal very little more than Goodman and Kramer's prior beliefs. Several relate to the use of unemployment and the procedures for computing percentages. To find whether the criticisms are substantive, we recomputed in the following ways: (1) using Goodman and Kramer's data series and ours; (2) using levels of unemployment, changes in unemployment, and percentage changes in unemployment; and (3) using percentages computed on the base  $t-1$  and on the base  $t$ . A small sample of our results for aggregate economic variables is shown in Table 1. Others will be sent on request. Had Goodman and Kramer used some of the time lavished on their reply to compute these results, they would have found, as we did,



Table 1. The Effect of Alternative Measures of Unemployment on the Democrats' Share of the Vote  
Coefficients (*t*-statistics in parentheses)

Variable	Using Our Data			Using Goodman and Kramer Data		
<i>o</i>	-.42	-.46	-.45	-.46	-.51	-.52
<i>p</i>	(3.37)	(3.97)	(3.42)	(3.93)	(4.56)	(4.04)
<i>U</i>	.02			.02		
	(.21)			(.20)		
$\Delta U$		-.05			-.01	
		(.16)			(.02)	
<i>o</i>			.00			.00
<i>U</i>			(.15)			(.07)
<i>o</i>	.16	.08	.08	.15	.08	.10
<i>C/P</i>	(1.00)	(.47)	(.48)	(1.03)	(.51)	(.58)

All percentages are computed as  $\frac{t-(t-1)}{t-1}$

*U* = level of unemployment  
 $\Delta U$  = change in unemployment  
Other variables as defined in our paper

that their prior beliefs, conjectures about possibilities, and most of their criticisms are empty.<sup>7</sup>

Our general conclusion is that most of the Goodman and Kramer points lack substantive content. Either they are inconsequential or they concern potential, not actual, bias. If we printed all of the estimates using the various data sets, we doubt whether any reader would change any conclusion as a result of reading the many pages of output.<sup>8</sup>

<sup>7</sup>We made available to Goodman and Kramer a printout of all of our results, including computations of the covariance matrix and other intermediate results to facilitate comparisons. We are therefore surprised and puzzled at comments about our errors. If there are errors in our data or computations, Goodman and Kramer should report them instead of offering suggestive hints.

<sup>8</sup>One point on which comment is required: Goodman and Kramer note (p. 1260) that there are some substantial discrepancies between their estimates and the results shown in Table 1 of our paper. We have used both sets of data and, aside from differences attributable to computer routines, we find no substantial differences in results.

Conclusion

The effects of short-term changes in economic conditions on votes for Congress seems to us to remain unsettled. The work to date has produced mainly null results, our own included.

Discussion of this kind occasionally leads scientists to reformulate the disputed proposition. For this reason we find the efforts by Bloom and Price and their evidence interesting. The proposition for which they find support is substantially different from earlier statements of the effect of short-term changes in aggregate economic variables on congressional votes.

Our own work has followed a different course. The basic unit of interest is the distribution of seats, not votes. Investigation of the distribution of seats requires disaggregation to the district level. Preliminary results suggest that incumbency alone accounts for nearly 80 per cent of the variation in the partisan distribution of seats. That leaves very little room for aggregate economic variables, but it does not rule out a small effect. Until such effects are found and confirmed, the null hypothesis cannot be rejected.

# The Limits of Consensual Decision\*

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The idea that governments should function by some approximate consensus—unanimous agreement as a condition to action—has deep intuitive and analytic roots in liberal thought: in the myth of the social contract, in the doctrine of consent, in the structure of markets, in the utilitarian ethic which survives economic theory under the title of efficiency.<sup>1</sup> It is everywhere understood that consensus—this is the way with ideals—has serious practical limits, but these hardly disqualify it from service as a normative criterion to be approximated in experience. Yet, if I am right, we should not say merely that consensus *cannot* be duplicated in practice, we should say that it *should not* be approximated in practice. This, because consensual decision displays structural defects which, for any society requiring politics, spoil its normative promise. I will try at some length to explain this vague slander.

The paper argues three main theses. First, that consensual decision does indeed have deep roots in liberalism, of which the deepest run toward consent of the governed and toward utilitarian efficiency. This is the rather thin history<sup>2</sup> presented

\* I wish to thank John Chipman, Gordon Tullock, Duncan MacRae, Ken Shepsle, and J. Rolland Pennock for their useful advice. And I thank the Center for Advanced Study in the Behavioral Sciences, Palo Alto, for its hospitality and support during the time this work was done. The paper's length should be blamed entirely on a broken ankle which made my desk an attractive alternative to the California hills.

<sup>1</sup> I presuppose at the outset that consensual decision does not emerge as a self-enforcing norm of collective action. There are structures for which this may be expected: ones in which the defection of any denies the reward of cooperation at all. This so-called "essential coalition" problem has its historical instances: in the cartel, the minimal parliamentary coalition, the Western marriage, the isolated exchange of goods and services, the international alliance between approximate equals. The European Economic Community, especially in light of the Luxembourg Accords (1966), also seems a case of this sort. And in all these cases, consensus is self-enforcing: disagreement opens the prospect of defection, which in turn denies the rewards of joint action to others. But the nation-state deviates decisively from the "essential coalition" ideal for at least three reasons: (1) relations of authority and control permit the repression of deviance, (2) *slack* permits the toleration of at least minor deviance and defection and (3) *substitution* permits the recruitment of alternative collaborators in collective action when deviance does occur. For all these reasons, it is no historical surprise that consensual decision, like substantive equality, serves as a normative ideal raised in protest against experience.

<sup>2</sup> This account is admittedly preliminary and covers

as Part I. Second, that neither consensual decision nor any other structure can conceivably grant an unconditional right of consent to person living together in political society. Part II thus argues that some outcome to any decision must portend a violation of consent.<sup>3</sup> Third, that the new political economy—Knut Wicksell, Buchanan and Tullock<sup>4</sup>—is wrong in claiming that consensual decision leads toward social efficiency. They would be right only for a society requiring no politics. This is the burden of Part III. A final section offers one general and perhaps ironic conclusion.

## I. Historical Place of the Consensus Doctrine

At least so far as it is a theory of public life, liberalism has always been committed to a fundamental symmetry among individuals—in John Schaar's phrase, to "men of standard size and shape."<sup>5</sup> This is a kind of equality which sleeps, in practice and theory, with partners as inequalitarian as Hobbes's *Leviathan* or as divisive as the class differences which attend private capital and managerial or technical expertise. Its fundamental meaning is that relevantly similar individuals must be treated with relevant similarity, and its central implication for argument is that men may be distinguished by ability and the social utility of their work, but not by their formal value as persons. This in turn constricts normative analysis to general or symmetrical judgments: to the con-

a large sweep of arguments: Locke on consent, through Godwin, Mill, Calhoun, to the new political economy on the best structure for governmental decision.

<sup>3</sup> The argument here is addressed mainly to Calhoun's doctrine of "concurrent majorities," Wicksell's *laissez faire* consensus, and Robert Paul Wolff's "direct unanimous democracy." Cf. J. C. Calhoun, *A Disquisition on Government*, ed. Gordon Post (Indianapolis: Bobbs-Merrill, 1973); Knut Wicksell, "Ein neues Prinzip der gerechten Besteuerung," *Finanztheoretische Untersuchungen* (Jena, 1896), abridged and translated as "A New Principle of Just Taxation," by J. M. Buchanan in *Classics in the Theory of Public Finance*, ed. Richard A. Musgrave and Alan T. Peacock (New York: St. Martin's Press, 1964) pp. 72–118; and Robert Paul Wolff, *In Defense of Anarchism* (New York: Harper & Row, 1970).

<sup>4</sup> James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

<sup>5</sup> John H. Schaar, "Some Ways of Thinking About Equality," *Journal of Politics*, 26 (November, 1964) 867–95.

ent of *all* relevant subjects, to utility measured across a whole community.<sup>6</sup>

#### A. Consent in Political Regime or Their Decisions

The fundamental idea of government by *consent*, enveloped from its beginning in this equalitarian symmetry,<sup>7</sup> became important for a quite specific historical purpose: the rationalization of liberal revolution. Locke's *Second Treatise* was, in part, an attempt to discredit Stuart absolutism against a parliamentary alternative in the paradigm case of 1688.<sup>8</sup> And its central dogma is government by consent. For our purposes, two facts about Locke and his ideological task seem important.

First, like all of the great liberal constitutionalists, Locke was reacting against a structure of power—a structure which permitted some to take actions whose consequences fell upon others. This was a particular species of power—autocracy—and the doctrine of consent was shaped by it. In particular, it was *asymmetric* and *public*. By "asymmetric" I mean that it matched elite against subject public, so that authority flowed in one direction and not the other. The contrasting case, symmetrical power, was not faced by Locke's doctrine of consent. It was "public" in the sense that it had a necessary and explicit relation to the choice of policy by government: Locke did not choose to emphasize the alternative cases of anomic and civil power.<sup>9</sup> It is, at least in part, this attention to the special case of autocracy (and its parliamentary alternative) which underlies the peculiar quality of Locke's central fiction, a social contract in which "free, equal, and independent"

men agree with one another—consent—to a government by majorities:

When any number of men have so consented to make one community or government, they are thereby presently incorporated and make one body politic wherein the majority have a right to act and conclude the rest.<sup>10</sup>

This is not the least bit odd as a weapon of parliamentary revolution, and it carried a certain ideological weight in the later revolutions of 1776 and 1789. But it is an odd idea if one looks *backward* from the experience of democratic societies.<sup>11</sup> It attaches the ideal of consent to *regimes*, and it was clear enough that Stuart absolutism had its objectors at that level. But once regimes are legitimated, the question of consent turns on particular *decisions*, and there exists no self-evident link between (1) a fictional (or, for that matter, historical) consent in regimes, and (2) an actual consent in government policy. If I am governed by some approximate democracy, and find my own will trampled by its law, government by consent may seem a shallow artifice. And, save perhaps Rousseau, no major theorist escapes the inference that consent—or self-government—may be threatened by the symmetry of democratic decision just as it may be threatened by the asymmetry of autocracy. We are bound, that is, to honor the nonconsenting minority as a legitimate problem of value. This is the second point about Locke's invocation of consent as a doctrine: it arose in one quarrel, but survived to haunt parties to very different quarrels.

The line of argument which brings us to the doctrine of consensual decision is faced by this later problem, by the ideological heritage of consent and the persistent historical importance of less-than-unanimous decisions by parliamentary regimes. As early as 1798, before the democracies had got very democratic, William Godwin was able to debunk the transition from consent in regimes to consent in policies:

... if government be founded in the consent of the people, it can have no power over any individual by whom that consent is refused. If a tacit consent be not sufficient, still less can I be deemed to have consented to a measure upon which I put an express negative. This immediately follows from the observations of Rousseau. If the people, or the individuals of whom the people is constituted, cannot delegate their authority to a representative; neither can any individual delegate his authority to a majority, in an assembly of

<sup>6</sup> This universality defines a nearly constant element of political reform in Europe and the United States from the early nineteenth century forward, as rules of exclusion based on property, race, sex, and religion are constricted and overturned. "All" comes closer and closer to meaning all, even if "consent" does not come closer and closer to meaning consent. In moral philosophy, its analog is the principle of universalizability, e.g., roughly, answers to the question "what if everyone did that?" See, for example, R. M. Hare, *Freedom and Reason* (London: Oxford University Press, 1963).

<sup>7</sup> Were it not enveloped in such a symmetry, its value as an assault on monarchy and aristocracy would of course seem slight. If one asymmetry, why not another?

<sup>8</sup> It is not altogether certain that Locke saw his book in this light. Cf. John Plamenatz, *Man and Society*, Vol. I (London: Longman's, 1963), 209 ff.

<sup>9</sup> By "anomic power" I have in mind coercion which does not rely on a public code, as in Hobbes's state of nature or many contemporary cities; by "civil" or "private" power I have in mind control over others based on the use of public codes, as with the rights of ownership.

<sup>10</sup> Locke, of course, goes on to develop a very loose doctrine of consent for "later," historical acts of government, thus avoiding the issue raised by Godwin and others as discussed below.

<sup>11</sup> See in particular Willmoore Kendall, *John Locke and the Doctrine of Majority-Rule* (Urbana: University of Illinois Press, 1941).

which he is himself a member. That must surely be a singular species of consent, the external indications of which are often to be found, in an unremitting opposition in the first instance, and compulsory subjection in the second.<sup>12</sup>

Partly, no doubt, his temperament, but equally important his position as an outsider, carry Godwin toward anarchism, not consensus. As we will see (Part IIE), this is not so great a leap—from anarchism to consensus—as it may seem. The later view of Mill embodies essentially the same observation:

It was now perceived that such phrases as "self-government," and "the power of the people over themselves" do not express the true state of the case. The "people" who exercise the power are not always the same people with those over whom it is exercised; and the "self-government" spoken of is not the government of each by himself, but of each by all the rest. The will of the people, moreover, practically means the will of the most numerous or the most active *part* of the people; the majority, or those who succeed in making themselves accepted as the majority; the people, consequently, *may* desire to oppress a part of their number; and precautions are as much needed against this as against any other abuse of power.<sup>13</sup>

Mill's response is essentially libertarian: to argue that the common good compels a highly constrained use of state authority, confining prohibitions to acts that harm others,<sup>14</sup> mandates to acts whose omission would do similar harm. The resulting economy of liberty is notoriously complex. We should note, for future reference, that this argument is at least ostensibly built on an hypothesis connecting restraint from coercion with aggregate social utility:

I forego any advantage which could be derived to my argument from the idea of abstract right, as a thing independent of utility. . . .

Mankind are greater gainers by suffering each to live as seems good to themselves, than by compelling each to live as seems good to the rest.<sup>15</sup>

This claim later becomes the cornerstone on which the most sophisticated case for consensual

decision is built. Mill himself, however, comes closer to consensual decision than his advocacy. Mr. Hare's scheme of proportional representation: giving minorities voices if not vetoes on state policy.<sup>16</sup> It was for J. C. Calhoun to "redeem" these difficulties with a full doctrine of consensual decision. His recognition of symmetrical coercion is especially clear in his account of representative government:

If the whole community had the same interests so that the interests of each and every portion would be affected by the action of the government that the law which oppressed or impoverished one portion would necessarily oppress or impoverish all others—or the reverse—then the right of suffrage, of itself, would be all-sufficient to counteract the tendency of the government to oppression and abuse of its powers, and, of course, would form, of itself, a perfect constitution of government. . . . But such is not the case. On the contrary, nothing is more difficult than to equalize the action of the government in reference to the various and diversified interests of the community; and nothing more easy than to pervert its powers into instruments to aggrandize and enrich one or more interests by oppressing and impoverishing the others; and this too, under the operation of laws couched in general terms and which, on their face, appear fair and equal.

Calhoun, perhaps more than any other theorist, speaks for a defensive minority, for the Southern planter after the Tariff of 1828, during the rise of Yankee industrialism, at the ascendance of abolitionism. It is this, perhaps, which leads his excellent imagination to a doctrine of consensual decision by "concurrent majority":

. . . the government of the concurrent majority, where the organism is perfect, excludes the possibility of oppression by giving to each interest, or portion, a veto order—where there are established classes—the means of protecting itself by its negative against all measure calculated to advance the peculiar interests of other classes at its expense. Its effect, then, is to cause the different interests . . . to desist from attempting to adopt any measure calculated to promote the prosperity of one or more, by sacrificing that of others: and thus to force them to unite in such measures only as would promote the prosperity of all, as the only means to prevent the suspension of the action of government, and, thereby, to avoid anarchy, the greatest of all evils.<sup>18</sup>

Calhoun's interests are partly sinister, and his sociology is Panglossian, but his doctrine is nevertheless a prime historical leap from the concern with symmetrical conflict to the ideal of view as briefly as possible.

<sup>12</sup> Mill's further proposal for a second Chamber composed of intellectuals is meant, evidently, as a method of moral persuasion, not a center of countervailing power against parliament. Cf. *Representative Government*, ch. 13.

<sup>13</sup> Calhoun, p. 13.

<sup>14</sup> Calhoun, p. 3C.

<sup>12</sup> William Godwin, *Enquiry Concerning Political Justice* (edition of 1798), 3 Vols., F. E. L. Priestley (Toronto: University of Toronto Press, 1946), book III, chap. 2, p. 193. In his reference to Rousseau, Godwin evidently has in mind the argument that sovereignty is indivisible as it appears in chaps. 1 and 2 of Book II in the *Social Contract*.

<sup>13</sup> John Stuart Mill, *On Liberty in Utilitarianism, Liberty, and Representative Government*, Everyman edition (New York: E. P. Dutton and Co.; and London: J. M. Dent and Sons, 1951), chap. 1, p. 88.

<sup>14</sup> As will become apparent, in III below, acts of private harm, combined with static government policy, are critical to the present analysis.

<sup>15</sup> Mill, p. 97, p. 100. I have perhaps unfairly juxtaposed two sentences separated by nearly one hundred lines, but I think they give the main sense of Mill's

consensus.<sup>19</sup> American readers, reared on *Federalist* 10, may imagine that I have slighted James Madison or perhaps Alexander Hamilton here. But any careful reading of that great essay and its companions reveals a mistrust of consensual decision, born partly of experience with *immobilism* under the Articles of Confederation:

... What at first sight may seem a remedy, is, in reality, a poison. To give a minority a negative upon majority (which is always the case where more than majority is requisite to a decision) is, in its tendency, to subject the sense of the greater number to that of the lesser.<sup>20</sup>

This might seem momentary opportunism, were it not a sample from a substantial theme, and were not its sense backed up by the partial record we have of the 1787 Convention.<sup>21</sup>

It is Calhoun, then, who draws the relevant consequence of government by consent when we consider policies rather than regimes: consent in policy is assured only by a regime requiring consensus, not majoritarian democracy or its representational approximation. In Part II, this claim will be tested, and found partly incorrect. First, it is important to note that it is open to the most obvious objections as it stands.

#### B. Immediate Objections

The doctrine of consensual decision would be frail indeed if it had maintained a direct connection with the theory of consent in the nation-state. It can be shown, first, that consensus is less

<sup>19</sup> Two antecedents to this argument deserve notice. There is some evidence that the earliest European equivalents for positive law in our sense of that term were made by something like unanimity among local elites. Cf. Max Weber, *On Law in Economy and Society*, transl., Edward Shils and Max Rheinstein, ed. Max Rheinstein (Cambridge, Massachusetts: Harvard University Press, 1954), especially, p. 84. This perhaps corresponds to the "essential coalition" problem, footnote 1 above. A second antecedent is in the doctrine of separation of powers, in its less pragmatic formulations. Cf. Moses Mather, *America's Appeal to an Impartial World* (Hartford, 1775) especially p. 8, where he writes of estates "... armed with a power of self-defense against the encroachments of the other two, by being enabled to put a negative upon any or all of their resolves. ..."

Cited in Bernard Bailyn, *The Ideological Origins of the American Revolution* (Cambridge: Harvard University Press, 1967) p. 57, 73. The explicit veto makes the parallel to Montesquieu or *Federalist* 51 less clear.

<sup>20</sup> Alexander Hamilton, *Federalist* 22. All quotations from the *Federalist* are as in the edition by Benjamin Fletcher Wright (Cambridge, Massachusetts: Harvard University Press, 1961).

<sup>21</sup> See *Federalists* 45 and 58 by Madison, in Wright, and consult Max Farrand, *The Records of the Federal Convention*, 4 vols. (New Haven: Yale University Press, 1937). Particularly revealing are the internal organization of the convention (majority rule by states) and the reaction against the Articles.

than optimal if one begins from the ideas of consent and formal equality. For example, we may interpret "consent" as a matching problem between individual preferences and policy outcomes: the optimum, among simple voting schemes, is majority rule if we make one set of assumptions, and will be consensual decision only if we make the evidently strong assumption that changes of policy opposed by individuals are *a priori* very much more costly than continuances of policy opposed by individuals. Similarly, it can be very simply shown that majority decision minimizes the maximum number of voters who can possibly be dissatisfied with such outcomes.<sup>22</sup> And, from the view of equality, it is evident that consensual decision favors negative minorities over positive majorities—"weights" them unevenly: "... unless government policy responds to the preferences of the greater number, the preferences of some individuals (the lesser number) must be weighted more heavily than the preferences of some other individuals (the greater number). But to weight preferences in this way is to reject the goal of political equality."<sup>23</sup>

Moreover, the doctrine is easy prey to the charge that defensive minorities may use their special leverage—the potential cost of their votes to those who want change—as a means to exploit others. Indeed, there seem to be incentives for persons indifferent or even modestly favorable to change to falsify their preferences as a means of extortion. This is the sense of Ben Franklin's opposition to executive veto as he recounts his experience with the colonial Governor of Pennsylvania for the Convention of 1787:

The negative of the Governor was constantly made use of to extort money. No good law whatever could be passed without a private bargain with him. An increase of his salary, or some donation, was always made a condition; till at last it became the regular practice, to have orders in his favor on the Treasury, presented along with the bills to be signed, so that he might actually receive the former before he should sign the latter.<sup>24</sup>

This no doubt betrays the liberty of an aging wit, and has the special structural feature of monop-

<sup>22</sup> Douglas W. Rae, "Decision Rules and Individual Values in Constitutional Choice," and "Political Democracy as a Property of Political Institutions," both, *American Political Science Review*, 63 (March, 1969), 40–56 and 65 (March, 1971), 111–19.

<sup>23</sup> Robert A. Dahl and Charles E. Lindblom, *Politics, Economics and Welfare* (New York: Harper & Row, 1953) p. 44. A more formal argument with similar implications is K. O. May, "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision," *Econometrica*, 20 (October, 1952), 680–684. See also, Neal Reimer, "The Case of Bare Majority Rule," *Ethics*, 62 (October, 1951), 16–32.

<sup>24</sup> As reported by Madison's notes, in Farrand, p. 99.

oly: only one actor could singlehandedly threaten veto. But, it is easy to show that the same may happen where veto rights are symmetrical, and, perhaps worse, that bluffing may lead to the rejection of policies which would in fact have benefited everyone.<sup>25</sup>

There is also the intuitive historical sense that consensual decision may saddle societies with policies designed to meet circumstances which no longer exist, making it all but impossible to cope with present exigencies:

When the concurrence of a large number is required by the Constitution to the doing of any national act, we are apt to rest satisfied that all is safe, because nothing improper will be likely *to be done*; but we forget how much good may be prevented, and how much ill may be produced, by the power of hindering the doing what may be necessary from being done, and of keeping affairs in the same unfavorable posture in which they may happen to stand at particular periods.<sup>26</sup>

This criticism is in the end decisive, but in much altered form. This is because other developments lead toward a far subtler account of consensual decision and its relation to the public good.

#### IC. Utilitarian Efficiency and the Market Analogy

These developments, emerging during about the same historical era, center on a doctrine, utilitarianism, and an institution, the market. From them rises a theory of social value—utilitarian efficiency—which unites the practice of consensus, the doctrine of consent, and a withered residue of utilitarian philosophy.

Benthamite utilitarianism seems to have codified a kind of enlightened common sense for a civilization whose institutions had only just begun to accommodate the notion of formal equality: if each is to count for one, class and standing aside, what could be more sensible than a grand addition of welfare in social decision? Yet the doctrine has always displayed singular vulnerabilities. One of these is the leap from an ostensibly descriptive idea of greatest happiness to a prescriptive judgment that it ought to be pursued, even where the happiness fails to accrue to the person who must

decide. The question, quite simply, "Why?" not answerable within the unamended utilitarian doctrine.<sup>27</sup> For our argument, however, the relevance of the greatest-good idea is precisely that some important analysts *have* made the transition. If one studies a scheme of partial conflict, where the goals of groups and individuals cross one another, and one simultaneously claim a measure of neutrality, then a plausible and in personal criterion is required. One can then attach judgments to it—"If you want that, do this . . ."—and produce a prudential science which avoids at least the most obvious forms of special pleading. This was the position of 19th century political economy, and the parts of which remained liberal seem at least tacitly to have taken this line. The greatest happiness idea thus becomes an underlying axiom of analysis. Its apogee is no doubt to be found in the quite incredible pages of F. Y. Edgeworth's *Mathematical Psychics*.<sup>28</sup> Although the doctrine has grown less fashionable with time, its at least tacit acceptance seems essential to an understanding of what followed.

Three major difficulties are at once encountered if one proposes to apply a utilitarian ethic: (1) for a given individual, it seems impossible to "measure" utilities cardinally;<sup>29</sup> (2) if such measurements are possible, it seems impossible to aggregate them among individuals—at least not with arithmetic precision; and (3) it has never been clear how to conceive the underlying "stuff" of utility apart from the subjective preferences of individuals. These difficulties, taken together, have three important implications. First, they seem to undermine direct utilitarian engineering; for example, the "leveling" of real income to maximize aggregate utility as suggested by a line of analysts stretching from Mill to Abba Lerner in our own time.<sup>31</sup> Second, they offer a line of re-

<sup>27</sup> Cf. John Plamenatz, *Man and Society*, 2 vols. (New York: McGraw-Hill, 1963), vol. II, chap. 1, pp. 1-36. A related difficulty, worth noting, is the blindness of the utilitarian doctrine to fair distribution. We will touch on this later, but a particularly useful attempt to offer an alternative doctrine, sensitive to fairness, is John Rawls, *A Theory of Justice* (Cambridge: Harvard University Press, 1971).

<sup>28</sup> It would perhaps be more accurate to call it a "tacit assumption" than an axiom, since the present economic theory of utility is reducible in principle to an entirely formal schema.

<sup>29</sup> *Mathematical Psychics* (London: Kegan Paul, 1881). Despite the elegance of his analysis, Edgeworth's contribution is flawed by Victorian prejudice: he manages to use the utilitarian calculus to justify nearly all the major inequalities of his time. See especially pp. 74, 77-82.

<sup>30</sup> Apart from the Von Neumann-Morgenstern system on the assumption that risk does not affect utility.

<sup>31</sup> *The Economics of Control* (New York: Mac-

<sup>25</sup> Brian Barry, *Political Argument* (London: Routledge and Kegan Paul, 1965), pp. 245-250.

<sup>26</sup> Hamilton, *Federalist* 22. Hamilton's example demonstrates an almost clairvoyant intuition for what is now the recent past: "Suppose, for instance, we were engaged in a war, in conjunction with one foreign nation, against another. Suppose the necessity of our situation demanded peace, and the interest or ambition of our ally lead him to seek the prosecution of the war, with views that might justify us in making separate terms. In such a state of things, this ally of ours would evidently find it much easier, by his bribes and intrigues, to tie up the hands of government from making peace. . . ."

instance against arguments predicated on ethics of distribution—equality, fairness, justice—especially as these are used in attacks upon market institutions. Finally, they define the analytic challenge met by the normative *principle of efficiency* as we know it today.

This notion of efficiency arises indirectly from the astonishingly recent discovery, dating apparently to the work of Adam Smith, that exchange *itself* is a source of utility. If Peter gives *X* to Paul and receives *Y* from him in return, both acting voluntarily,<sup>32</sup> does it not follow that both have gained utility? Is not *Y* worth more than *X* to Peter and *X* worth more than *Y* to Paul? How else to explain the constituents of market behavior? Both analytically and normatively, this has become a central pillar in the theory of the market. Moreover, its modest methodological requirements ingratiate themselves nicely with the caveats against utilitarianism noted a moment ago.

If we accept those caveats, we are entitled to precisely two inferences about individual utility: (1) if a person successfully chooses to alter his position, he has gained utility, and (2) if he is unwillingly compelled to change his position, he has lost utility. We can, of course, drop "utility" and reduce this to a discussion of indifference curves, but the important point is this: we can neither quantify gains and losses for an individual, nor compare those of two individuals. It follows, and this is critical, that *any event in which some persons gain and some lose is indeterminate with respect to aggregate welfare*. We cannot, on this view, decide whether losses outweigh gains or vice versa: such inferences are impossible within the analytic scheme. Two things follow immediately. First, if (say by majority voting)  $n-1$  people gain and one loses, we cannot claim to have done the group a good turn.<sup>33</sup> Second, what is somewhat improperly called the "Pareto principle"<sup>34</sup> takes center stage: we will call it the "efficiency rule."

millan, 1944). See also Kenneth Arrow, "A Utilitarian Approach to the Concept of Equality in Public Expenditure," *Quarterly Journal of Economics*, 85 (August, 1971), 409-15.

<sup>32</sup> And as rational egoists who know what's good for them.

<sup>33</sup> This of course raises the "intensity problem" from democratic theory. Cf. Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), especially chap. 4; Willmoore Kendall and George M. Carey, "The 'Intensity' Problem and Democratic Theory," *American Political Science Review*, 62 (March, 1968), 5-24; Douglas Rae and Michael Taylor, "Some Ambiguities in the Concept of Intensity," *Polity*, 1 (Spring, 1969), 298-308. Notice the devastating effect of the present position on any utilitarian argument for majority rule.

<sup>34</sup> "Improperly" because it appears that Pareto did not intend the use presently given to his criteria. Cf.

Let us define the efficiency rule as we will use it later.<sup>35</sup> Suppose we have a "policy set," consisting of possible outcomes to a particular social decision. In the market case, these would be bundles of goods associated to owners. One of these outcomes may be distinguished as the status quo. We have a community of people whose welfare is relevant to the selection of an outcome from the policy set. Each of these people entertains preferences about outcomes which are at least sufficient to produce a three-cell division of this set: (1) a *preference set* of outcomes which he would substitute for the status quo, (2) a *rejection set* of outcomes against which he would retain the status quo, and (3) an *indifference set* for which neither (1) nor (2) holds. Each person has such a division for any given status quo point, and it is ostensibly based on a weak ordering of all alternatives. The efficiency rule has two relevant provisions:

- (1) If an outcome belongs to at least one preference set and no rejection set, its substitution for the status quo is efficient.
- (2) If no outcome meets requirement (1), the status quo is efficient (or "Pareto optimal").

The efficiency rule is subject to at least four misunderstandings. First, a transaction, considered as a whole, may meet (1), although its component elements do not: this is true for simple market exchange, as well as more complex cases. Second, provision (2) usually defines a very large, sometimes infinite, set of outcomes, all of them efficient. If this is so, many, perhaps most, social choices are "undecidable" from the standpoint of efficiency. What should we do about undecidable cases? This question is central to our analysis of the consensus argument later. Third, the efficiency criterion is blind to both distributive and perfectionist morals. As Amartya Sen has it,

An economy can be optimal in this sense even when some people are rolling in luxury and others are near starvation as long as the starvers cannot be made better off without cutting into the pleasures of the rich. If preventing the burning of Rome would have made Emperor Nero feel worse off, then letting him burn Rome would have been Pareto-optimal. In short, a society or an economy can be Pareto-optimal and still be perfectly disgusting.<sup>36</sup>

Last, the efficiency rule is extremely "brittle" in its sensitivity to the status quo: Once he ignites the curtains, Nero must be bribed to desist; before he

Talcott Parsons, *The Structure of Social Action*, Vol. 1 (New York: The Free Press, 1937, 1968), 241-249, and, less importantly, Vincent J. Tarascio, *Pareto's Methodological Approach to Economics* (Chapel Hill: University of North Carolina Press, 1968).

<sup>35</sup> Cf. III below.

<sup>36</sup> *Collective Choice and Social Welfare* (San Francisco: Holden-Day, 1970), p. 22.

does so, the Romans must be bribed to have their city burned. In the case of economic exchange, this makes property rights pivotal, and where they are ambiguous—as with external diseconomies not contemplated by existing civil law<sup>37</sup>—the criterion itself is ambiguous. We will find this difficulty in exacerbated form when we come to political decision.

These difficulties, important as they are, fail to deny the efficiency rule its enormous influence. Its immediate effect is to make the connection between consent and utility (vouchsafed by efficiency) a tautology. How do we know a change is efficient? By the consent of the parties, by their willingness to consent in the substitution. How do we know a status quo is efficient? By someone's unwillingness to substitute any alternative for it. Apart from the special case of "hypothetical compensation,"<sup>38</sup> this analytic seam is airtight. And one need not be the least bit cynical to see that it has enormous ideological value to people well placed in society, for no redistribution can be justified without the consent of those who stand to lose, even if they are numerically overwhelmed by prospective beneficiaries.

We are in the midst of a chain relationship. If we retain a tacit loyalty to Benthamite utilitarianism, then the utility caveats draw us toward the efficiency rule. This in turn renders the utility-consent hypothesis we earlier attributed to Mill an analytic inevitability. Finally, this gives market exchange a normatively privileged position. My consent is my willingness to deal, and if you and I are rational egoists, the market's rules lead us to act out the doctrine "automatically," by trading efficiently until an efficient outcome is reached. It is the voluntary quality of market exchange that makes this possible, and—for the doctrine of consensual decision—this is a momentous fact. For *we should now imagine the market as a normative ideal, against which ordinary political decisions look "sub-optimal" at best.*

<sup>37</sup> For a brilliant analysis of such cases see R. H. Coase, "The Problem of Social Cost," *Journal of Law and Economics*, 3 (October, 1960), 1-44.

<sup>38</sup> Hypothetical compensation undoes the connection because it lets me carry out a change so long as I gain enough from it that I *could have* compensated you for your losses. The idea, implicit in Wicksell (see below), is formalized by N. Kaldor, "Welfare Propositions of Economics and Interpersonal Comparisons of Utility," *Economic Journal*, 49 (September, 1939), 549, which is excellently discussed in I. D. Little, *A Critique of Welfare Economics* (London: Oxford University Press, 1950), p. 88 ff; and E. J. Mishan, *Welfare Economics* (Amsterdam: North Holland, 1969), p. 38-51; and Sen, pp. 30-32. A further difficulty, evidently proved by Skitovski, is that the criterion gives intransitive results. Its importance, for us, is that it brings the practical implications of "efficiency" very nearly full circle to the Benthamite position.

The critical inference is a simple one: the normative virtue of markets lies in the voluntariness of exchange, and this voluntariness is guaranteed by the consensual structure of market exchange. Leaving aside external diseconomies and the occasional prospect of blackmail, we can represent the choice-structure of market transactions thus,

Coalitions that can impose exchanges:  
((seller & buyer))

Coalitions that can veto exchanges:  
((seller) or (buyer))

Which is, formally at least, homologous with

Coalitions that can change public policy  
((A & B & C . . . & N))

Coalitions that can veto changes of public policy  
((A) or (B) . . . or (N))

The market analogy" is that, for both market exchange and consensual decision, anyone can prevent action, so that no party can be damaged by it.<sup>39</sup> This provides the background for Knut Wicksell's claim (in his account of taxation) that

provided the expenditure in question holds out any prospect at all of creating utility exceeding costs, it will always be theoretically possible, and approximately so in practice, to find a distribution of costs such that all parties regard the expenditure as beneficial and may therefore approve it unanimously. Should this prove altogether impossible, I would consider such failure as an *a posteriori*, and the sole possible, proof that the state activity under consideration would not provide the community with utility corresponding to the necessary sacrifice and should hence be rejected on rational grounds.<sup>40</sup>

We will later examine Wicksell's argument more closely, but here it is necessary merely to see that this is a very strong new case for consensual decision, and that it completes the complex development we have been sketching. Very nearly the same claim is offered by James Buchanan and Gordon Tullock, as they write,

. . . we admit as "better" only those changes that are observed to be approved unanimously by all members of the group. Any change that secures unanimous support is clearly "desirable," and we can say that such a change is "in the public interest." Few would, we suspect, dispute this half of our criterion for evaluating social changes. However, we go further and state that

<sup>39</sup> Ironically, the idea of the social contract seems to have an at least intuitive relation to this analogy in pre-Lockean thought. This is suggested by the multiple contract theory of Althusius' *Politica Methodice Diogesta* (1603), as analyzed by Kendall, *John Locke*, pp. 46-9. The explicit analogy reappears in Buchanan and Tullock, p. 250 ff.

<sup>40</sup> Wicksell, pp. 89-90.



or any change in the public interest, unanimous support can be achieved.<sup>41</sup>

It is evident that these authors entertain a utilitarian conception of the public interest. And they seem to have combined a constrained version of his philosophy with the consent of the governed.

#### D. Summary

This completes an admittedly fragmentary historical sketch, which leaves us, I think, with three important facts. First, that the ideology of parliamentary revolution—most of all a symmetrical doctrine of government by consent—left liberal ideology with an enormous leap to be made: from fictive consent in regimes to actual consent in their policies. Second, that a plausible response to this leap is to be found in a doctrine of consensual decision. Finally, that the additional doctrine of efficiency and the “market analogy” offer an apparent synthesis of these two—consent and utilitarianism.

My purpose from this point is to offer an analytic appraisal for these conjectures. I begin with the contention that consensual decision would, at least in theory, guarantee the consent of the governed.

### II. Consensus and the Consent of the Governed

The most fundamental claim for consensual decision is also the simplest: that consensus implies consent. That is, if an outcome is adopted under a system of consensual decision, we are entitled to assume that everyone has given his consent to it. This is *not* a mere tautology, and it is not always true. Let me begin by looking briefly at two arguments which have this consent-consensus hypothesis as their cornerstones.

#### ■(IA. The Consensual Systems Proposed by Calhoun and Wolff

Calhoun is central to the consensus doctrine. His scheme for concurrent majority decision is an effort to approximate consensual decision in the face of large numbers and the resulting escalation of decision-making costs.<sup>42</sup> Summarized very simply, Calhoun's plan is this:<sup>43</sup>

- (1) The community is to be divided into a set of mutually exclusive classes.
- (2) These classes are to be homogeneous with respect to the effects of government policy.
- (3) For any proposal of government policy, each such class is to make a separate decision: approve or disapprove.

<sup>41</sup> Buchanan and Tullock, p. 285.

<sup>42</sup> A concept owing to Buchanan and Tullock, cf. III below.

<sup>43</sup> Calhoun, *Disquisition*, pp. 19–23, 27–31, 35–54.

- (4) Given homogeneity (2), and some rationality assumption, the method of class decision (3) is not very important: majority rule will do.

- (5) If a government policy is to be enforced, every class must agree.

This presupposes a cleavage structure without cross-cutting: a “pillar society,” such as the Dutch are said to have. And, more demanding still, this structure must remain essentially constant through time, else repeated constitutional crisis. Calhoun is obviously thinking about the southern planters as a class, and is tactically willing to permit other classes to fall as they may. If the required assumptions be granted, I believe the concurrent majorities must be admitted as a valid approximation to consensual decision. The question however remains: Does this guarantee consent? Let me return to that momentarily.

A more recent analysis is Robert Paul Wolff's *In Defense of Anarchism*.<sup>44</sup> This author writes from the American moral left during the Vietnam era; he is concerned with the moral autonomy of citizens as it is threatened by government authority. And the overall structure of his argument is straightforward: (1) each citizen should do as he thinks best, (2) the state necessarily entails authority, which requires that citizens do as someone else thinks best; (3) these are incompatible, so that (4) there ought to be no states. This argument is compelling only on the unlikely supposition that the abolition of states would be attended by no other form of coercion. Buried within this larger argument, however, is the proposition that states would be acceptable if only it were practicable to organize them consensually. This position may be summed up as follows:<sup>45</sup>

- (1) Citizens should do as they think best.
- (2) This is possible if they must obey only laws and policies to which they have each given their consent.
- (3) If government authority were exercised only by unanimous direct democracy, (2) would obtain and (1) would therefore be possible.

Wolff imagines that this scheme would in principle resolve his normative dilemma:

<sup>44</sup> *In Defense of Anarchism* (New York: Harper and Row, 1970).

<sup>45</sup> Wolff, pp. 22–27. Notice the similarity to Rousseau's search for “... a form of association which may defend and protect with the whole force of the community the person and property of each associate, and by means of which each, coalescing with all, may nevertheless obey only himself, and remain free as before.” (Emphasis added.) *Social Contract*, book I, chap. VI. Rousseau does propose consensus as the basis of a fictive social contract, but urges that “the citizen consents to all the laws, even to those which are passed in spite of him. . . .” (book IV, chap. II). The sense of this consent has always escaped me.

The solution is a direct democracy—that is, a political community in which every person votes on every issue—governed by a rule of unanimity. Under unanimous direct democracy, every member of society wills freely every law which is actually passed. Hence, he is only confronted as a citizen with laws to which he has consented. Since a man who is constrained only by the dictates of his own will is autonomous, it follows that under the directions of unanimous direct democracy, men can harmonize the duty of autonomy with the commands of authority.<sup>46</sup>

Now Wolff must be aware that each citizen is born into a society with a history; that this history will have deposited a residue of laws; that each of these laws cannot be conditionally revoked at the birth of each fresh infant, pending his yes vote during the morning of his eighteenth birthday. Yet this difficulty alone would seem sufficient to shatter his hypothesis that consensus implies autonomy (which is, of course, a species of consent).<sup>47</sup>

### II.B. A Revised Market Analogy

To avoid this difficulty, one would have to change the "market analogy" (IC above) so that it looked like this:

- Coalitions that can impose exchanges:  
(seller & buyer))
- Coalitions that can veto exchanges:  
(seller & buyer))

and analogously,

- Coalitions that can impose new policies:  
((A & B, . . . & N))
- Coalitions that can retain old policies:  
((A & B, . . . & N))

Thus, each would hold a right of veto over every policy, including old laws in politics and the non-exchange outcome in the market. But there is something very wrong here, for these structures open the prospect that *every* outcome will be blocked by some coalition. In the market case, this would suggest that you and I might neither exchange nor fail to exchange certain bundles of goods, in politics that we might neither retain nor rid ourselves of a law. Both prospects are in some fundamental way preposterous, and important foundations are missing from the analysis. These

are supplied by a simple and very partial account of public choices (IIC), and a criterion of robustness for the structures by which they are resolved (IID).

### IIC. Forced Choice

The full policy of a government at any moment will be some historical combination of outcome to specific issues. Suppose we treat issues,  $X_1, X_2, \dots, X_n$ , as sets of mutually exclusive government policies. Each such issue, or "policy set"  $X_i$  thus contains a collection of policy outcome which exclude one another,  $X_i = (x_{1i}, x_{2i}, \dots, x_{mi})$ . On any such issue, at any given time, there must be some status quo, whether explicit or tacit. So we can represent a full public policy as an  $n$ -tuple of alternatives for  $x_1 \dots x_n$ , and mark their standing as status quo outcomes with a bar

$$\bar{x}_{i1}, \bar{x}_{i2}, \bar{x}_{i3}, \dots, \bar{x}_{in}.^{48}$$

This account carries the implication that each issue involves a *forced choice* among alternative outcomes. Forced in two senses (1) not more than one outcome can be the status quo, and (2) some outcome must be the status quo. This applies to issues, and by an obvious extension to the  $n$ -tuples of issues which define public policies.

Here are some classes of issues which illustrate the forcing of choice:

- (1) Even if government chooses to permit market decision makers to establish it, a society must have some given social product over any interval of time, and must have some set of rules for its allocation.
- (2) Consider any given public good<sup>49</sup> which any given government might produce: it must always be producing it at some level, even if that level be zero.
- (3) Consider the public law as a compound of triples in the form:  
[(subject class) (norm-operator) (action class)]  
and think of three norm-operators (must, may, must-not).<sup>50</sup> Then, for any pair of subject and

<sup>48</sup> E.g., the  $i$ th outcome to the first issue, the  $j$ th to the second, etc.

<sup>49</sup> E.g., a good for which exclusion is impossible or expensive, so that its distribution follows the pattern "if any then all," or at least, "if some then others." Mancur Olson's *Logic of Collective Action* (Cambridge: Harvard University Press, 1965) offers a classic analysis of the incentive problems raised by public goods.

<sup>50</sup> This three-operator analysis is controversial, but our point would hold for the four- or  $n$ -operator case as well. Cf. Georg Henrik von Wright, *Norm and Action* (London: Routledge & Kegan Paul, 1963) or Alf Ross, *Directives and Norms* (New York: Humanities Press, 1968). I myself believe von Wright is correct in claiming that these three operators are sufficient to express the content of any law or other rule, given the auxiliary privilege of negation.

<sup>46</sup> Wolff, p. 23.

<sup>47</sup> Godwin's response to the myth of the social contract seems appropriate to the point: How am I obliged? "Surely not upon the contract into which my father entered before I was born?" *Enquiry*, book III, chap. 2, p. 189. In his assault on the Pareto principle, I. D. Little makes essentially the same observation in *A Critique*, p. 94.

action classes, there must be *one and only one* effective norm-operator. For example, let the subject class be "persons living in California" and the action class be "deliberately kill other people": the law must either prohibit or command or permit this pattern of behavior for those people. It is shallow at best to imagine that such legal choices can be put off: to "not decide" must imply one of two outcomes:

(a) tacit permission, *nullum crimen sine lege*, or

(b) continuance of an explicit status quo mandate, prohibition or permission.

To see the point's obviousness, try to devise a way of effectively postponing a decision on the legality of abortion which can be explained to a pregnant woman.

- (4) In the choice of candidates for office, either someone is elected or nobody is (yet) elected at any given moment: a final choice can be put off only by accepting an interval of indeterminacy or "provisional" control which is a very real outcome (consider the Greek colonels or the Portuguese Communists).
- (5) International agreements can be put off, but a very real and often painful status quo remains to be experienced.

These cases suggest a ubiquitous forcing of choice:<sup>51</sup> even when explicit outcomes can be postponed, tacit ones are *ipso facto* chosen. In the

<sup>51</sup> "... government inaction is as much a choice of policy as government action," Dahl and Lindblom, *Politics, Economics and Welfare*, p. 338. See also IV below. Two contrary observations need mention. First, it has been suggested that this choice from mutually exclusive alternatives rules out compromise and creates a zero-sum constraint on outcomes. This is simply incorrect: the compromise outcome *is* an alternative, and *does* exclude other outcomes. Second, there is no reason whatever to imagine a zero-sum constraint for mutually exclusive alternatives. Consider, for example, an Edgeworth box for two traders. Its points are in the end mutually exclusive, but choices among them are certainly not zero-sum. These erroneous views are expounded by Buchanan and Tullock, *The Calculus of Consent*, p. 253 ff. Second, some decisions suggest the possibility of multiple outcomes. Say we are awarding medals to national heroes: awarding one may deflate the value of awarding another, but surely does not exclude it. This is correct, but rather superficial. The effective alternatives are lists of national heroes. With  $n$  candidates, there would be  $2^n$  possible lists. But any one list does rule out any other list. A more important example: public works projects as authorized by the American Congress. At the margin, when each new project is considered, it seems irrelevant that others have been approved already, so each may be treated as a separate issue. But the choices are nonetheless forced in two ways: (1) each must be approved in some form or rejected, and (2) one grand list of projects must emerge from each Congress. The current proposal for centralizing the appropriations process in Congress would make this latter point clear even as each project was considered.

schemes proposed by Calhoun and Wolff, this suggests that *some* outcomes and not others are subject to the requirement of unanimity. If *all* outcomes were subject to unanimity, then we would risk the position in which we both refused to change policy and refused to keep it the same. This brings us to a somewhat more exact account of policy decisions.

Considering any single issue, we are confronted with a status quo, and some alternatives to it. Assuming  $m$  alternatives in  $X_1$ , and repressing the subscript which identifies the issue, we have a list of possible status quo points as the left column of the matrix below. We also have a list of alternatives to each of these, as shown in the top row of the matrix. Notice that the status quo is included as "an alternative to itself," in order to display the rejection of change as a choice:

		to these alternatives			
		$X_1$	$x_1$	$x_2 \dots x_m$	
from these possible status quo policies:	$x_1$	$D_{11}$	$D_{12}$	$D_{1m}$	
	$x_2$	$D_{21}$	$D_{22}$	$D_{2m}$	
	$\dots$				
	$x_m$	$D_{m1}$	$D_{m2}$	$D_{mm}$	

Society's problem is to pick a *transition* from the alternatives thus offered. Decision  $D_{11}$ , for example, means retaining outcome  $x_1$ . By contrast,  $D_{12}$  represents the substitution of outcome  $x_2$  for  $x_1$ . We will operate always in a single row of the matrix, as defined by the particular status quo we confront. We can therefore confine our attention to a single arbitrary row with status quo  $x_1$ :

moving from $\bar{x}_i$ to		$x_j \dots x_i \dots x_m$	
is decision			
		$D_{ij}$	$D_{ii} \dots D_{im}$

We can define consent, consensus, and the criterion of robustness over the choice so defined.

### IID. Robustness, Consent, and Why Consensus must be Conditional

A *control-structure* defines the coalitions which can impose decisions. An arbitrary decision  $D_{ij}$  can be imposed by one or more minimally sufficient coalitions, and the set of such coalitions is labelled  $C_{ij}$ . Thus, for example, we might let  $x_i$  and  $x_j$  represent arbitrary imputations of goods

to two traders,  $A$  and  $B$ . Then the normal rules of market exchange would display the structure,

	$x_i$	$x_j$
$x_i$	$C_{ii} = ((A) \text{ or } (B))$	$C_{ij} = ((A \& B))$

(1)

so that the nonexchange decision  $D_{ii}$  can be imposed by either; exchange decision  $D_{ij}$  only by both.

A given actor enjoys a *right of consent* in decision  $D_{ij}$  only if he belongs to every minimally sufficient coalition in  $C_{ij}$ . Both  $A$  and  $B$  enjoy such a right for the exchange outcomes; neither enjoys such a right for the nonexchange outcome. Similarly, a control structure is *consensual for a specific decision* if and only if every relevant actor enjoys a right of consent in that outcome. The market is consensual for exchange but not for nonexchange outcomes.

Earlier observations (IIC) suggest that society must always be prepared to choose exactly one outcome. It is therefore inadmissible to have a structure which could deny us *any* decision, any location for the status quo. Such a structure could be workable in exactly the same way that sugar could be "insoluble"—until, that is, it was wet. In our case, that means until there is a conflict in which coalitions at once block all decisions. I have in mind a concept of *robustness*. Here is the peculiar market structure mentioned in IIB.

	$x_i$	$x_j$
$x_i$	$C_{ii} = ((A \& B))$	$C_{ij} = ((A \& B))$

(2)

This scheme has two properties. First, it is unconditionally consensual: each person enjoys a right of consent in each outcome. Second, it is non-robust. Intuitively: what if  $A$  blocks the non-exchange outcome and  $B$  blocks the exchange outcome(s)? Precisely: a structure is robust if it meets the following requirement:

- (1) Consider a coalition (\*) which is insufficient to impose any decision (impotent)
- (2) For every coalition (\*) there must exist at least one other coalition (\*\*) with two properties:
  - (a) it is disjoint with (\*)—no members in common
  - (b) it is potent—can impose some outcome

Ordinary market exchange (1) meets this requirement. Neither  $(A)$  nor  $(B)$  nor  $(AB)$  is impotent. The empty set  $(\emptyset)$  is impotent like (\*). But all three of the other coalitions just mentioned satisfy

(ii) by being disjoint with  $(\emptyset)$  and potent. Now consider the special market exchange system defined by (2). The coalition  $(A)$  is impotent like (\*). The coalitions  $(B)$  and  $(\emptyset)$  are disjoint with  $(A)$ , but both are impotent. The coalition  $(AB)$  is non-impotent, but is not disjoint with  $(A)$ . Thus this odd scheme is nonrobust. It permits failure to adopt any outcome.

More generally, consider a structure which is unconditionally consensual and therefore gives a universal right of consent to all decisions,

	$x_i$	$x_j$	$\dots$	$x_m$
$\bar{x}_i$	$C_{ii} = ((\forall))$	$C_{ij} = ((\forall))$		$C_{im} = ((\forall))$

(3)

where  $(\forall)$  represents the coalition of all relevant actors. No other structure is unconditionally consensual and gives an unconditional right of consent. But this structure is nonrobust. Any proper subset of  $(\forall)$  is impotent like (\*). Some other subset of  $(\forall)$  may be disjoint with this impotent coalition, but will itself be impotent. Only  $(\forall)$  is nonimpotent, but it can hardly be disjoint with its own subset. Therefore, this structure and all its concrete analogs are nonrobust.

The choice of structure is then between *robustness and unconditional consensus with its correspondingly perfect right of consent*. We do not choose robustness; necessity chooses it for us. We therefore can choose at most a conditional form of consensus and correspondingly conditional right of consent. If there are  $m$  decisions available, as many as  $m-1$  may be consensual, but *at least one must not be consensual*.

### III. Anarchic Consensus: Calhoun and Wolff

Calhoun seems to have known this. Notice that he attributes a motive for agreement to his actors: they must come to unanimous agreement on positive policy "... as the only means to prevent the suspension of the action of the government, and, thereby, to avoid anarchy, the greatest of all evils."<sup>52</sup> The structure seems to be this:

	$x_i$	$x_j$	$x_0$
$\bar{x}_i$	$C_{ii} = ((\forall))$	$C_{ij} = ((\forall))$	$C_{i0} = ((\exists))$

To sustain the status quo requires unanimity;<sup>53</sup> to

<sup>52</sup> Calhoun, *Disquisition*, p. 30, as cited above, IA.

<sup>53</sup> This critical point of interpretation is perhaps counterintuitive, but fits well with the text of Calhoun's essay and is consistent with his tactical posi-

substitute another positive policy requires unanimity; but to block either course of action and thereby suspend all positive policy ( $D_{10}$ ) requires only a single class (let  $\exists$  be any single class). In other words, each class can impose anarchy, which functions as the nonconsensual outcome and preserves the robustness of the concurrent majorities as a structure. If we classify conditional (hence, robust) schemes of consensus by their "escape" decisions, this might be termed *anarchic consensus*. It is, so far as I can tell, precisely the theme which answers to Godwin's objection against government: insofar as Calhoun's structure imposes any status quo save anarchy, it does so by a functional equivalent of unanimous consent. Calhoun's system—or the more general structure of anarchic consensus—abandons consent in regimes and imposes direct consent in policy.

However fanciful this may seem, it forces the question: should we *choose* the prospect of being bound by government without our own consent? In other words, should we reject universal consent to policy as a normative criterion? Calhoun avoids this deeper issue. From his vantage, remember, the force of government was used to bind one large class—the blacks—without their consent or even a gesture in that direction. Calhoun clearly *would not* have liked to part with this leverage by universalizing his concurrent majorities to include the blacks as a class. Calhoun clearly *would* have liked to gain new leverage by extending the concurrent majorities to the class of southern planters, in their conflict with other sectors of the society, notably the industrial North. Since the blacks do not qualify as citizens, he is able to finesse the obvious contradiction.

But if one is bound by symmetry of argument, when the fundamental dilemma is unavoidable. Let "Ego" and "Alter" be any two persons or classes. Then symmetry implies that if Ego would bind Alter by governmental decision, against his will, he too must face the prospect of being bound unwillingly by Alter. Calhoun's proposal, read symmetrically, is a mutual agreement to rule out such binding. It is clear that both must surrender any right of consent in the ungoverned action of the other. If Alter chooses to aggress against Ego, directly or indirectly, Ego cannot invoke a gov-

ernmental constraint against him. *The effect of Calhoun's proposal is to repeat the social contract position at the level of policy.* Each governmental decision is like a fresh social contract, and each such contract is subject to unilateral revocation at any time.<sup>54</sup> We purchase unconditional consent in government by surrendering any further right of consent in the behavior of others.

Robert Paul Wolff's *In Defense of Anarchism* carries this facet of Calhoun's theory to its conclusion. We should seek a society in which each does as he thinks best, thus enjoys autonomy by consent in all decisions. This could be accomplished if all decisions were taken by direct unanimous democracy. But we could achieve this unanimous democracy only if we *already* had "... substantial agreement among all members of a community on all matters of major importance."<sup>55</sup> Wolff thus points to a problem (autonomy or consent); proposes a solution (consensual decision); and cautions that it will work only if we haven't got the problem in the first place (e.g., no real disagreements exist). It is as if we had contrived a medicine useful only to the well.

It is worth noticing that this exercise should lead, as it does in Wolff, to a doctrine of philosophical anarchism only if a stateless society promises a better approximation to a condition of universal autonomy. This might mean that we asked everyone to internalize the interests of others under a universal principle of just action so that no part of society would ever choose to act in a way unacceptable to others. And, as Wolff admits by way of footnote, unanimous agreement is a useful mental experiment in choosing the principle which would be so internalized.<sup>56</sup> But this is not to be confused with the problem of political structure—with the rules under which public decisions are in fact to occur. Here, Wolff's analysis offers very little. For suppose that either (1) a principle of just action has become the guide to action by all or (2) that it hasn't. In the first case, anarchism will work but so will everything else; in the second case anarchism won't work—won't function as an approximation to universal autonomy.

It can indeed be shown that *no* structure of decision unanimity—majority-rule, combat, dice-throwing, oracular contemplations, or the reading of coffee grounds—can of itself unhinge the fundamental problem of *political vulnerability*:

tion. Calhoun, it will be recalled, had taken a "nullificationist" position against federal tariff policy on behalf of South Carolina in 1828. And, during the 1840s when the present theory was concocted, his region found itself at an increasing disadvantage in federal policy. Only a doctrine which at once offered a defense against further damage and a recourse against existing policy would suit his need. I believe he found this in the "anarchic consensus" theory as interpreted here.

<sup>54</sup> I draw this inference in full knowledge that Calhoun rejected the social contract as an historical doctrine, in *Disquisition*, pp. 3-4. His analytic motive for this seems obvious.

<sup>55</sup> Wolff, p. 24.

<sup>56</sup> See, for a main example, John Rawls, *A Theory of Justice* (Cambridge: Harvard University Press, 1971).

unanimous universal consent has the logical form of the square circle.

### IIF. Stasis Consensus: Buchanan and Tullock

An especially important consensual structure, argued by Buchanan and Tullock, has this shape,

	$x_i$	$x_j$
$\bar{x}_i$	$C_{ii} = ((\exists))$	$C_{ij} = ((\forall))$

The *stasis consensus* has the status quo as its non-consensual outcome, offering a universal right of consent only in change. This is the version of consensus which fits the "market analogy," and the version which goes to the doctrine of utilitarian efficiency: if a new outcome is adopted, must not this change benefit society? It evidently cannot have damaged society if nobody objects and blocks its adoption. But the issue is more subtle, and I postpone it for Part III below.

### IIIG. Laissez Faire Consensus: Wicksell

Yet another structure is Knut Wicksell's *laissez faire* consensus.<sup>57</sup> Here, the policy which removes least from the private sector can be imposed unilaterally by anyone, or at least by a small minority. Unless it also happens to be the *laissez faire* solution, the status quo enjoys no special privilege, and all spending is open to what might be called the "Earl Landgrebe syndrome"<sup>58</sup> in which all activity is vetoed. Wicksell thus asks us to accept the rules of a market society—property, contract, rights of disposition—nonconsensually, and to offer a right of consent in any deviation from them.

### IIIH. Implications of Consensus Analysis

The right of consent cannot be unconditionally vouchsafed by any workable structure. For any issue, there must be at least one outcome against which no universal right of consent is available. If we follow Calhoun's anarchic consensus, this is the "no-policy" solution, which leaves people free

to act as they can and will without government intervention. We would never be freighted with positive policies against our will, but the price measured in the events which might occur without governmental intervention. Calhoun, in effect, asks us to surrender the right to coerce others through government in return for freedom from coercion by them through government. Whether this is an appropriate idealization depends on our estimate for the consequences of anarchy. Should be noted in closing that only the anarchic consensual system resolves the problem of consensus in policy left open by Locke's initial argument. Any alternative structure must leave some positive policy which may be imposed nonconsensually, be it the status quo created by an earlier generation, the bare market solution, or the law whose first word stands highest in alphabetical order.

### III. Consensus and Utilitarian Efficiency

Bearing in mind, then, that all viable structures offer an at most conditional right of consent, we may at last test the central dogma of the new political economy: that consensual decision leads to utilitarian efficiency. For this purpose, we put aside the objections already raised (Part I), and begin with a spatial interpretation for the rule of efficiency.

#### IIIA. The Efficiency Rule

Consider an  $N$ -space, each of whose dimensions corresponds to the utility of a single individual, and each of whose points is defined by an  $n$ -tuple of individual utilities. The dimensions are "elastic," and distance is meaningless: only direction is determinate. This corresponds to the caveat against cardinality and interpersonal comparison of utility. Each dimension is an individual weak ordering, and each point is defined by the intersection of  $n$  such orderings. In Figure 1, we have picked one two-dimensional slice from this space and use it to depict the intersection between the orderings of two arbitrary individuals  $A$  and  $B$ . These allow us to define efficiency and its alternatives spatially.

The weak ordering attributed to  $A$  defines three sets with respect to a status quo at  $\bar{x}_i$ : (1) a preference set north of  $A-A'$ , (2) an indifference set on  $A-A'$ , and (3) a rejection set south of  $A-A'$ . Line  $B-B'$  defines analogous sets for  $B$ . From  $\bar{x}_i$ , according to our earlier definitions, we can identify the set of *efficiency dominant changes* as those north, east, or northeast of  $\bar{x}_i$  (the set defined inclusively by line  $B'-\bar{x}_i-A'$ ). These bestow gains without losses. *Counterefficient changes*, if we may be permitted such a barbarism, are defined inclusively by the southwesterly set bounded at  $A-\bar{x}_i-B$ . These bestow losses without gains.

<sup>57</sup> Wicksell. Since Wicksell's system is not symmetrical with respect to the status quo, its structure must be represented in two matrix rows, as follows:

	$\bar{x}_i$	cost-minimal $x_j$	other $x_j$
cost-minimal $\bar{x}_i$	(0)	(?)	(&)
other $\bar{x}_i$	(&)	(0)	(?)

Unanimity is thus required to increase or sustain spending. The system is undefined for cases in which spending is not a relevant variable.

<sup>58</sup> Landgrebe is Congressman from the Indiana 2nd District and claims to have voted against every non-military appropriation in the 92nd Congress.

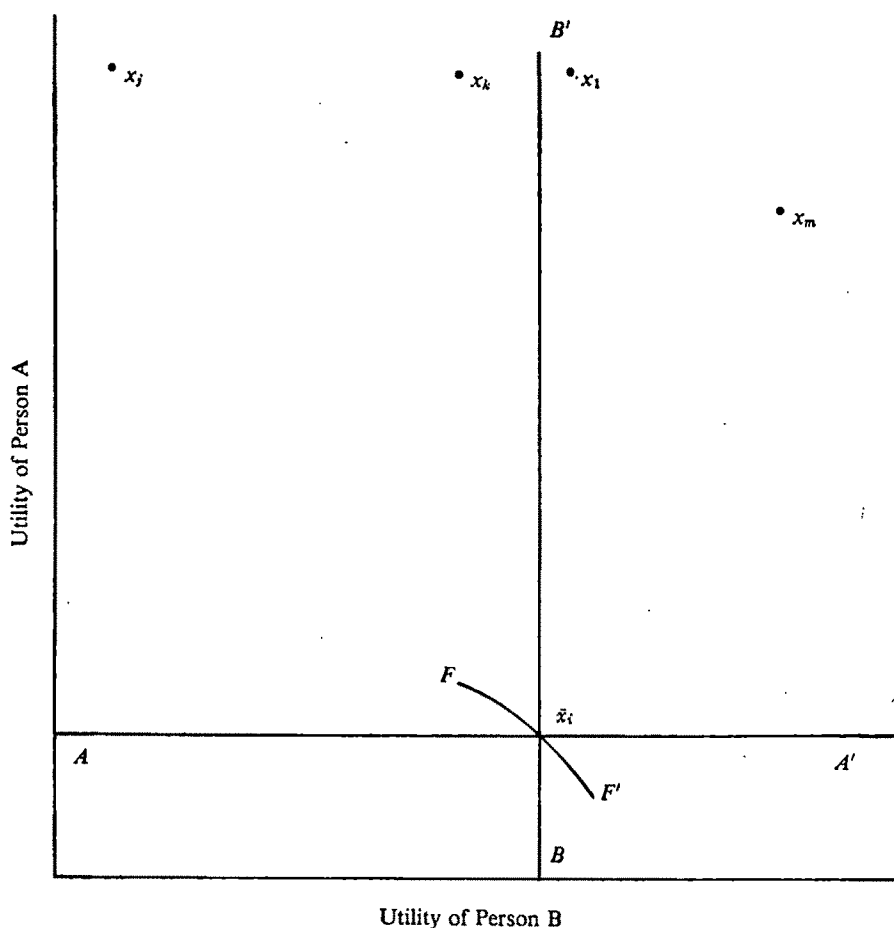


Figure 1. The Efficiency Rule

*Undecidable changes* are located in the sets exclusively defined by  $A - \bar{x}_i - B'$  and  $B - \bar{x}_i - A'$ —northwest and southeast of  $x_i$ . Notice that such undecidable substitutions may be very tempting: as with  $x_k$ , which seems to offer a great gain to  $A$  at small cost to  $B$ . In Calhoun's terms, this seems a profitable sacrifice of one interest to another. But within the present frames of reference, we need not argue against it from consent, since *we are not entitled to the inference itself*.<sup>59</sup> That is, we cannot distinguish  $x_k$  from a point like  $x_j$ , cannot know that we are making a "social dividend" by this change. We can, of course, construct a *new* alternative, like  $x_1$ , perhaps by side-payment from  $A$  to  $B$  or log-rolling—either simultaneously

with a "rider" or by promise of future votes. But this is not the same as moving to  $x_k$  (no more the same than selling me a car is the same as giving it to me.)

Suppose, for the moment, that line  $F-F'$  defines a possibility frontier passing through  $\bar{x}_i$ . This means there can be no efficient substitutions for  $\bar{x}_i$  (the sector bounded by  $B' - \bar{x}_i - A'$  being entirely out of reach). In this case, the rule tells us that  $\bar{x}_i$  is an *efficiency maximum*. We may reformulate the efficiency rule in strong terms: *Make substitutions if and only if they are efficiency dominant*. This is precisely what a well-functioning market does if each person behaves as a rational egoist. If certain other conditions are also met, the market moves toward an efficient outcome: an equilibrium from which no further efficient progress is possible. We wish to analyze the claim that consensual political decision will function similarly. The detailed implications would seem to be as follows:

- (1) Only efficient changes occur.

<sup>59</sup> A limitation which rules out a very great deal: any compromise with consensual decision (agreement by  $n - 1$  members, say) is, in this light, qualitatively indistinguishable from majority-rule or a personal dictatorship. In policy terms, it also entitles me to retain my twenty-seventh fur coat against the feeding of your sick child unless I choose to part with the coat.

- (2) All efficient changes occur.  
and (3) Efficiency equilibria will be attained.

### IIIB. Stasis Consensus and Efficient Decision

We want to see whether consensual decision will produce these results. We consider a stasis consensual system which I believe embodies the most attractive features of the account given by Buchanan and Tullock.<sup>60</sup> This is sufficiently determined by the following model:

- (1) Each person whose utility may be affected by a decision either has a vote or is faithfully represented by an agent who does.<sup>61</sup>
- (2) Each such person's vote is determined as follows:
  - (a) If a substitute outcome is in his preference set, a yes vote,
  - (b) if a substitute outcome is in his rejection set, a no vote,
  - (c) if a substitute policy is in his indifference set an abstention.
- (3) A substitute for the status quo is imposed if and only if there is at least one yes vote and no no votes.

This scheme is a sophisticated cousin of the consent theories with which we began. Does it fulfill the requirements we have adduced for the efficiency principle? This would seem to be the minimum implication of the claim made by Buchanan and Tullock as they set unanimity as a necessary and sufficient condition to change.<sup>62</sup>

Implication (a) is confirmed for this model: only efficient changes occur. Suppose a policy change is undecidable or counterefficient. Someone loses from the substitution, and the policy belongs to one or more rejection sets. A no vote follows, and the necessary condition for change is violated. Thus, only efficient changes occur. With side-payments, this may mean the adoption of complex packages, efficient in aggregate but composed of inefficient elements (e.g., an inefficient policy outcome and an inefficient side-payment to make an efficient package). This is consistent with our hypothesis, which we confirm for this model.

Now implication (b): that all efficient changes occur. Assume, first, a fixed and fully visible set of efficient alternatives to the status quo. Each of these will be adopted against the status quo. Thus in Figure 1, either  $D_{i1}$  or  $D_{im}$  would substitute  $x_1$  or  $x_m$  for  $x_i$  under the stasis consensual system. Notice, however, that once either was adopted,

<sup>60</sup> See Buchanan and Tullock, *The Calculus of Consent*, especially pp. 63–96, 249–262; and section II G above.

<sup>61</sup> Wicksell imagines this as achieved by parliamentary decision with proportional representation (pp. 96–97), while Buchanan and Tullock focus mainly on direct decision. The word *each* is very important, for the logic of the efficiency argument collapses even under minor deviations.

<sup>62</sup> Buchanan and Tullock, p. 285; above, p. 1276–1277.

the other would *become* an efficiency-undecidable proposal for change, and would be rejected under our model. In other words, we should say, *any* rather than all efficient changes will be adopted. The significance of this is both distributive and tactical. *A* would rather move to  $x_1$  and *B* to  $x_m$ . Each may well be tempted to express insincere preferences so that *both* outcomes are rejected. *A* may try to impose  $x_1$  by alleging that  $x_m$  belongs to his rejection set for  $x_i$ , and *B* may take the obverse strategy. It is, then, evident that *neither* outcome may be adopted: decisions  $D_{i1}$  and  $D_{im}$ , both efficient, may be rejected in favor of the status quo choices  $D_{ii}$ . This suggests that the model contains within itself incentives for departure from the voting rule (2). This is a serious practical limitation, despite the formal result implied if the model is faithfully acted out. This point of brittleness is already well understood.<sup>63</sup>

A second difficulty: suppose we do not imagine a fixed set of visible alternatives, but assume that only a few of the many possible outcomes are searched out and proposed. This would seem realistic. It follows immediately that an efficient substitution (say  $D_{im}$ ) may be omitted. It should, nevertheless, be noted that the model contains strong incentives for search: somebody inevitably stands to gain from any efficient substitution. A plausible supposition would be that each participant will search up to some personal threshold of "satisficing,"<sup>64</sup> so that this omission would be less serious than one might imagine. Yet, it is also plausible to imagine large inequalities of information and technical competence which would lead to search biased toward large gains for some minorities and not others.<sup>65</sup> Partial and possibly biased search, then, adds a second caveat to our confirmation of implication (b).

### IIIC. Utilitarian Maxima, Equality, and the Limits of Efficiency

Suppose, next, that we go along way beyond implications (a) and (b), and begin to engage in loose talk about the maximization of aggregate

<sup>63</sup> Barry, pp. 245–250. Also Hamilton, *Federalist* 22, and Franklin as cited above, and William Baumol, *Welfare Economics and the Theory of the State* (Cambridge: Harvard University Press, 2nd ed., 1965), p. 42ff.

<sup>64</sup> The notion is Herbert Simon's. See, for example, "Theories of Decision-Making in Economics and Behavioral Science," *American Economic Review*, 49 (June, 1959), 253–283.

<sup>65</sup> Notice that no *sinister* motive need be attributed to an elite under this account; a merely self-centered search for alternatives will suffice. It is to Calhoun's credit that he sees the possibility of extractive outcomes without recourse to evil motive: *Disquisition*, especially pp. 3–7. The classic formulation is Robert Michels, *Political Parties*, trans. Eden and Cedar Paul (1915; rpt., New York: Dover Books, 1959), especially pp. 377–392.



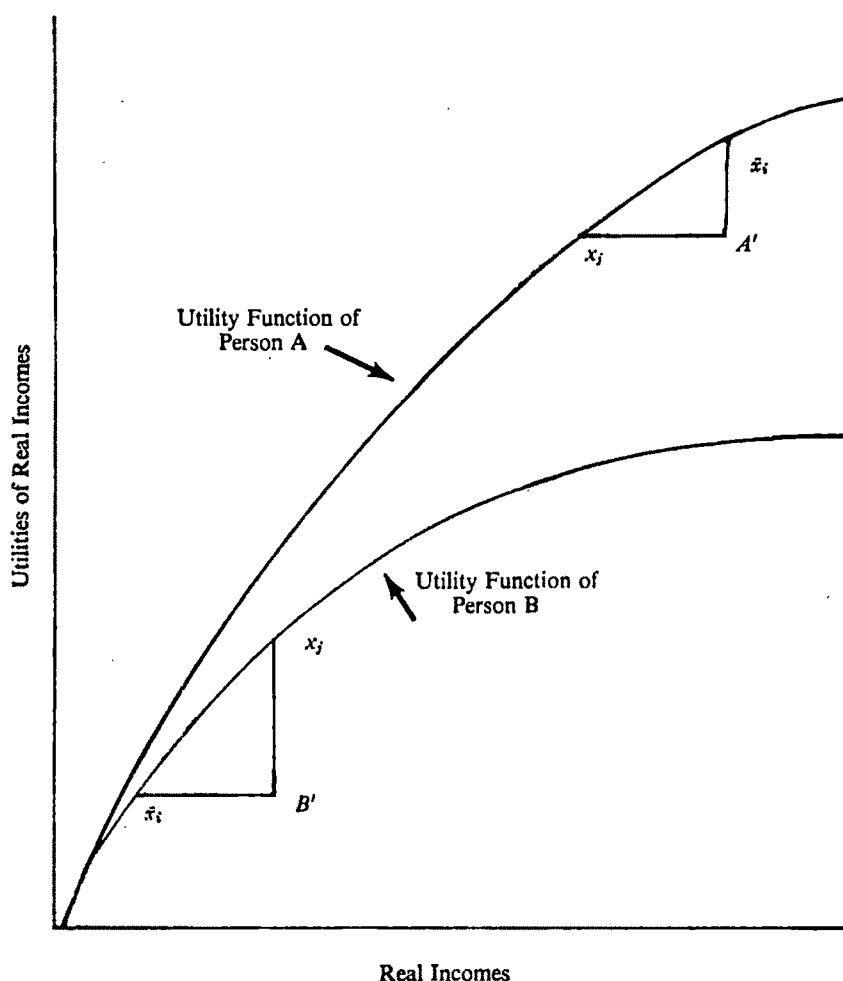


Figure 2. An Inefficient Increase of Aggregate Utility by Redistributive Decision  $D_{ij}$

utility. We might allege that our confirmation of (a) and (b) supports Wicksell's conjecture that "[failure to reach unanimity provides] an *a posteriori*, and the sole possible, proof that the state activity under consideration would not provide the community with utility corresponding to the necessary sacrifice. . . ."<sup>66</sup> Consider a redistributive program, whose incidence for two arbitrary individuals is to reduce the difference between their real incomes. Person A loses a given increment of real income ( $A' - x_j$  in Figure 2) and Person B gains the same amount ( $B' - \bar{x}_i$  in Figure 2). Clearly, this package is efficiency-undecidable<sup>67</sup> and will be rejected consensually if

<sup>66</sup> Wicksell, as quoted above, p. 1276. Wicksell has a different consensus structure in mind, but this happens to coincide exactly with the stasis structure under consideration for the present case: both the status quo and the *laissez faire* solutions would leave real incomes undisturbed.

<sup>67</sup> In the spatial interpretation given as Figure 2, it is analogous to decision  $D_{ik}$  or  $D_{ij}$ .

(a) holds: Person A will withhold his consent. Does this entitle us to claim that the redistribution would therefore have failed to increase aggregate utility? Not at all. Following the common supposition that real income yields a decreasing marginal utility, an opponent could claim that this downward redistribution would increase aggregate utility.<sup>68</sup> Suppose he conjectures utility functions for real income shown as Figure 2. Since B's utility gain ( $x_j - B'$ ) exceeds A's loss ( $\bar{x}_i - A'$ ), his case is good: this has nothing to do with the labeling of the two functions, and his argument would actually be stronger if the two curves were reversed. His argument does violate our utility caveats, but so does any refutation we may offer.

This digression has four consequences for the larger analysis. First, efficiency and consensual

<sup>68</sup> Lerner and Arrow give far subtler arguments. The simple case made here appears in Christopher Jencks et al., *Inequality* (New York: Basic Books, 1972), p. 5ff.

decision (so far as it is efficient), are blind to distributive equality. Blind twice: (1) because equality as ordinarily conceived requires interpersonal comparisons, and (2) because consensus gives the privileged a right of consent in reform. If then, the object is to reduce inequalities, we must either reject consensual choice or hope fervently for the good will of the rich. It is, in short, clear that equalitarians, with or without utilitarian roots, have good reason to reject efficient consensus as an ideal. Conversely, Wicksell has suggested this as an advertisement for his scheme of consensus to the rich.<sup>69</sup> Second, if our underlying normative position is utilitarian, it follows that consensual decision may exact serious *opportunity costs* precisely because it respects implication (1) of the efficiency rule, rejecting efficiency-undecidable decisions (like  $D_{ij}$  in Figure 2). This in turn modifies the normative sense of implication (2): that we adopt any *efficiency-improvement* does not imply that we adopt any *utility-improvement*. We are sheltered from this conjecture by the fact the utilitarian case for redistribution requires interpersonal comparisons, which permits us to deny that any valid evidence could even in principle be displayed for the hypothesis that leveling increases aggregate utility. By the same stroke, of course, it denies us any claim to the falsification of that hypothesis.

Both of these difficulties are overcome by a political economy of abundance. If the total supply of real income expands inexorably, then we need not face the harsh trade-off between "haves" and "have nots." This optimism has deep roots in American thought, and is elegantly caricatured by David Potter in its contrast to the European alternative,

... Europe has always conceived of redistribution of wealth as necessitating the expropriation of some and the corresponding aggrandizement of others; but America has conceived of it primarily in terms of giving to some without taking from others ... without necessarily treating one class as the victim or even, in an ultimate sense, the antagonist of another.<sup>70</sup>

This permits us to look past these objections to consensus. First, we can "level up"<sup>71</sup> by giving dividends of growth to the poor while allowing the rich to maintain their position. Second, we need not take the opportunity costs of consensual decision very seriously: the future presents a steady supply of fresh (and painless) opportunities. This optimism is expressed in the market

analogy itself: Since traders trade, they are surely doing themselves a good turn; since citizens accept change (if they do), they too are doing themselves a good turn.<sup>72</sup>

But this line of escape seems to me increasingly implausible. There is every reason to suspect that the future does not promise boundless economic growth, at least not at a price we ought to be willing to pay.<sup>73</sup> If (by grace) the future does offer such a prospect, another difficulty must be faced. We have so far tacitly supposed independence in the utility of real income. Thus, the case goes, if *A* gets more while *B* stands pat, gross utility must have increased. But there is some evidence that the utility of real income is defined *invidiously*: If you gain in real income, the utility of my real income is diminished by invidious comparison. This ancient supposition is supported by intuition: Why else are "poor" Americans, whose incomes excel more than 90 per cent of the world's population, poor? Partly, to be sure, because the structure of everyday life presses high demands for consumption in a society like our own. But partly also because they are poor in relation to the rest of us. It is also supported by some contemporary research: deprivation is relative, and the utility of income varies not with its absolute amount but with its placement in specific social distributions.<sup>74</sup> If this is so, then "leveling up" is chimerical: if it is possible to level up real incomes, it may still be impossible to level up welfare.

A third implication returns us to the doctrine of consensus itself. If governmental decisions were taken without side-payments, consensual decision would clearly be vulnerable to Hamilton's objection, would make adaptive change all but impossible. The open use of bribes is the punch behind the market analogy, and only Victorian prissiness would suggest that it is an inherently bad idea.<sup>75</sup> The present digression on equality and redistribution, however, calls to mind a distressing prospect. Suppose that, under the status quo,

<sup>72</sup> Cf. Buchanan and Tullock, pp. 249-262.

<sup>73</sup> The literature of pessimistic economics grows daily. I have in mind, for example, the Committee of Rome's argument as expressed in Danella H. Meadows et al., *The Limits to Growth* (New York: Universe Books, 1972). A very concise and pointed analysis of material limits on growth is Earl Cook, "Energy for Millenium Three," *Technology Review*, 75 (December, 1972), 16-23.

<sup>74</sup> See, for example, W. G. Runciman, *Relative Deprivation and Social Justice* (Berkeley: University of California Press, 1966) and Richard A. Easterlin, "Does Money Buy Happiness?", *The Public Interest*, 30 (Winter, 1973), 3-10.

<sup>75</sup> The institutionalization of side-payments is discussed in James S. Coleman, "Political Money," *American Political Science Review*, 64 (December, 1970), 1074-87, and Buchanan and Tullock, pp. 265-81.

<sup>69</sup> Wicksell, p. 95-96. Given a *laissez faire* status quo, his argument would apply to the stasis consensual scheme as well.

<sup>70</sup> *People of Plenty* (Chicago: University of Chicago Press, 1954), p. 113.

<sup>71</sup> The phrase is Potter's, *People of Plenty*, p. 121.

persons are well stocked with side-payments valued by others, and some other persons are utterly without such side-payments. Let  $A$  represent a "fat cat" and  $B$  represent a "pauper." Suppose then that a policy change advantaging  $A$  arises: he can make it an efficiency improvement by giving  $B$  a sufficient bribe, and it will be adopted. But the reverse case is distressing: a measure advantaging  $B$  arises, but he cannot lay hands on a sufficient bribe for  $A$ . If side-payments have diminishing marginal utility, this effect will be magnified, since  $A$  will set less store on a bribe of fixed size than  $B$  will. Neither as utilitarians nor as equalitarians are we apt to rest happily with the fact that  $A$ 's proposal goes through while  $B$ 's does not. Insofar as we escape Hamilton's objection, then, the case for consensual decision must presuppose a relatively equalitarian and fluid distribution of transferable, valuable goods.

Finally, this case once again calls into question our commitment to the idea of unanimous consent in government policy. The consent assumption, built into the argument from efficiency, here rises and falls with that doctrine. We have suggested three reasons—utilitarian opportunity costs, distributive fairness, and the distribution of side-payments—to hold the doctrine of efficiency at arm's length. Here, as before, we are given cause to deny that the right of consent in change should always be honored in governmental decision.<sup>76</sup>

### IIID. Consensual Decision and Progress Toward Efficiency Maxima

We have yet to examine implication (3)—the achievement of efficiency equilibria—for our consensual model. This line of analysis leads, if I am not mistaken, to a serious objection against the consensus doctrine even in this sophisticated form. Let us proceed on the supposition that implications (1) and (2) are essentially correct: consensual decision makes only efficient substitutions

of policy, and makes such substitutions whenever they are visible.

If this were so, we might expect to find "histories" of the kind suggested by Figure 3. Possibility frontier  $F-F'$  defines an efficiency equilibrium set: once such a point is achieved, no efficient changes are possible. Policies  $x_1, x_2, \dots, x_5$  define a hierarchy of efficiency, each an improvement over its predecessors. It follows that decisions  $D_{12}, D_{23}, D_{45}$  represent a "history" of progress toward equilibrium. Each narrows the remaining set of efficiency improvements, defined by the nested sectors radiating from each policy point (the dotted lines in Figure 3). The status quo plods toward equilibrium, as these sets of remaining improvements diminish with each decision.<sup>77</sup> Now, if consensual decision displays this sort of sequence, we are entitled to imagine a steady progress and enjoy a strong objection against "risky" deviations from consensual efficiency. Moreover, we have given real substance to the earlier doctrine of consent: each citizen is free from any loss of welfare. But is this what we ought to expect?

Consider a scenario which seems to confirm the optimistic history just outlined.<sup>78</sup> I have in mind

<sup>77</sup> Notice that this history is exactly the "market case" in which successive gains from trade drive partners toward a contract locus from which further gains are impossible. This is the very heart of the "market analogy."

<sup>78</sup> Here is the account given by Buchanan and Tullock, p. 91:

... if decisionmaking costs are neglected, this test [consensus] must be met if collective action is to be judged "desirable" by any rational individual calculus at the constitutional level. We may illustrate this point by the classical example of Pigouvian welfare economics, the case of the smoking chimney. Smoke from an industrial plant fouls the air and imposes external costs on residents in the surrounding areas. If this represents a genuine externality, either voluntary arrangements will emerge to eliminate it or collective action with unanimous support can be implemented. If the externality is real, some collectively imposed scheme through which the damaged property owners are taxed and the firm's owners are subsidized for capital losses incurred in putting in a smoke-abatement machine can command the assent of all parties. If no such compensation scheme is possible (organizing costs neglected), the externality is only apparent and not real. The same conclusion applies to the possibility of voluntary arrangements being worked out. Suppose that the owners of the residential property claim some smoke damage, however slight. If this claim is real, the opportunity will always be open for them to combine forces and buy out the firm in order to introduce smoke-abatement devices. If the costs of organizing such action are left out of account, such an arrangement would surely be made. All externalities of this sort would be eliminated through either voluntary organized action or unanimously supported collective action, with full compensation paid to

<sup>76</sup> It should be clear that these shortcomings are not unique to consensual decision. It is obvious that implication (1) does not hold for other structures: All other structures permit changes opposed by some persons and thus brook the possibility that efficiency-undecidable choices will result. Implication (2) is also violated by other structures: efficiency-dominant outcomes may be passed up. Biased search is perhaps universal; many structures, such as majority voting, invite strategic manipulation. (See, for instance, Robin Farquharson, *A Theory of Voting* (New Haven: Yale University Press, 1969). Finally, other structures fail to enforce equalitarian outcomes even where these may arguably serve to maximize total welfare. See William Riker, "Bargaining in a Three-Person Game," *American Political Science Review*, 61 (September, 1967), 642-56. For an introduction to the related "games of fair division" problem, see Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1967) pp. 363-68.

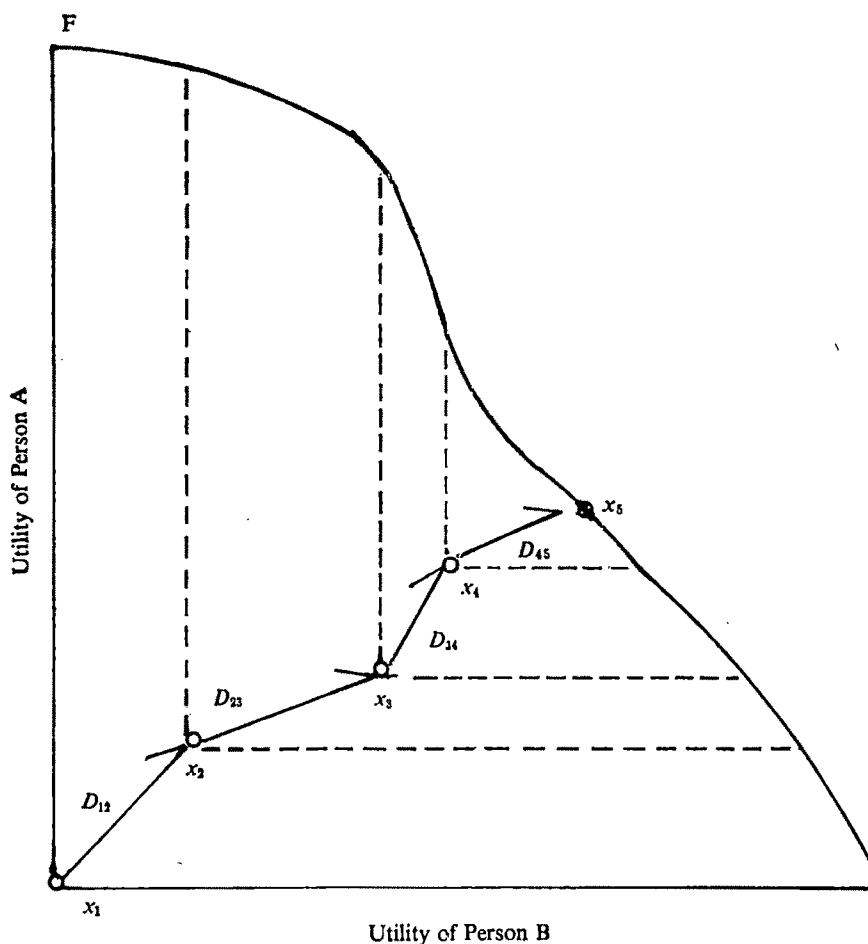


Figure 3. A Sequence of Efficiency Improvements Approaching Equilibrium

the famous Pigouvian chimney:

- (1) A chimney emits smoke, reducing the welfare of people living nearby.
- (2) The victims of this diseconomy would gain from its elimination, but the chimney owners would lose from its elimination.
- (3) It is proposed that government eliminate the smoke by simple prohibition. This is rejected (implication 1).
- (4) It is proposed that government eliminate the smoke by the combination of a prohibition and a tax on former victims sufficient to make this a gain for the chimney owners. This is accepted (implication 2).

This is a happy story. We reject an efficiency-undecidable reform at step (3), and simultaneously honor the chimney owner's right of consent in the

parties damaged by the changes introduced by the removal of the externalities." This is an unconventional use of "externality." But, as will be seen momentarily, it is also incorrect analytically.

alteration of government policy. Notice that we do *not* honor the victims' right of consent in the retention of status quo policy, but put this aside (momentarily) as inconsequential. Better yet, we are able to achieve an efficiency improvement by combining two (efficiency undecidable) changes in a single package of reform which is accepted by everyone. In step (4), that is, we have made an efficiency improvement. And we have done so without violating anyone's right of consent. This move would be analogous to a decision in Figure 4, say  $D_{12}$ , where  $A$  is a smoke victim and  $B$  is a chimney owner. But what if we had rejected the reform package at (4)? This would suggest that the package was not an efficiency improvement, perhaps because the tax-bribe was insufficient compensation to the chimney owners. Again, all is well and good for the optimistic history: we have here one of its constituent elements.

Notice that this telling might founder on any of several difficulties noted already. First, the reform

might never be proposed for the lack of a sufficient search, especially if the victims of the smoke lacked the confidence or skill to seek it out and communicate the result to the owners. Second, it might be rejected not for its inefficiency, but out of strategic misrepresentation. The chimney owners, for example, might calculate thus: (1) the tax-bribe offered is acceptable to the smoke victims, so (2) they must therefore enjoy a margin of improved welfare from its adoption; but (3) why not obtain some of this margin for ourselves by demanding a higher level of compensation? The result might be the rejection of an efficiency improvement. Third, the story offers no hint of fairness. Fourth, we have no assurance that a rejected alternative might not constitute an efficiency-undecidable gain of aggregate utility. This would be an especially tempting hunch if there were far more victims than chimney owners. Finally, it might happen that the victims simply did not have the assets required to offer sufficient compensation. For all these reasons, we have no right to propose the story as an historical probability or to take unambiguous satisfaction in its occurrence.

### IIIF. Drift and the Detachment of Incremental from Aggregate Efficiency

But all of these difficulties are less serious than the ones which arise from the effort to extend the analysis *backward* in time.<sup>79</sup> The telling we have so far given *begins* with the existence of an external diseconomy. But suppose we supply step (O) as follows:

- O. Government policy permits industrial siting in the neighborhood, and then the owners site their chimney so that the smoke begins to fall on the neighborhood.

This suggests two relevant inferences. First, government policy is a *partial* constraint on action: government decisions only partially determine social states. From this a second point follows: it is possible for people to suffer welfare losses *within* a given status quo policy. Let us define the "private sector" rather unconventionally as the sum total of action within any fixed government policy. Then we may divide the class of all possible private sectors in two: (1) uniformly efficient private sectors, and (2) sometimes inefficient private sectors. The former will *never* impose welfare losses, and corresponds to a "market society" without external diseconomies (like the smoke in our example). The latter corresponds to society as we know it. I will discuss this more generally

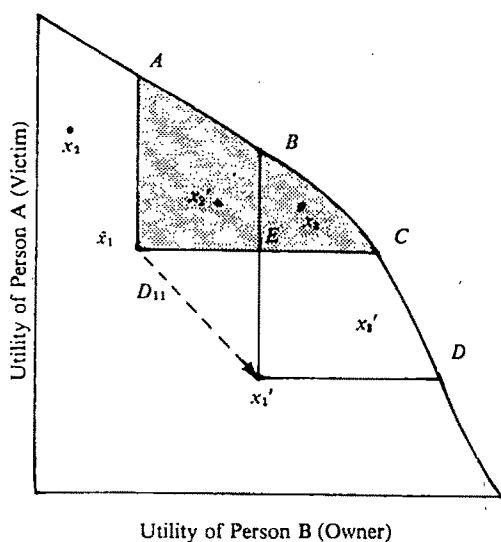
below, but let us apply it to the smokestack immediately.

Consider  $x_1$  to be a permissive government policy. Perhaps the permission is tacit, *nullum crimen*, because nobody has anticipated the social cost of chimneys. In the initial position, we map  $\bar{x}_1$  onto the welfare surface of  $A$  and  $B$  as we have it in Figure 4. Then the chimney's prospective owners, acting within  $\bar{x}_1$ , site their factory and it begins to belch smoke. They have, we assume, gained from this enterprise. If  $B$  is an owner, this is reflected as an eastward shift in the space defined by Figure 4. But, again within  $\bar{x}_1$ , some local residents have lost utility. If  $A$  is one of these victims, this is accounted as a southerly shift in Figure 4. The result is a *drift in the utility of the status quo policy*. The drift, which is itself efficiency undecidable, is traced by the stasis outcome,  $D_{11}$ . It is a *change in social welfare without a change in government policy*. On this new telling, the story begins not at  $\bar{x}_1$  but at  $\bar{x}_1'$  in Figure 4. This detail destroys the optimistic history.

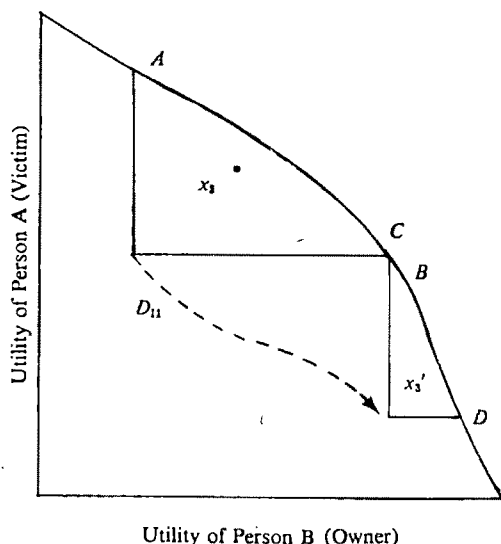
The consensus-efficiency hypothesis is split by this. In Figure 4, we begin at  $\bar{x}_1$  (no smoke, no prohibition). Overall or *aggregate* efficiency, therefore, is defined with respect to the shaded set of outcomes bounded by  $A-\bar{x}_1-C-A$ . Only by achieving outcomes in this set, and avoiding outcomes elsewhere, can we claim aggregate conformity to the efficiency principle. And, only by this result can we give substance to the notion of consent—protecting citizens from coercive losses of welfare. But efficiency also has a marginal or *incremental* interpretation. We find ourselves at  $\bar{x}_1'$ , and efficient increments from here are bounded by the line  $B-\bar{x}_1'D-B$ , so that precisely the confirmation of implications (i) and (ii) for (incremental) governmental action detaches the effect of action from the prospect of aggregate efficiency.

This analysis forces us to revise our earlier claims. First, what does it mean that consensual decision rejects efficiency undecidable changes as per implication (i)? Suppose, in our chimney story, that a proposal is rejected. Perhaps the rejected decision is an order to close the chimney along with compensation which the owners judge inadequate. By hypothesis, this must be something other than an efficiency improvement in incremental terms. In Figure 4, it therefore lies outside  $B-\bar{x}_1'-D$ . There are two normatively relevant locations for the rejected proposal in the remaining space. Like  $x_2$ , it may be an efficient change in neither the incremental nor the aggregate sense. Its rejection is therefore appropriate. But, it may also resemble  $x_2'$ : conforming to aggregate but not incremental efficiency. In this case, its rejection will violate implication (ii): failing, that is, to adopt an efficiency improve-

<sup>79</sup> See Barry, p. 256 ff, in connection with this and succeeding criticisms of Buchanan and Tullock.



4a



4b

Figure 4. The Difference between Aggregate and Incremental Efficiency

ment (in the aggregate). The troublesome set of rejected alternatives (like  $x_2'$ ) have the following property: they assure both  $A$  and  $B$  their pre-drift utility levels, but fail to assure  $B$  his post-drift utility level. The set is bounded by  $A-\bar{x}_1-E-B$  in Figure 4a. Consensual decision permits  $B$  to veto these policy changes, and therefore denies us the inference that any aggregate improvement of efficiency will be selected. In other words, implication (ii) holds in the increment but not the aggregate

for a society whose private sector permits efficiency-undecidable drift.

But consider the converse case. According to implication (ii) consensus will lead to the adoption of any alternative which is an efficiency improvement. We re-examine this inference from the view of aggregate efficiency. Suppose an outcome is adopted. Perhaps a sufficient bribe for the chimney's closure. By hypothesis, this outcome is incrementally efficient, and thus lies within  $B-\bar{x}_1'-D$  in Figure 4a. There are again two relevant possibilities. Like  $x_3$ , the new outcome may be an aggregate improvement. This is good. But, like  $x_3'$ , it may be efficiency-undecidable in the aggregate. This will be so if  $B$  equals or surpasses his post-drift utility level but  $A$  fails to reach his pre-drift level. The embarrassing set of outcomes with this property is bounded by  $E-\bar{x}_1'-D-C$  in Figure 4a. Consensual decision withholds from  $A$  the authority to impose his initial level of utility, and therefore denies us the inference that adopted consensual decisions will be efficient in the aggregate. Implication (ii) is therefore false in the aggregate for societies whose private sectors permit efficiency-undecidable drift.

The chimney case is in fact even more distressing than this analysis indicates. If there is to be any hope of aggregate efficiency within incremental consensus, then there must be a non-empty set of outcomes which are improvements over both  $\bar{x}_1'$  and  $\bar{x}_1$ . The test for membership in that set: an outcome must exceed each person's utility threshold both before and after drift occurs. The critical edge of this is in our case:  $A$  must reach his pre-drift level without  $B$  going below his post-drift level. The set of such points would be  $B-E-C$  in Figure 4a. Consider the common solution:<sup>80</sup>  $A$  and his fellow victims pay  $B$  and his fellow owners a sufficient bribe. If this is offered, it must exceed  $A$ 's post-drift level of utility, but will it also exceed his pre-drift level? Surely not, for he loses something he values (his share of the bribe), but gains only what he had in the first place (clean air). Only if he derives the side-payment from some effect of the chimney's operation—a job in the factory?—does it seem so much as possible that aggregate efficiency will be achieved. Indeed, if such a byproduct is ruled out, there seems every reason to think Figure 4b is appropriate: the drift is sufficient to collapse the set of incrementally and aggregately efficient points:  $B-E-C$  has evaporated. If this is so, we are forced to the still more pessimistic conclusion that the consensual outcome cannot possibly be an aggregate improvement of efficiency. In Figure 4a,  $x_3$  will be rejected against  $\bar{x}_1'$ , and this stasis outcome,  $D_{11}'$ , is not efficient in aggregate. But  $x_3'$

<sup>80</sup> See note 79.

will be adopted. The latter, efficient in the increment, is not efficient in the aggregate. Since this depends on special features of the example chosen, we will not claim any generality for it. We do, however, note it as a prospect, and insist that it denies any guarantee that successive movements will return the outcome to aggregate efficiency.

If we commit the (remissable) sin of interpersonal comparison, a further difficulty emerges. Suppose *A* initiates a change within the status quo which gains him a little, say 1 International Util. The social cost may be very great, perhaps 5 International Utiles from each of a thousand persons. If this is so, and an appropriate medium of exchange happens to exist, it may become evident that no change can return us to aggregate efficiency even if we are willing to expropriate assets corresponding to *A*'s full gain of 1 utile. There is nothing in the doctrine of consensual decision to refute this conjecture. Except, of course, the denial of interpersonal comparison. But this leads to more serious difficulty still. Suppose only one person is damaged by the drift in question. The chimney's shadow ruins a rose garden. Without interpersonal comparison, we cannot claim that  $7-1$  gains—it is perhaps a People's Chimney—will balance out this single loss. Surely, this is not a satisfying response to the difficulties we have been exploring.

It may be argued that we have missed our own point. We began with the hypothesis that efficiency maxima will be achieved consensually, but we have discussed only transitions from one sub-maximal point to another. Granting that consensus will not assure aggregate efficiency in such transitions, it may be argued that maxima will nevertheless be achieved. Indeed it is easily possible to reach an efficient equilibrium by a decision which violates implication (i) by imposing an efficiency-undecidable change that turns out to reach a maximum. (See Fig. 5.)

This may of course happen, but then consensual decision holds no special distinction: majority-rule,<sup>81</sup> dictatorship, or perhaps the Oracle of Delphi might also accomplish the same result. Indeed drift itself might press the community to an efficient maximum. These possibilities suggest that the maximum criterion by itself implies neither consensual decision nor any fixed right of consent in policy. But consensual decision still holds two distinctions. First, if it follows implications (1) and (2) in aggregate, then consensus offers us a "safe" line of travel toward maxima. This is the idea of our optimistic history in Figure 3. Second, ask yourself how we are to know that a maximum has been achieved. Assuming that the

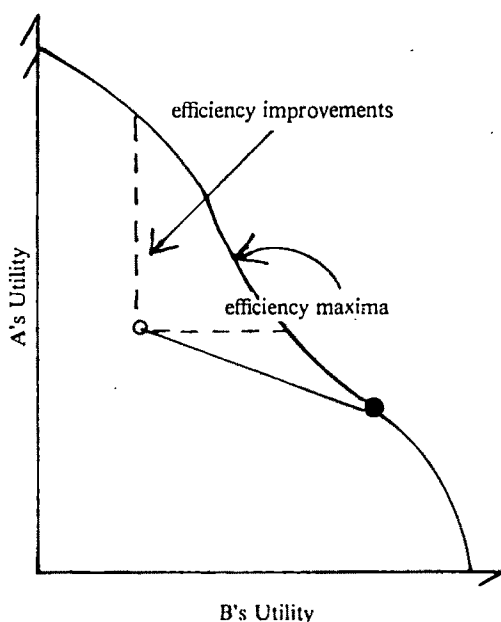


Figure 5. Reaching an Efficiency Maximum by an Efficiency-Undecidable Change

number of possible states is very large or infinite, we will not be able to identify efficiency maxima by any casual inspection. Nor can we use the shortcut assumptions of economics since we lack goods of the kind they consider. Under the complex conditions of government decision, the fact that an outcome is a maximum will emerge as an *inference from experience*. Even with drift, stasis consensus offers the appropriate laboratory: if protracted search produces no acceptable change, we are entitled to the practical hunch that a maximum has already been achieved. Of course we will not stay at a maximum merely because we fail to change policy, which renders this slight comfort.

### III. Buchanan and Tullock on the Elimination of External Cost

Let us pause to formulate this in the "external cost" language proposed by Buchanan and Tullock. An external cost is what "... the individual expects to endure as a result of the actions of others over which he has no direct control."<sup>82</sup> Like Locke—and Calhoun and Wolff and Wicksell—these authors are thinking mainly about the threat posed by *public* power over the individual: the cost of action through government. They do not say so, but it is otherwise impossible to comprehend their most fundamental conclusion: "Only the unanimity rule will insure that all external effects will be eliminated by collectiviza-

<sup>81</sup> Buchanan and Tullock, p. 186 recognize this prospect where side-payments are available.

<sup>82</sup> Buchanan and Tullock, p. 45.

tion."<sup>83</sup> Or, "... only if the consent of all members of the group is required will he be free of all expectations of external costs."<sup>84</sup> This is the punch behind the general claim with which we began—consensus as a means of utilitarian efficiency.<sup>85</sup>

The claim that consensus eliminates external cost is equivalent to claiming that implication (1) is fulfilled: only efficient changes occur.<sup>86</sup> (This is because other changes will damage some people and therefore exact external costs.) With some qualification, we have accepted this as a property of consensual decision in the increment. But, critically, we have shown it not to hold in the aggregate, unless the private sector is "perfect." If we confine our attention to the former case, what Buchanan and Tullock say is correct. If we consider the latter, what they say is not correct. In this latter instance, we are compelled to consider *two* forms of external cost, two classes of harmful acts over which we lack direct control. First, changes of government policy may be carried over our objection. This is eliminated by consensus. Second, private or civil actions, consistent with existing government policy, may damage our interests. If we cannot control these by governmental initiative, surely they are relevant external costs. Precisely because it rules out the first sort of cost, consensus permits those who benefit from these actions and the resulting "drift" to veto our efforts at government intervention.

It follows immediately that Buchanan and Tullock err when they offer the conclusion that unanimity excludes external costs—even if decision-making costs are ignored. Yet it may be claimed that the private sector *is* perfect, permitting no efficiency-undecidable drift. But in this case, the same authors would have urged that the activity in question not be subject to governmental decision.<sup>87</sup> They propose that government consider only imperfect portions of the private sector, but these are exactly the sectors in which the relevant difficulty occurs. Moreover, as Olson has suggested this seems to imply the optimality of an anarchic solution similar to Calhoun's.<sup>88</sup> But this is a slight exaggeration. At what point would a man expecting to eliminate future external costs agree to the sort of consensual government decision we have been discussing? At the point when he believed that existing government

policy, with no further change, would eliminate all external effects occasioned by the private conduct of others; when, in other words, a code corresponding to a perfect private sector was in hand.

### IIII. Perfection of Private Sector as a Condition of Consensual Efficiency

A perfect private sector—a society in which no harm was done to others except by action of government—is historically implausible. I cannot explore the point fully here, but three observations seem at once obvious and important. First, let us suppose I claim that some present public code brooks no imperfection. How are we to test my hypothesis? Presumably by waiting for someone to claim that his welfare has been diminished under existing policy. Apart from the sheer statistical brittleness of the hypothesis,<sup>89</sup> and allowing that the status quo would include a court system capable of absorbing small and middle-sized exceptions,<sup>90</sup> we still confront an appalling epistemological dilemma: How are we to judge the accuracy of any claims which do arise? Suppose Smith is the claimant: Am I to be the judge? Or are we to poll the society? Or is Smith alone to judge? If we choose any but the last alternative we confront the prospect of disagreement, perhaps a Smith-Rae debate. How is this to be settled? I know no way which is consistent with consensual decision, and that seems the appropriate criterion unless our activity is an idle one. I our hypothesis is politically relevant—its violation having some relevance to policy—we should resolve it consensually. But if the alternatives become "Smith is right" or "Rae is right," then on-disputant must prevail over the other, probably Smith over Rae, yet this is hardly consensual.

Second, unless the citizenry is dull as dirt or the makers of the public code utterly clairvoyant, it seems to me very nearly certain that innovation will occur, creating relations not proscribed by present policy, which are nonetheless misfortune for some groups and individuals. The Pigouvian chimney is an example, but an easy one readily

<sup>89</sup> If the probability of an exception is some small number,  $p$ , and there are  $n$  citizens with independent chances of being harmed, I should expect the hypothesis to stand in only one out of  $(1 - p)^n$  trials. With  $p$  greater than, say, .01, and  $n$  greater than 1,000, this comes to practically nothing. And regardless what non-zero value  $p$  takes, the relevant function approaches zero as  $n$  increases toward infinity.

<sup>90</sup> But such a system of courts would be odd: judges could not make law, and could not therefore cope with ambiguity. This suggests, however, either that the courts would be virtually inert or that the law would be very different from any law we presently know. It would be utterly mechanical and lack "open texture" of the sort H. L. A. Hart describes so interestingly in *The Concept of Law* (Oxford: Oxford University Press, 1961).

<sup>83</sup> *Ibid.*, p. 39.

<sup>84</sup> *Ibid.*

<sup>85</sup> Above, Section IC, or Buchanan and Tullock, p. 285.

<sup>86</sup> *Ibid.*, pp. 43–62.

<sup>87</sup> *Ibid.*

<sup>88</sup> Mancur Olson, Jr., "The Principle of 'Fiscal Equivalence': the Division of Responsibilities among Different Levels of Government," *The American Economic Review*, Papers and Proceedings, 59 (May, 1969), 479–87, the point in question is at p. 480.



■faced down by the simplest of zoning ordinances. Here are some general categories which illustrate the point.<sup>91</sup>

- (1) Changes in the technology of productive activity imposing material diseconomies on others, including the marginalization of workers.
- (2) Pecuniary diseconomies resulting from market transactions, such as wage inflation or the price fluctuation of goods under changing demand by third parties.
- (3) Declines of welfare resulting from static codes of behavior in the face of generational change in values.
- (4) Declines of welfare arising from static policy and changes in the size or distribution of population.
- (5) Declines of welfare resulting from changing patterns of personal decision with complementarity between individual behaviors.
- (6) Historical change in the material environment brought about by the cumulative effect of past and present human behavior.

All these patterns will occasion "drift" of the sort analyzed in IIIF unless they are fully anticipated by government policy. This is simply implausible as a long-term proposition. And once the drift occurs, we have seen that stasis consensual decision will not necessarily protect us by achieving aggregate efficiency.

Finally, the analysis given here is unfair to the doctrine of consensus. The critique of its claim to efficiency does not, strictly speaking, apply to consensual decision *itself*. It applies rather to the combination of consensual decision in government policy and the lack of consensual decision for behavior within government policy. If *everything* were decided consensually, then incremental and aggregate efficiency would coincide, and would be at least approximated by consensual decision. Drift would not occur, for each action would require unanimous consent. The private sector would be perfectly efficient precisely because it would be empty: everything would be a governmental policy. This is wildly implausible sociologically, and utterly unattractive to the liberal conscience. It is the odd suggestion that consensual decision will achieve its stated relation to utilitarian efficiency precisely as the government's agenda becomes total.<sup>92</sup>

<sup>91</sup> The relevant literature is stupendous. Two at once entertaining and informative analyses are offered by William F. Baxter, "A Parable," *Stanford Law Review*, 23 (May, 1971), 973-77, and Thomas Schelling, "Hockey Helmets, Concealed Weapons, and Daylight Saving," Kennedy School Discussion Paper No. 9, 1972. Schelling's famous paper, "On the Ecology of Micromotives," *Public Interest*, 25 (Fall, 1971), 61-98, is also worth consulting, along with Barry p. 256ff.

<sup>92</sup> The nearest historical approximation seems to occur within certain small utopian communities such as the Hutterite colonies of Western Canada. Cf. Victor Peters, *All Things Common* (Minneapolis: University of Minnesota Press, 1965).

### III-I. Free Exit and the Failure of the Market Analogy

A main conclusion: *Consensual decision will cash its guarantee—assure utilitarian efficiency—precisely where politics itself seems unnecessary—i.e., in a perfect private sector.* This last, however improbable, would make politics a risky luxury: If no harm can befall me under static policy, yet losses can be inflicted by governmental action, why should I not choose to end political history?

But what has become of the "market analogy?" I am afraid the answer is that the relation between market transactions and governmental choices of policy will not bear so strong an assertion of similarity. The distinguishing property of free exchange is ease of escape: a well-functioning market permits each trader to *exit* any relationship he finds odious. Suppose it didn't: I could buy salt only from you. You could of course use your veto over the exchange outcome to exact a blackmail price from me, because of the market's consensual structure—because, that is, I have no right of consent in the status quo. The voluntary quality of the market arises from the freedom of exit and the ready availability of substitute partners.<sup>93</sup> But government is another matter: one can typically escape governments only at a high price, measured in material and psychic dislocation. If the government chooses to sell me salt at monopoly prices—some have—my line of escape is clogged, and the right of consent in a *change* of policy far from helpful. "Drift" of the kind discussed above can be escaped cheaply by traders, but not by citizens. And it is at just this point that the market analogy fails.

With it goes the marriage of consent and utility in consensual efficiency. It is no comfort that increments of governmental change honor this union if aggregates of social change abuse it. And this last must be expected in a world which needs politics and in that way resembles our own experience. I turn now to a finishing conjecture.

### IV. One Implication

I will withstand the (very slight) temptation to repeat the essay's main points, and finish instead a single line of conjecture which leads to an odd conclusion. We began with consent as a response against autocracy. The critical transition from consent in regimes to consent in their policies, presents an historical embarrassment, and the doctrine of consensual decision speaks in answer to it. The doctrine has grown subtler, but retains the mark of its birth: a single-minded attention to *public* power and a corresponding inattention to its alternatives. If it were possible to put these

<sup>93</sup> See Albert Hirschmann, *Exit, Voice and Loyalty* (Cambridge, Mass.: Harvard University Press, 1970).

aside by granting an unconditional right of consent to everyone, then the doctrine of consensual decision would be an appropriate ideal for government decision. It is not possible to do so. I do not mean to raise the sociological difficulties occasioned by entrenched elites, or the division of labor in politics, or the mere costs of talk and newsprint. If the obstacles to unconditional consensus were of this order, it would provide an intelligent guide to reform—an optimum to be approximated in the face of difficulty. The objection is a deeper one which arises from the inner structure of decision making itself: choices must have outcomes, and the structures by which they are made must contain points of vulnerability so that these may be achieved. These in turn deny a universal right of consent, no matter what configuration society takes. If this is so, then any unconditional ideal of consensual decision must present an *inherently* unreachable structure which cannot be approximated by actual institutions.

Whatever practical ideal of consensual decision is available must therefore be conditional: must leave open a nonconsensual path to some outcomes. In the theories considered by this essay, the line of escape leads to private as opposed to public forms of power. With Calhoun's anarchic consensus, we are vulnerable—without a right of

consent—to the nullification of positive government policy. The *laissez-faire* proposal of Wicksell leaves us vulnerable to the bare market outcome. The stasis consensual system of Buchanan and Tullock leaves us vulnerable to action within the status quo. I personally believe these proposals should be rejected because their points of vulnerability are more than a little unattractive under the historical circumstances of 20th-century society. Others may of course disagree, but I hope at least that the analysis clarifies the central question. If we are to decide by an approximation of consensus for some outcomes, we must surrender that right for other outcomes: What should they be?

There is, in fact, exactly one structure of decision which escapes this choice. Such a structure would grant somebody an unconditional right of consent in *every* outcome for every issue. This cannot be a scheme which grants more than one person a right of consent in some incomplete class of outcomes—like the structures examined here. It likewise cannot be a nonconsensual structure like majority-rule democracy. Given the criterion of robustness, it can brook no disagreement within the consenting class. A moment's reflection suggests that the only such structure for the formation of policy in society is pure autocracy.

## Comment on Rae's The Limits of Consensual Decision"

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On the whole, I am very much an admirer of Douglas Rae's article. Indeed, if the editor will not object to my revealing this fact, I was one of the referees who recommended its publication. I find myself with two objections to it, however, and as I suppose the reader will not be very surprised to learn, these concern its treatment of the general approach to politics which James Buchanan and I put into *The Calculus of Consent*.<sup>1</sup> These objections fall into two general fields: first, the intellectual history of the doctrine of consent, and second, the methodological importance of Pareto optimality.

With respect to the intellectual history, Rae, I think, may have been misled by the literature. The origins of the doctrine of consent or consensus, which he traces through political scientists from Locke and Calhoun, were actually quite disparate from those of the doctrine of Pareto optimality, which was drawn from economics, and there is no obvious reason to believe that the earlier political thinkers influenced the economists particularly. Pareto, for example, was an Italian engineer and Wicksell a Swede. Their educations would have had little emphasis on Locke, and I doubt that they had even heard of Calhoun.

The positions they adopted were essentially attempts to deal with technical problems which had arisen in economic analysis in their particular fields. It was only after their positions had been developed that the resemblance between their position and that of certain earlier political philosophers was noticed. Even to this day there is a great difference between the two. I myself, for example, have never read so much as one page of Calhoun and have no desire to do so. Similarly, my knowledge of Locke and John Stuart Mill and even Bentham is largely derived from secondary sources. Buchanan, on the other hand, who is interested in philosophy, has done a good deal of reading in these areas, but I must say even in his case he is totally ignorant of the original text of Calhoun's work.

The fact that there is some resemblance between the two points of view has, however, led to a number of recent writers, who are writing in what I might call the economic tradition, to refer to these political philosophers. In fact, what we have are

two totally different streams of intellectual development which happen to lead to somewhat similar ends. I should like to emphasize the "somewhat" in the previous sentence. In my opinion, the resemblance between, shall we say, Locke's views and those of Wicksell is very superficial. The modern Paretian economists are essentially motivated by technical problems in economic analysis and not by the grand considerations of traditional philosophy which motivated Locke and Mill. Certainly they are not prompted by the desire to protect slavery which, in my opinion, was the foundation of Calhoun's political philosophy.<sup>2</sup>

In modern economic thinking, unanimity or the Pareto principle is not thought of as a practical policy but as part of an analytical machine. No one suggests that unanimity be used as a practical means of making decisions and, indeed, there are probably more economists than political scientists who are opposed to requiring unanimity for the jury. This is of some interest because the jury is the only major part of our present institutional system requiring unanimity. Of course, this is not a Pareto optimal decision, since the two parties to the litigation are not permitted to vote.

Economic analysis very commonly proceeds by figuring out what the result would be if unanimity were required and there were no transactions costs. The strength of that second requirement—no transactions costs—is immense, and it is very difficult to imagine any real-world situation which meets it. Nevertheless, after this calculation has been made, the adding on of transactions costs turns out to be a convenient approach. It can readily be demonstrated that, leaving distributional matters aside, with zero transactions costs you do reach an optimal system, although there obviously may be many other optimal outcomes.

In the real world, of course, there are transactions costs, and so far as I know, every economist is aware of that fact. Indeed, the addition of transactions costs to the analytical apparatus is, in a way, the heart of modern economic reasoning, whether in economics proper or in the new extensions of economics into previously noneconomic fields.

Note, however, that Pareto optimality is as

<sup>1</sup>James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

<sup>2</sup>Defenders of Calhoun will no doubt wish to emphasize that I have never read so much as one page by Calhoun himself.

much a restriction on market processes as it is on political processes. Rae's example of the chimney involves the assumptions that we do not require unanimous consent for a "private" decision and that, in the chimney case, with large externalities we do require it for a political decision. Requiring unanimity throughout will not reach the same solution. No one suggests that we require unanimous consent for all private decisions, e.g., that Rae may not acquire an additional cat without my permission; but the reason we do not is simply that the transactions costs would be too high, and hence we shift to a system which saves on transactions costs.<sup>3</sup> In *The Calculus of Consent* we applied the same rule to political activities. Like the economist, we first applied the Paretian apparatus and then considered the effect of the transactions costs on it.

The argument for this procedure is, I think, strong, but it is widely recognized that income redistribution raises different issues. The problem, of course, is that if I am compensated for having my income reduced, the end result is that my income is not reduced. Thus, compensation is not possible in this case. A certain amount of income redistribution could conceptually achieve unanimous consent. In *The Calculus of Consent* we pointed out that people making decisions on institutions are making decisions for a future which is not certain. Under the circumstances, they may be interested in requiring a certain amount of income redistribution as a way of reducing their own personal risk in this not-perfectly-predictable future. The close resemblance between this pattern of reasoning and Rawls's, of course, is clear.<sup>4</sup> He makes the ignorance of the future complete by way of his "veil of ignorance." While we did discuss complete ignorance of the future and the possibility under these circumstances of getting general agreement to a high degree of redistribution in that future, that ignorance, in the real world of course, would not be complete.

Another motive for income redistribution in a Pareto optimal system is discussed by Harold M. Hochman and James D. Rodgers in their famous article, "Pareto Optimal Redistribution."<sup>5</sup> They

<sup>3</sup> As a matter of fact, the market process is Pareto optimal in those cases in which there are no externalities. In cases in which all externalities are trivial (which is, after all, the common case for market transactions), it is a close approximation of Pareto optimality.

<sup>4</sup> John Rawls, *A Theory of Justice* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1971).

<sup>5</sup> Harold M. Hochman and James D. Rodgers, "Pareto Optimal Redistribution," *American Economic Review*, 59 (September, 1969), 542-57. The whole discussion of the economics of income redistribution is summarized by Anthony J. Culyer, *The Economics of Social Policy* (New York: Dunellen, 1973).

point out that people are, to some extent, charitable, and hence would be interested in progress. income redistribution. There are reasons having to do with externalities that suggest that the government should be used for this redistribution. This article set off a spate of further articles on the same point, but also stimulated an examination yet a third category of possible Pareto-optimal income redistribution by Geoffrey Brennan. Brennan discusses primarily redistribution and taken for what we might call precautionary motives, i.e., avoidance of revolution, etc.

But it is not obvious that the amount of income redistribution that would be obtained through these Pareto-optimal techniques would be thought by the reader to be ideal. The problem here is to decide what is the proper amount. Once we have determined that, it is very easy to modify the status quo point in such a way that we will always have this ideal income distribution. For example, suppose it were thought that the ideal income distribution were one that provided just half as much inequality as now exists. We could simply enact world law providing that 50 per cent of all income above \$500 be taken by some redistribution agency,<sup>7</sup> and this be used to supplement all incomes below \$500 by half of the difference between the income and the \$500.<sup>8</sup> Having made a constitutional decision, we could, then, apply Pareto optimality subject to this particular general set of rules. Note, however, that the basic decision would not be Pareto optimal.

A great many people, however, feel that the decision-making process should be so designed to generate an appropriate amount of equality rather than putting the equality in as something before the decision-making process. Presumably if we knew the correct amount of redistribution and if this amount were greater than one would get under the Pareto-optimal type of system, we could design a constitution which would generate that amount of redistribution. Clearly this would require knowledge we do not have now, but it does not seem an impossible task for future research. However, one cannot justify a particular method of voting—say majority voting—on the ground that the income redistribution that it generates (which we observe around us) is optimal, if ■

<sup>6</sup> Geoffrey Brennan, "Pareto Desirable Redistribution: The Non-Altruistic Dimension," *Public Choice* 14 (Spring, 1973), 43-68.

<sup>7</sup> Calculated on the method suggested in Henry Simon, *Personal Income Taxation* (Chicago: University of Chicago Press, 1938), rather than that used by the Internal Revenue Service.

<sup>8</sup> This calculation is very rough and is based on preliminary figures. It might be that the point at which one begins taking 50 per cent under present circumstances would be \$600 or even \$700. Note that this is in addition to existing taxes.

only evidence that it is optimal is that it is generated from majority voting. Thus, you can argue for majority voting on some other grounds and then point out that it generates a particular amount of income redistribution.<sup>9</sup> On the other hand, it is possible that we might have an idea obtained by some other line of reasoning about what is the optimal amount of redistribution, and then seek out the voting rule which achieves it. Except by coincidence, this would not be majority voting. But we must not engage in circular reasoning. Rae does not, but in several parts of his article I felt that he was on the verge.

It will be observed that my criticisms of Rae's article are not really severe. He is unhappy with the present situation in the social sciences in which the conceptual use of unanimity is our only method of obtaining optimality other than simply deciding that we know what is good for other

people. I, too, am unhappy about it. *The Calculus of Consent*, although most political scientists may not realize this, actually made a major step away from the unanimity criterion by demonstrating that Pareto optimality at the constitutional level would not entail Pareto optimality at the operational level. Since then, in *The Logic of the Law*,<sup>10</sup> I have attempted another line of reasoning, admittedly very close to Pareto optimality, which I hope may eventually be a step toward a better welfare criterion.

The number of problems raised by Pareto optimality is not an argument for any other particular system. Like Rae, I would like something else. I suspect that I am unlike Rae, however, in believing that the disadvantages of majority voting as a conceptual tool are greater than those of Pareto optimality. I suspect this because, although Rae seems to have intended to defend majority voting against Pareto optimality, at no point in his article does he actually offer any positive argument for majority voting. Most (not all) of his criticisms of Pareto optimality are just; but in order to choose the best tool, we must undertake comparative evaluation rather than simply pointing out that one tool has its defects.

<sup>9</sup> It should be emphasized that the bulk of the income redistribution obtained in modern welfare states is not from the wealthy to the poor, but from certain members of the middle class to other members of the middle class. There is, indeed, some aid to the poor and some money removed from the wealthy, but this is a relatively modest phenomenon in most modern democracies. See Gordon Tullock, "The Charity of the Uncharitable," *Western Economic Journal*, 9 (December, 1971), 379-92.

<sup>10</sup> Gordon Tullock, *The Logic of the Law* (New York: Basic Books, 1971).

## Rejoinder to "Comment" by Gordon Tullock

DOUGLAS RAE

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Alas, Professor Tullock's remarkable temperance and generosity leave no room for a quarrel. But it may be worthwhile to comment on three aspects of his piece:

First, the connection between the early constitutionalists (say, Locke) and the Paretian economists (say, Wicksell or yourself). If Professor Tullock wants to say that the former exert no (direct, literal) influence upon the latter we are nearly agreed. But my concerns are more complicated. I want to show—apart from important indirect patterns of influence and tradition—that fundamental structural problems persist from one era (say 1688) to another (say 1789, or 1975). It is not so interesting to see that earlier workers influence later ones; but it is fascinating to see that their *tasks* retain their shape. By this, I do not mean anything literal enough to collide with Tullock's view that "the modern Paretian economists are essentially motivated by technical problems in economic analysis and not by the grand considerations of traditional philosophy which prompted Locke and Mill." I want, really, to say that there is a structural level at which technical problems of contemporary economics *are* the problems of liberal constitutionalism. The Pareto principle is not on the surface much like Locke's doctrine of consent or Bentham's principle of the greatest good. But beneath words lies the struc-

tural fact that Paretian economics amalgamates both of these earlier arguments in confronting problems of the same size and shape as those faced by Locke and Bentham. It hardly needs saying that Paretian economics can be technical or scientific without for a moment ceasing to be political.

Professor Tullock generously acknowledges that some of the criticisms offered against *The Calculus*—and other Paretian works—do in fact stick. He does, however, indulge the temptation to say that "it can readily be demonstrated that, leaving distributional matters aside, with zero transactions costs you do reach an optimal system, although there may be many other optimal outcomes, of course." This leaves three loopholes: (i) what counts as distribution, (ii) what counts as transaction, and (iii) at which level "optimal system" is to be defined. If (i) and (ii) are defined to include *whatever* might get in the way of (iii) you are on safe ground. I doubt, however, that either of us would want to embrace this tautology, and I remain convinced that the critique of *Calculus* . . . is mostly right.

Finally, Professor Tullock and I now agree that the Pareto principle won't do as a general rule of constitutional design. We need only to agree on what should take its place.

# Prerequisites Versus Diffusion: Testing Alternative Explanations of Social Security Adoption\*

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Social security is one of the most important means by which modern nations protect the welfare of their citizens. Through programs that deal with the hardships of workers' injury, illness, old age, unemployment, and low income, social security attempts to set a minimum standard of living for the sectors of society covered by the programs. In countries with fully developed programs, social security now protects nearly all members of society.

Given the importance of social security, it is hardly surprising that scholars have shown considerable interest in analyzing its evolution. Among the many aspects of social security development, the timing of the first adoption of social security merits particular attention. The circumstances of the first appearance of any policy are inherently interesting, and the first appearance of social security is particularly important because it has represented in many nations a major break with the antiwelfare doctrine of traditional liberalism.

The timing of first adoption has received considerable attention in comparative research on social security.<sup>1</sup> However, this research has gen-

erally used the timing of adoption to explain other aspects of the social security experience of nations, and not as an outcome that is itself to be explained.<sup>2</sup> The present research is concerned with explaining the timing of the first adoption of social security among the 59 countries which had formal political autonomy with regard to domestic policy at the time of first adoption (see Appendix).

The analysis focuses on two of the most important explanations of social security development: the prerequisites explanation,<sup>3</sup> which emphasizes causes of social security development *within* nations, most commonly the level of social and economic modernization; and diffusion, which focuses on the imitation of social security programs *among* nations. These alternative theoretical approaches have received very unequal attention in political research. The prerequisites approach has been widely used, particularly in the area of comparative politics.<sup>4</sup> By contrast,

George Allen and Unwin, Ltd., 1968); and Koji Taira and Peter Kilby, "Differences in Social Security Development in Selected Countries," *International Social Security Review*, 22 (1969), 139-153.

<sup>2</sup>In quantitative research, the only exception of which we are aware is a two and a half page analysis in Appendix E-12 in Pryor, *Public Expenditures*. Historical studies such as Gaston V. Rimlinger, *Welfare Policy and Industrialization in Europe, America and Russia* (New York: John Wiley & Sons, 1971) have also attempted to explain timing of adoption.

<sup>3</sup>This expression is used loosely here to refer to what Marion Levy has labeled functional and structural prerequisites. In using the expression prerequisites, we are following his distinction between the *prerequisites* for the appearance of a given phenomenon and the *requisites* for its continued existence. See Marion J. Levy, Jr., *The Structure of Society* (Princeton: Princeton University Press, 1952), pp. 62-63 and 71-72.

<sup>4</sup>Examples of cross-national studies that examine various forms of the prerequisites and requisites hypotheses (see footnote 3) with regard to democratic political outcomes are S. M. Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review*, 53 (March, 1959), 69-105; James S. Coleman, "Conclusion: The Political Systems of the Developing Areas," in Gabriel A. Almond and James S. Coleman, eds., *The Politics of the Developing Areas* (Princeton: Princeton University Press, 1960)

\* This is a revised version of a paper presented at the 1973 Annual Meeting of the Midwest Political Science Association, Chicago. The research was supported by grants from the Cross-Cultural Fellowship Program and the Honors Division of Indiana University and by a Ford Foundation Political Science Faculty Research Fellowship. John V. Gillespie played a major role in stimulating our concern with the place of diffusion in cross-national research, and Ruth B. Collier provided helpful comments on earlier drafts of the article. We are obviously solely responsible for the final form which the article has taken.

<sup>1</sup>See Margaret Gordon, *The Economics of Welfare Policies* (New York: Columbia University Press, 1963); Phillips Cutright, "Political Structure, Economic Development and National Social Security Programs," *The American Journal of Sociology*, 70 (March, 1965), 537-550; Phillips Cutright, "Income Redistribution: A Cross-National Analysis," *Social Forces*, 46 (December, 1967), 180-190; Henry Aaron, "Social Security: International Comparisons," in Otto Eckstein, ed., *Studies in the Economics of Income Maintenance* (Washington, D.C.: The Brookings Institution, 1967); Frederick Pryor, *Public Expenditures in Communist and Capitalist Nations* (Homewood, Ill.:

diffusion explanations have received only occasional attention in political analysis, though they have been widely used in other areas of research.<sup>5</sup>

The present study is based on the belief that it is extremely important to bring these alternative explanations together in comparative political analysis. It is important not only on substantive grounds—because they are both plausible expla-

nations of many aspects of political life—but also on methodological grounds. This is due to Galton's problem, the problem that the finding based on the analysis of causal relations within nations (or other units of analysis) may be distorted by the effect of diffusion.<sup>6</sup> This occurs when the joint diffusion of two or more societal characteristics produces high correlations between them which lead researchers to infer incorrectly that they are causally related. Incorporating hypotheses about diffusion into the analysis in effect turns this problem into an opportunity to consider a type of explanation that has previously received limited attention in political research. We recognize that it may be impossible to test definitively the relative importance of causation within a unit of analysis, as opposed to diffusion.<sup>7</sup> The present research has the more modest goal of attempting, through a combination of simple tests and the presentation of qualitative evidence to reach tentative conclusions about the relative importance of diffusion as opposed to prerequisite explanations.

It is also hoped that this research will add to the variety of types of diffusion that are considered in diffusion analysis. Some writers have argued that the diffusion approach to the study of modernization has placed excessive emphasis on the spread of innovations from advanced centers to back-

pp. 532-576; Everett E. Hagen, "A Framework for Analyzing Economic and Political Change," in *Development of the Emerging Countries*, ed. Robert E. Asher et al. (Washington, D.C.: The Brookings Institution, 1962) pp. 1-38; Phillips Cutright, "National Political Development: Measurement and Analysis," *American Sociological Review*, 28 (April, 1963), 253-264; Dick Simpson, "The Congruence of Political, Social, and Economic Aspects of Development," *International Development Review*, 6 (June, 1964), 21-25; Hayward R. Alker, Jr., "Causal Inference in Political Analysis," in Joseph Bernd, ed., *Mathematical Applications in Political Science*, II. Arnold Foundation Monographs, XVI (Dallas: Southern Methodist University Press, 1966) pp. 7-37; Deane E. Neubauer, "Some Conditions of Democracy," *American Political Science Review*, 61 (December, 1967), 1002-1009; Marvin E. Olsen, "Multivariate Analysis of National Political Development," *American Sociological Review*, 33 (October, 1968), 699-712; Phillips Cutright and James A. Wiley, "Modernization and Political Representation: 1927-1966," *Studies in Comparative International Development*, 5 (1969-1970). For studies in this tradition using political participation as a dependent variable, see Daniel Lerner, *The Passing of Traditional Society* (Glencoe, Ill.: The Free Press, 1958); Hayward Alker, "Causal Inference in Political Analysis"; Donald J. McCrone and Charles F. Cnudde, "Toward a Communications Theory of Democratic Political Development: A Causal Model," *American Political Science Review*, 61 (March, 1967), 72-79.

<sup>5</sup> References to the idea of diffusion may be found in much political research (see, for example, Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown and Company, 1965), p. 2, but the number of studies that have presented explicit tests for diffusion is limited. These have included Stewart A. Rice, *Quantitative Methods in Politics* (New York: Alfred Knopf, 1928); Robert L. Crain, "Fluoridation: The Diffusion of an Innovation Among Cities," *Social Forces*, 44 (June, 1966), 467-476; Robert D. Putnam, "Toward Explaining Military Intervention in Latin American Politics," *World Politics*, 20 (October, 1967), 102-3; Jack L. Walker, "The Diffusion of Innovations Among the American States," *The American Political Science Review*, 63 (September, 1969), 880-899; Manus Midlarsky, "Mathematical Models of Instability and a Theory of Diffusion," *International Studies Quarterly*, 14 (November, 1970), 60-84. For a discussion of systematic attempts to examine the role of diffusion in social security adoption, see the discussion later in this article. The literature on diffusion outside of political science is usefully summarized by Everett M. Rogers and F. Floyd Shoemaker, *Communication of Innovations*, 2nd ed. (New York: The Free Press, 1971) and John C. Hudson, *Geographical Diffusion Theory* (Evanston, Ill.: Northwestern University, Department of Geography, 1972).

<sup>6</sup> See Raoul Naroll, "Galton's Problem: The Logic of Cross-Cultural Research," *Social Research*, 32 (Winter, 1965), 428-451; Brian J. L. Berry, "Some Methodological Consequences of Using the Nation as a Unit of Analysis in Comparative Politics," paper prepared for the Committee on Comparative Politics of the Social Science Research Council, 1970; John V. Gillespie, "Galton's Problem and Parameter Estimation Error in Comparative Political Analysis," paper prepared for delivery at the Annual Meeting of the Midwest Political Science Association, Chicago, 1970 and Richard E. Hildreth and Raoul Naroll, "Galton's Problem in Cross-National Studies," paper prepared for the Committee on Comparative Politics of the Social Science Research Council, 1970.

<sup>7</sup> Berry has made this point with particular reference to spatial diffusion. He has argued that whereas "in time series a natural distinction exists between past and future, no such property characterizes spatial series. Dependence will extend in a variety of directions, and often on vectors at angles to the Cartesian grid, leading to elaborate cross-product locational terms. . . . [M]ost of the functions introduced by statisticians into the field of spatial processes have been introduced simply because the mathematics exists, as extensions of time-series analysis, without thought for their usefulness or interpretability. And even more significant, all of the existing models rely on an assumption of stationarity, i.e., that the relation between values of the processes is the same for every pair of points whose relative positions are the same. This is patently invalid." See Brian J. L. Berry, "Problems of Data Organization and Analytical Methods in Geography," *Journal of the American Statistical Association*, 36 (September, 1971), 521.



ward peripheral areas.<sup>8</sup> One of the subpatterns of diffusion which is examined in the present research—diffusion from a somewhat less advanced country to progressively more advanced countries—represents an entirely different pattern through which an innovation can spread. The presentation of this finding may suggest to other researchers opportunities for analyzing less conventional patterns of diffusion.

In focusing on the timing of social security adoption, this research also addresses the issue of the timing and sequence of different aspects of modernization. Several writers have recently attempted to measure the timing of certain aspects of modernization,<sup>9</sup> and a number of authors have explored hypotheses regarding the sequence in which different aspects of modernization occur.<sup>10</sup>

<sup>8</sup> See Susanne J. Bodenheimer, *The Ideology of Developmentalism: The American Paradigm-Surrogate for Latin American Studies*. Sage Professional Papers in Comparative Politics, Vol. 2 (Beverly Hills, Cal.: Sage Publications, 1971), pp. 22ff and A. Eugene Havens, "Methodological Issues in the Study of Development," paper prepared for delivery at the Third World Congress of Rural Sociology, Baton Rouge, Louisiana, August, 1972.

<sup>9</sup> See W. W. Rostow, *The Stages of Economic Growth* (London: Cambridge University Press, 1960), p. 38, and *Politics and the Stages of Growth* (London: Cambridge University Press, 1971), p. 55; C. E. Black, *The Dynamics of Modernization: A Study in Comparative History* (New York: Harper and Row, 1966), pp. 90ff; Arthur S. Banks and Robert B. Textor, *A Cross-Polity Survey* (Cambridge: The M.I.T. Press, 1963), pp. 78–80; Steven L. Barsby, "Economic Backwardness and the Characteristics of Development," *Journal of Economic History*, 29 (September, 1969), 449–473; Fernando H. Cardoso and Jose Luis Reyna, "Industrialization, Occupational Structure, and Social Stratification in Latin America" in *Constructive Change in Latin America*, ed. Cole Blasler (Pittsburgh: University of Pittsburgh Press, 1968), pp. 25–26; and David Collier, "Timing of Economic Growth and Regime Characteristics in Latin America," *Comparative Politics*, 7 (April, 1975), 331–360.

<sup>10</sup> See, for instance, Eric A. Nordlinger, "Political Development: Time Sequences and Rates of Change," *World Politics*, 20, No. 3 (April, 1968), 494–520; Peter Heintz, *Ein Soziologisches Paradigma der Entwicklung* (Stuttgart: Ferdinand Enke Verlag, 1969); Richard A. Pride, *Origins of Democracy: A Cross-National Study of Mobilization, Party Systems and Democratic Stability*, Sage Professional Papers in Comparative Politics Vol. 1 (Beverly Hills, Cal.: Sage Publications, 1970); Sidney Verba, "Sequences and Development," in *Crises and Sequences in Political Development*, ed. Leonard Binder et al. (Princeton: Princeton University Press, 1971), pp. 283–316; Robert A. Dahl, *Polyarchy* (New Haven: Yale University Press, 1971); William Flanigan and Edwin Fogelman, "Patterns of Political Development and Democratization: A Quantitative Analysis," in *Macro-Quantitative Analysis*, ed. John V. Gillespie (Beverly Hills, Cal.: Sage Publications, 1971), pp. 441–473 and "Patterns of Democratic Development: An Historical Comparative Analysis," in Gillespie and Nesvold, pp. 475–497.

The present research demonstrates that there are systematic variations in the sequence of social security adoption in relation to other aspects of modernization. The most important overall relationship is the tendency for later adopters to adopt at far lower levels of social and economic modernization. It is argued that this tendency is in part an aspect of the more general tendency toward a larger role of the state in later developing countries, and that it is also due to certain special characteristics of social security as a public policy.

### The Development of Social Security

Social security is generally considered to consist of five distinct programs which provide cash payments to individuals to replace income lost as a result of injury related to employment, sickness and maternity, old age, and unemployment, and to supplement income for families with children, usually, though not always, on the basis of need. In many countries, medical services are provided directly by the state in place of the system of cash reimbursements for medical expenses. Social security differs from the many types of private insurance arrangements, according to standard definitions, in that it is initiated by the state, although it may not be administered or financed entirely by the state.<sup>11</sup> Following the example of most comparative analyses of social security, this research does not consider insurance programs maintained by the state for its own employees.

Although social security first appeared in Europe at the end of the last century, its origins must be seen within the context of growing state intervention in private mutual aid societies in the nineteenth century.<sup>12</sup> Mutual aid societies are known to have existed as early as ancient Grecian times and have been present in Western Europe from the Middle Ages and down to the present day.<sup>13</sup> They have ranged greatly in size and scope: some have consisted of groups of neighbors who set aside a small sum each week to cover funeral expenses,<sup>14</sup> while others have counted their membership in the thousands and provided not only

<sup>11</sup> This is a paraphrase of the definition found in U.S. Department of Health, Education and Welfare, Social Security Administration, *Social Security Programs Throughout the World, 1971* (Washington, D.C.: Government Printing Office, 1971), p. ix.

<sup>12</sup> For a detailed discussion of the early development of social security, see Vladimir Rys, "The Sociology of Social Security," *Bulletin of the International Social Security Association*, 17, No. 1 (January-February, 1964), 3–34; and Rimlinger, *Welfare Policy and Industrialization*.

<sup>13</sup> Rys, "The Sociology of Social Security," p. 6.

<sup>14</sup> J. M. Baernreither, *English Associations of Working Men* (London: Swan Sonnenschein & Co., 1889, republished in 1966 by Gale Research Company, Detroit), p. 165.

burial expenses, but sickness and accident insurance as well.

As the size of the industrial labor force began to grow in the eighteenth and nineteenth centuries in Europe, these societies grew as well, both in size and number. This proliferation brought an increasing number of cases in which, because of mismanagement or fraud, societies went bankrupt and the contributors lost their equity. As bankruptcies continued, government regulation of the funds began to appear. While the type of regulation varied from country to country, it generally included an audit to insure that the funds were actuarially sound. In some cases where regulation was not mandatory, incentives such as free postage or tax exemptions were given to those funds which accepted it.<sup>15</sup> In other cases, state intervention went even further in relation to certain dangerous occupations, such as mining. For instance, an 1854 law in Prussia made membership in mutual aid funds compulsory for all workers in mines, quarries, and salt works.<sup>16</sup>

According to the definition cited above, which identifies social security programs as insurance schemes that are *initiated*, and not merely regulated or required, by public law, social security first appeared in Germany in 1883 with the adoption of a program to cover the costs of sickness and maternity. A work injury program was adopted a year later, and a pension program in 1889.<sup>17</sup> Other programs have been slower in coming, even in Germany. Unemployment insurance was enacted in 1927, and family allowances were established in the German Democratic Republic in 1950 and in the Federal Republic of Germany in 1954.

This pattern of gradual development of social security has characterized all aspects of program growth. For instance, it has taken seven decades for work-injury programs—generally the first program to be adopted in each country—to spread to all 59 countries considered in this study. The appearance of other programs within each country and the broadening of the coverage of existing programs has also occurred gradually. Even in the most advanced countries, additions to the scope of coverage and the type of benefits offered continue to occur.<sup>18</sup>

<sup>15</sup> U.S. Department of Commerce and Labor, *Twenty-Fourth Annual Report of the Commissioner of Labor, 1909. Workmen's Insurance and Compensation Systems in Europe* (Washington, D.C.: U.S. Government Printing Office, 1911), p. 8.

<sup>16</sup> Robert Morse Woodbury, *Social Insurance: An Economic Analysis* (New York: Henry Holt & Co., 1917), p. 8.

<sup>17</sup> These and other dates are from U.S. Department of Health, Education, and Welfare, *Social Security Programs*.

<sup>18</sup> For an excellent discussion of this phenomenon of gradual growth in Latin America see "Gradual Ex-

### The Timing of Social Security Adoption

The principal variable used in this analysis is the timing of the adoption of the first social security program in each country. The dates for first adoption were taken from *Social Security Programs Throughout the World, 1971*.<sup>19</sup> The use of this source follows the precedent set by Aaron, Pryor, and Taira and Kilby, who derived measures of program age solely on the basis of the date of first adoption, and by Cutright, who constructed a measure of cumulative social security experience that is closely related to it.<sup>20</sup>

Because this study focuses on the first adoption of social security, it is appropriate to assess the extent to which the timing of first adoption is closely related to the timing of the emergence of other aspects of social security.<sup>21</sup> The examination of evidence about social security development reveals, not surprisingly, considerable variation in the way that social security first appears in each country. In some cases, initial laws with relatively wide coverage are quickly followed by additional programs in a pattern of rapid social security development. In other cases, a first law of limited coverage may be passed which is not followed by additional legislation for many years.

The data show, however, that the development of social security follows a surprisingly consistent pattern in which the different aspects of development are closely interrelated. For instance, the five different types of programs are adopted in a remarkably consistent order, to such an extent that the pattern of adoption among nations forms a Guttman Scale.<sup>22</sup> The correlations between the date of first adoption and the dates of adoption of the five types of programs are also high. Since the first program is usually work injury, the correlation with that program is nearly perfect, .98. The correlations of first adoption with pensions, sickness and maternity, unemployment, and family allowance are .79, .74, .53, and .22 respectively. The low correlation for the last program may be

tension of Social Insurance Schemes in Latin American Countries," *International Labour Review*, 68 (September, 1958), 257-283.

<sup>19</sup> U.S. Department of Health, Education, and Welfare (Washington, D.C.: U.S. Government Printing Office, 1971). The limitations of this source are discussed in the Appendix.

<sup>20</sup> See Aaron, "Social Security"; Fredrick Pryor, *Public Expenditures*; Taira and Kilby, "Differences in Social Security Development;" and Cutright, "Political Structure, Economic Development and National Social Security Programs" and "Income Redistribution."

<sup>21</sup> The procedure followed here is that recommended by Przeworski and Teune for confirming the reliability of an indicator. See Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: John Wiley and Sons, 1970), pp. 114-115.

<sup>22</sup> Cutright, "Political Structure, Economic Development and National Social Security," p. 540.

xplained by the qualitatively different issues involved in family allowance programs.<sup>23</sup>

Timing of first adoption is related to other aspects of social security as well. Cutright, Aaron, Pryor, and Taira and Kilby have all found that indicators based on the timing of adoption are strongly correlated with present level of social security spending as a percentage of gross national product, with the earlier adopters having consistently higher levels of spending.<sup>24</sup> Among the countries considered in the present study, the correlation with the percentage of the gross national product devoted to social security in 1960 is  $-.81$ .<sup>25</sup> The timing of first adoption is also strongly related to the proportion of the population that is covered by social security. For the two programs for which reasonably complete data are available—work injury and pensions—the correlations are  $-.77$  and  $-.71$ , respectively.<sup>26</sup>

Available data thus suggest that first adoption is strongly related to several other important aspects of social security development. This provides a firmer basis for arguing that the timing of first adoption is a valid indicator of the timing of over-all social security development and does not represent just an isolated aspect of it.<sup>27</sup>

### Prerequisites Explanations of Social Security Development

The prerequisites approach treats the development of social security as a result of the social and economic transformations associated with the transition from primarily agricultural to industrial economies. Within this perspective, one of the most important hypotheses is that the decline in the proportion of the work force in agriculture increases the need for social security. Pryor argues that "although agricultural workers have considerable welfare needs, they can make certain provisions for such purposes that are nonmonetary in nature and that are unavailable for those who have left the land."<sup>28</sup> Food and shelter are more readily acquired through nonmonetary means in agricultural settings, and it is easier to make nonmonetary provisions for sickness and old age in an agricultural setting in which extended family networks are often of greater importance. There unquestionably exist types of agricultural laborers for whom these nonmonetary means are not available, and the extent of the decay of extended family networks in cities has at times been exaggerated. Nevertheless, there are important differences in the availability of these nonmonetary provisions in rural as opposed to urban areas.<sup>29</sup>

Other writers have focused on the growth of industry as a crucial factor in the development of social security. Cutright argues that the percentage of the work force in industry is an indicator of the vulnerability of the population to drastic changes in income arising from unemployment.<sup>30</sup> Organski also emphasizes the importance of industrialization, both because it provides the opportunities for organization and leadership

<sup>23</sup> See J. Henry Richardson, *Economic and Financial Aspects of Social Security: An International Survey* (Toronto: University of Toronto Press, 1960), pp. 139–155 for a discussion of the issues involved in family allowance programs.

<sup>24</sup> See Cutright, "Income Redistribution"; Aaron, "Social Security"; Pryor, *Public Expenditures*; and Taira and Kilby, "Differences in Social Security Development." It should be noted that whereas these correlations are treated, in the present research as a means of assessing the validity of an indicator these authors have used them to examine causal relations among different aspects of social security experience.

<sup>25</sup> The negative sign of this correlation and the two others reported in the next paragraph results from the fact that timing of adoption is measured by the year of adoption, and hence involves a smaller number for the earlier adopters. The measure of spending is taken from *The Cost of Social Security*, published by the International Labor Organization (Geneva: 1967). This correlation is based on data on 34 countries.

<sup>26</sup> The data on coverage are taken from International Labor Office *The Yearbook of Labor Statistics, 1961* (Geneva, 1961). The correlation for work injury is based on 27 cases, and that for pensions on 30 cases.

<sup>27</sup> Probably the most important aspect of social security development—the quality of the benefits offered to those who are covered—is extremely difficult to measure. One might hypothesize that timing of adoption and quality of services are strongly associated, but that as one moves from earlier to later adopters, the lag between the first adoption of programs and the growth in the quality of programs would be greater and greater. Wolf's discussion of the quality of services in Latin America—a region in which the first adoption occurred nearly three decades after the first adoption in Germany—would appear to support this hypothesis. He suggests that dilution of the quality of service, long delays in insurance pay-

ments, and wide-spread corruption and bribery are found even in the most advanced countries in Latin America. See Marshall Wolfe, "Social Security and Development: The Latin American Experience," in *The Role of Social Security in Economic Development*, ed. Everett M. Kassalow (Washington, D.C.: U.S. Department of HEW, Social Security Administration, Research Report No. 27, 1968), pp. 155 and 165.

<sup>28</sup> Pryor, *Public Expenditures*, pp. 134–135.

<sup>29</sup> For a useful discussion of the literature that has overstated the importance of the transition for rural to urban life, see Wayne A. Cornelius, Jr., "The Political Sociology of Cityward Migration in Latin America: Toward an Empirical Theory," in *Latin American Urban Research, I*, Francine F. Rabinovitz and Felicity M. Trueblood, eds. (Beverly Hills, Cal., Sage Publications, 1971) pp. 95–150. In the present context it is important to note that opportunities for nonmonetary provisions of certain types may be present in cities. See, for instance, William Mangin's "Latin American Squatter Settlements: A Problem and a Solution," *Latin American Research Review*, 2, No. 3 (Summer, 1967), 65–98.

<sup>30</sup> Cutright, "Income Redistribution," p. 184.

that make it possible for workers to demand protection, and because it provides the higher productivity that makes social security financially possible.<sup>31</sup>

Pryor's finding that labor union strength is related to the timing of the adoption of social security appears to give support to the first of these hypotheses.<sup>32</sup> The relationship between productivity and social security is more complex, but also appears to support Organski's argument. Measuring productivity as gross national product per capita, Pryor finds that social security spending is related to productivity only if the sample of nations includes the entire range of economic modernization. If only the semi- or fully-developed economies are considered, there is no relationship. A similar finding emerges in Pryor's time-series analysis, and Taira and Kilby actually find a negative relationship among the most advanced countries.<sup>33</sup> Pryor interprets his findings by suggesting that productivity is an underlying, but not an immediate determinant of expenditures.<sup>34</sup> It would appear that increases in productivity at earlier levels of modernization produce more spending, but that variation in productivity makes no difference once it has gotten above a certain level. Since the first adoption of social security generally occurs at the lower levels of modernization, however, it appears that productivity may be a relevant explanation for first adoption.

The argument that the need for old-age pensions increases as economic modernization proceeds is also part of the prerequisites thesis. Pryor notes that the proportion of the population over 65 increases as per capita income rises, thereby increasing the need for pensions.<sup>35</sup> Rimlinger argues with reference to the United States that in addition to a change in the age composition of the population, the difficulty of accommodating older workers in mass production organization also increases the need for pensions as economic development proceeds.<sup>36</sup> Aaron specifically suggests that the aging of the population may be one factor that explains the timing of adoption.<sup>37</sup>

Another important theme in the social security literature—the human capital argument—may also be subsumed under the heading of prerequi-

sites explanations. This is the argument that as the capital equipment used in industry becomes more complex and more expensive, it is important to make a concomitant investment in human capital as well.<sup>38</sup> A recent study has suggested that greater investments in the welfare of workers actually increase productivity only at the middle levels of modernization.<sup>39</sup> Nonetheless, it is widely believed that there is a link between the welfare of workers and productivity, and the human capital argument has often been used by social security advocates because of its obvious appeal to the self-interest of employers.<sup>40</sup>

In examining prerequisites explanations of social security adoption, it is important to assess the kinds of assumptions that this approach requires. It might first be emphasized that it need not require the assumption that all nations will adopt social security at precisely the same level of modernization. A more realistic application of the prerequisites thesis might distinguish between the idea that a particular threshold level of modernization is a necessary, but not a necessary and sufficient, condition for adoption. If it is only a necessary condition, one would expect that cases of adoption below a certain threshold of modernization would be rare, but that nations might vary over a considerable range above the threshold in their level of modernization at adoption. If, however, a particular threshold level is both a necessary and sufficient condition, adoption should cluster closely around that level.

We must emphasize that the prerequisites thesis does not require a simplified conception of modernization. The departure of workers from agriculture, their entry into industry, increases in productivity, and many other changes that influence the likelihood of social security adoption may occur in a different order, or to differing degrees, in particular countries, and some of them may never occur at all in certain nations. The prerequisites argument does not assume that they necessarily occur together, but rather that to the extent that one or a combination of them does occur, the adoption of social security becomes more likely.

It is, however, an implicit assumption of much comparative research that has dealt with the prerequisites theme that causal relations exist only within nations. This is not because these writers explicitly reject diffusion, but rather because the statistical procedures employed in much of this

<sup>31</sup> A. F. K. Organski, *The Stages of Political Development* (New York: Alfred K. Knopf, 1965), p. 162.

<sup>32</sup> Pryor, *Public Expenditures*, p. 474.

<sup>33</sup> *Ibid.*, pp. 145–146 and Taira and Kilby, "Differences in Social Security Development," p. 142.

<sup>34</sup> Pryor, p. 146.

<sup>35</sup> *Ibid.*, p. 135.

<sup>36</sup> Rimlinger, *Welfare Policy and Industrialization*, p. 209.

<sup>37</sup> Aaron, "Social Security," pp. 32–33.

<sup>38</sup> See Rimlinger, pp. 103–104.

<sup>39</sup> Walter Galenson, "Social Security and Economic Development: A Quantitative Approach," *Industrial and Labor Relations Review*, Vol. 21, No. 4 (July, 1968), 559–569.

<sup>40</sup> Gaston V. Rimlinger, "Welfare Policy and Economic Development: A Comparative Historical Perspective," *Journal of Economic History*, 26 (December, 1966), 568.

search require the assumption of independence among units if erroneous conclusions are to be avoided. This difficulty of comparative research—called Galton's problem—has been noted earlier. One means of confronting this problem is to make diffusion explicitly part of the analysis. Analysts of social security can readily deal with this methodological problem by turning to the analysis of diffusion, since the qualitative literature on social security has already devoted considerable attention to diffusion effects.

### Diffusion Explanations of Social Security Adoption

The diffusion approach views social security adoption as taking place within an international system of communication and influence. As Rys has pointed out, since "... no modern State exists in splendid isolation, independent of the international community that surrounds it, the study of factors which influence the establishment of social insurance in a country will necessarily have to go beyond the frontiers of a national community and be considered also at the international level."<sup>41</sup>

A few examples from the literature on social security may serve to illustrate the types of imitation that occur. One type involves cases in which nations have directly imitated the legislation of other countries. For instance, the Italian insurance system for workers' pensions and sickness established in 1898 is a synthesis of corresponding laws in France and Belgium, and the first programs in Austria were modeled after those in Germany.<sup>42</sup> This latter imitation involved not only the overall form of the program, but technical details as well.<sup>43</sup>

In other cases that do not involve direct imitation, existing programs have had an important influence on new adopters. Lloyd George, who played a considerable role in the adoption of some of the early programs in the United Kingdom, is known to have been greatly influenced by the programs previously adopted in Germany and Belgium.<sup>44</sup> Various groups in the United States commissioned studies of European social security systems with the purpose of appraising the feasibility of social security in the United States. This included a 2,749-page, two-volume study of European systems of workers' insurance and

compensation published in 1911 by the U.S. Commissioner of Labor "in response to the very great demand in this country at the present time for information concerning all aspects of this subject."<sup>45</sup> Though it was many years before social security was adopted on the national level in the United States, the European experience had considerable influence on programs adopted at the state level.

International organizations and international agreements concerning reciprocity of benefits also play a role in the diffusion of social security. For instance, as a result of a reciprocal agreement with France, Belgium was forced to revise its entire social security system to bring it up to the French standard.<sup>46</sup> The activities of the Correspondence Committee of the International Labor Organization played a major role after the First World War in spreading the idea of social security from Europe to other parts of the world and supporting the consolidation of the already existing systems.<sup>47</sup> Since that time, the activities of groups such as the International Social Security Association, the Ibero-American Social Security Organization, and the Inter-American Social Security Conference have promoted the standardization of social security legislation.

Although the literature on social security provides considerable evidence that diffusion is present, only limited attention has been given to systematic analyses of patterns of diffusion. Pilcher, Ramirez, and Swihart have studied diffusion effects in the selection of the normal age for retirement under national pension programs, analyzing such factors as the influence of nations on their neighbors, common language, and colonial experience.<sup>48</sup> The strongest relationship appears for common language, with mixed results for the other two factors. Taira and Kilby have shown that geographical location may be important in social security development.<sup>49</sup> They find clear dif-

<sup>45</sup> U.S. Department of Commerce and Labor, *24th Annual Report*, p. xv. The National Association of Manufacturers sent a group to Germany in 1909 to study the social insurance system there, and the Russell Sage Foundation also sponsored a study of European systems of social insurance in 1908. See Rimplinger, "Welfare Policy and Economic Development," p. 566 and Lee K. Frankel and Miles M. Dawson, *Workingmen's Insurance in Europe*, (New York: Russell Sage Foundation, 1910).

<sup>46</sup> U.S. Department of Commerce and Labor, *24th Annual Report*, p. 25.

<sup>47</sup> Isabel Craig and Igor Tomes, "Origins and Activities of the ILO Committee of Social Security Experts," *International Social Security Review*, 22 (1969), 509.

<sup>48</sup> Donald M. Pilcher, Charles J. Ramirez, and Judson J. Swihart, "Some Correlates of Normal Pensionable Age," *International Social Security Review*, 21, 3 (1968), 387-411.

<sup>49</sup> Taira and Kilby, "Differences in Social Security Development," p. 143.

<sup>41</sup> Rys, "The Sociology of Social Security," p. 4.

<sup>42</sup> U.S. Department of Commerce and Labor, *24th Annual Report*, pp. 1862-1863 and V. Klumppar, "The Investment of Social Insurance Funds," *International Labor Review*, 27 (January, 1933), 53.

<sup>43</sup> U.S. Department of Commerce and Labor, *24th Annual Report*, p. 44.

<sup>44</sup> Thomas Jones, *Lloyd George* (Cambridge, Mass.: Harvard University Press, 1951), pp. 36-37.

ferences between countries in the European Common Market, other European countries, and non-European countries in levels of spending for social security.

The present research considers two patterns of diffusion that have received considerable attention in the general literature on diffusion: hierarchical and spatial. In the case of hierarchical diffusion, innovations appear in the most advanced or largest centers and are then adopted by successively less advanced or smaller units. Hierarchical diffusion has been observed for innovations as diverse as radio stations, trolley cars, electronic technology, policy innovations among the states of the United States, the diffusion of fluoridation among cities in the United States, and the diffusion of fire brigades and Rotary Clubs in Chile.<sup>50</sup> Hierarchical diffusion has also received considerable attention in research on diffusion among individuals, and Rogers has noted the consistent finding that earlier as opposed to later adopting individuals are more educated and of higher social status.<sup>51</sup>

Various causes of the hierarchical pattern may be identified. Larger or more developed units may be more disposed to innovations because they have more resources with which to experiment. They tend to have more information about and more contact with other units, and hence are likely to receive more information about an innovation once it begins to spread.<sup>52</sup> It might also be argued that smaller, less advanced units to some degree consciously adjust their behavior to larger, more advanced units, following a pattern of "positioning behavior" in which they are constantly imitating the behavior of the units that they have chosen as models.<sup>53</sup> This involves a pattern that is in a sense an extension of the two-step flow of communication described by Katz and Lazarsfeld and the queuing behavior among legislators discussed by Matthews and Stimson.<sup>54</sup>

<sup>50</sup> See Berry, "Problems of Data Organization," and Berry and Elaine Neils, "Location, Size and Shape of Cities as Influenced by Environmental Factors: The Urban Environment Writ Large," in *The Quality of the Urban Environment*, Harvey S. Perloff, ed. (Washington, D.C.: Resources for the Future, Inc., 1971); John E. Tilton, *International Diffusion of Technology: The Case of Semiconductors* (Washington: The Brookings Institution, 1971); Walker, "The Diffusion of Innovations Among the American States"; Crain, "Fluoridation," p. 572; and Paul O. Pedersen, "Innovation Diffusion Within and Between National Urban Systems," *Geographical Analysis* (July, 1970), pp. 203-254.

<sup>51</sup> Rogers and Shoemaker, *Communication of Innovations*, p. 186.

<sup>52</sup> Walker, "The Diffusion of Innovations," p. 885.

<sup>53</sup> The expression *positioning behavior* was suggested by Jack Walker in a personal communication.

<sup>54</sup> Elihu Katz and Paul F. Lazarsfeld, *Personal Influence* (Glencoe, Ill.: The Free Press, 1955) and Donald R. Matthews and James A. Stimson, "Deci-

An alternative type of diffusion is spatial diffusion: diffusion along lines of spatial proximity or, alternatively, along major lines of communication. There is a wide variety of possible lines of spatial communication, and the present research will only illustrate possible patterns and will not attempt to demonstrate definitively the relative importance of spatial as opposed to hierarchical diffusion. Indeed, hierarchical and spatial pattern may be closely interrelated, since economic growth itself has to a considerable degree diffused over a regular, spatial pattern. For this reason, diffusion that is ordered in terms of a hierarchy of economic growth may be spatially ordered as well.

### A Test of the Prerequisite Thesis

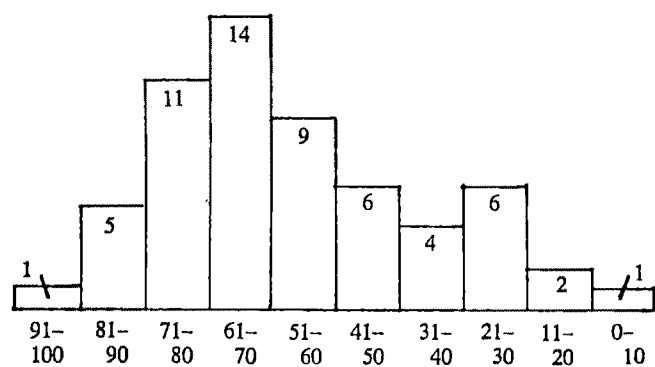
Under the heading of prerequisites explanations, a series of modernizing changes have been identified that presumably increase the likelihood that a nation will adopt social security. Two interpretations of the prerequisites thesis were suggested. On the one hand, it may be argued that it identifies only a necessary condition for adoption and that while countries would never fall below a given level of modernization at the time of adoption, there will actually be a wide variation above that level in the degree of modernization at adoption. Alternatively, there may be a certain level of modernization that is both a necessary and sufficient condition for adoption, such that adoption will tend to occur at roughly the same level of modernization in many or all countries.

Ideally, one would like to examine all of the characteristics of nations at the time of adoption that are suggested by the prerequisites arguments. However, because of the problem of finding data for the wide range of dates represented among the years of adoption,<sup>55</sup> sufficient data were available only for percentage of work force in agriculture, percentage of work force in industry, and real income per capita. The definition and sources of these measures are presented in the Appendix. In the following pages, the expression "level of modernization" is used to refer to these three indicators.

Table 1 shows the distribution of values for these three variables at the time of first adoption

sion-Making by U.S. Representatives: A Preliminary Model," in *Political Decision-Making*, ed. S. Sidney Ulmer (New York: Van Nostrand Reinhold Co., 1970), pp. 22-23.

<sup>55</sup> Even such outstanding sources as Stein Rokkan and Jean Meyriat, *International Guide to Electoral Statistics, Vol. 1: National Elections in Western Europe* (The Hague: Mouton, 1969), a major source of data on electoral participation, do not provide data on variables such as the proportion of the population voting in elections for enough countries at the appropriate points in time to permit any meaningful analysis.

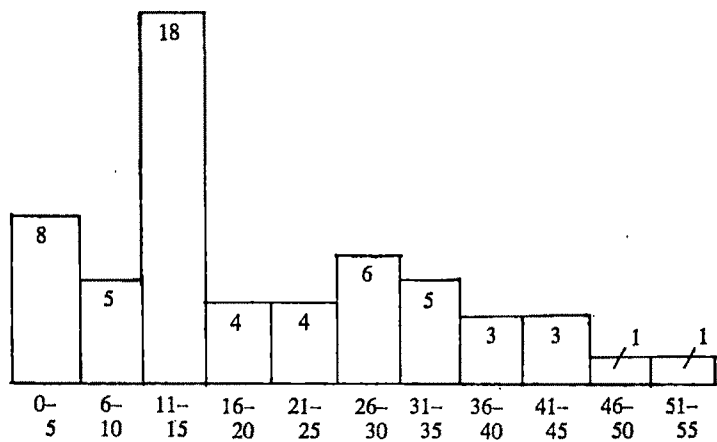


Percentage of the Work Force in Agriculture

Range

91 to 9

N=59

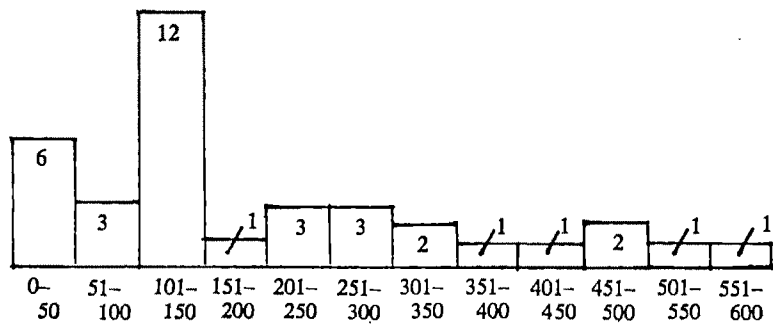


Percentage of the Work Force in Industry

Range

3 to 54

N=58



Real Income per Capita  
(Expressed in U.S. dollars around 1930)

Range

19 to 591

N=36

Table 1. Distribution of Level of Modernization at First Adoption of Social Security

of social security in each country. For work force in agriculture, there is clearly a clustering around a modal value, and for work force in industry and national income there is a small range (the third column in each case) that contains a substantial proportion of the cases. The range of variation is so wide for all three variables, however, that it appears to disconfirm the necessary and sufficient

condition version of the prerequisites argument. The percentage of the work force in agriculture at the year of first adoption ranges from nine per cent for the United Kingdom to 91 per cent for Saudi Arabia. Work force in industry ranges from three per cent for El Salvador to 54 per cent for Switzerland. Real income per capita, measured in units roughly equivalent to U.S. dollars around

1930, varies from 19 dollars for Ethiopia to 596 dollars for the United States and 504 dollars for the next wealthiest adopter, the United Kingdom.

The alternative interpretation of the prerequisites argument—that level of development is a necessary but *not* sufficient condition for adoption—is more strongly supported by these data, since for each variable there is a level below which few adoptions occur. There were no adoptions in countries with more than 91 per cent of their work force in agriculture, and only six countries had more than 80 per cent in agriculture at adoption. Work force in industry was as low as three per cent at adoption, yet there is a sharp increase in the number of cases in the 11 to 15 per cent range. For real income per capita, the lowest value is 19 dollars, with two other countries adopting at only slightly higher levels. Since the reader may be more familiar with income data expressed in terms of a more recent period, it may be noted that one 1930 dollar is worth about 2.7 1960 dollars.<sup>56</sup> The lowest level of income at adoption is thus 51 in terms of 1960 dollars. Though still very low, this figure gives a bit more support to the idea that there is a minimum level below which adoption does not occur. This level of modernization is so low, however, that the necessary condition version of the prerequisites argument would seem to have little explanatory power.

On the basis of this simple test, it therefore appears that the prerequisites argument, at least by itself, does not provide an adequate explanation of social security adoption. Other indicators of modernization might be found which produce better results, or there may be complex interactions among different aspects of modernization which, if properly represented in the test, would allow a better prediction of the timing of adoption. The analysis of diffusion, however, may provide a far simpler means of finding order in the apparent diversity of circumstances of social security adoption.

#### A Test for Hierarchical Diffusion

Considering first the possibility of hierarchical diffusion, a simple test is to examine the relationship between timing of adoption and level of modernization at adoption. If a pattern of diffusion is present in which countries tend to imitate other countries that are at higher levels of modernization, this should be reflected in a tendency for each successive adopter to adopt at a progressively lower level of modernization.

The tendency for later adopters to adopt at lower levels of modernization is shown clearly in

Figure 1, where modernization is measured as the percentage of the work force in agriculture.<sup>57</sup> Since the level of modernization is defined in terms of the year of adoption, this relationship does not really involve a prediction of the timing of adoption on the basis of level of modernization. Nonetheless, treating the variable measuring year of adoption as an interval scale, a correlation coefficient may be used to summarize the degree of consistency in the relationship. The correlation and hence the standardized regression coefficient is .61, reflecting a fairly strong tendency for later adopters to be less modernized at the time of adoption. Similar relationships appear if one uses the other two measures of modernization, except that the correlations have opposite signs because of the opposite polarity of the variables:  $-.61$  for work force in industry, and  $-.35$  for real income.<sup>58</sup> The lower correlation for real income may be due in part to the greater difficulty in comparing levels of income because of the effects of inflation and fixed exchange rates.

This tendency for social security to appear at an earlier stage of modernization among later developers doubtless arises in part from the desire noted by Rys for governments in the developing countries

to prove that they can give their populations the same protection other nations give to theirs, and in so doing only follow the general imperatives of social policy in any modern state.<sup>59</sup>

Lund has similarly noted the tendency for leaders in Third World countries to introduce social security out of a wish to acquire for their nations all of the most visible signs of national modernity.<sup>60</sup> A similar tendency has been noted with

<sup>57</sup> This method of illustrating the relationship between development and timing of adoption is similar to that used by quantitative geographers who have analyzed the diffusion of innovations among urban centers by plotting size of urban center at the time of adoption by the year of adoption for various types of innovations. See Berry, "Problems of Data Organization and Analytical Methods in Geography," p. 521; Berry and Neils, "Location, Size, and Shape of Cities," p. 299; and Pedersen, "Innovation Diffusion Within and Between National Urban Systems," pp. 209-212.

<sup>58</sup> Although there is considerable disagreement about the relevance of tests of significance for interpreting correlations in nonsample data, we will present them for readers who find them useful. These correlations for work force in agriculture and work force in industry are significant at the .01 level. That for real income is significant at the .02 level.

<sup>59</sup> Vladimir Rys, "Comparative Studies of Social Security: Problems and Perspectives," *Bulletin of the International Social Security Association*, Nos. 7-8 (July-August, 1966), 242-268, at p. 245.

<sup>60</sup> Michael S. Lund, *Comparing the Social Policies of Nations: A Report on Issues, Methods, and Resources* (Center for the Study of Welfare Policy, University of Chicago, 1972), p. 42.

<sup>56</sup> This coefficient is derived from the *Economic Report of the President, 1972* (Washington, D.C.: U.S. Government Printing Office, 1973), pp. 247-253.



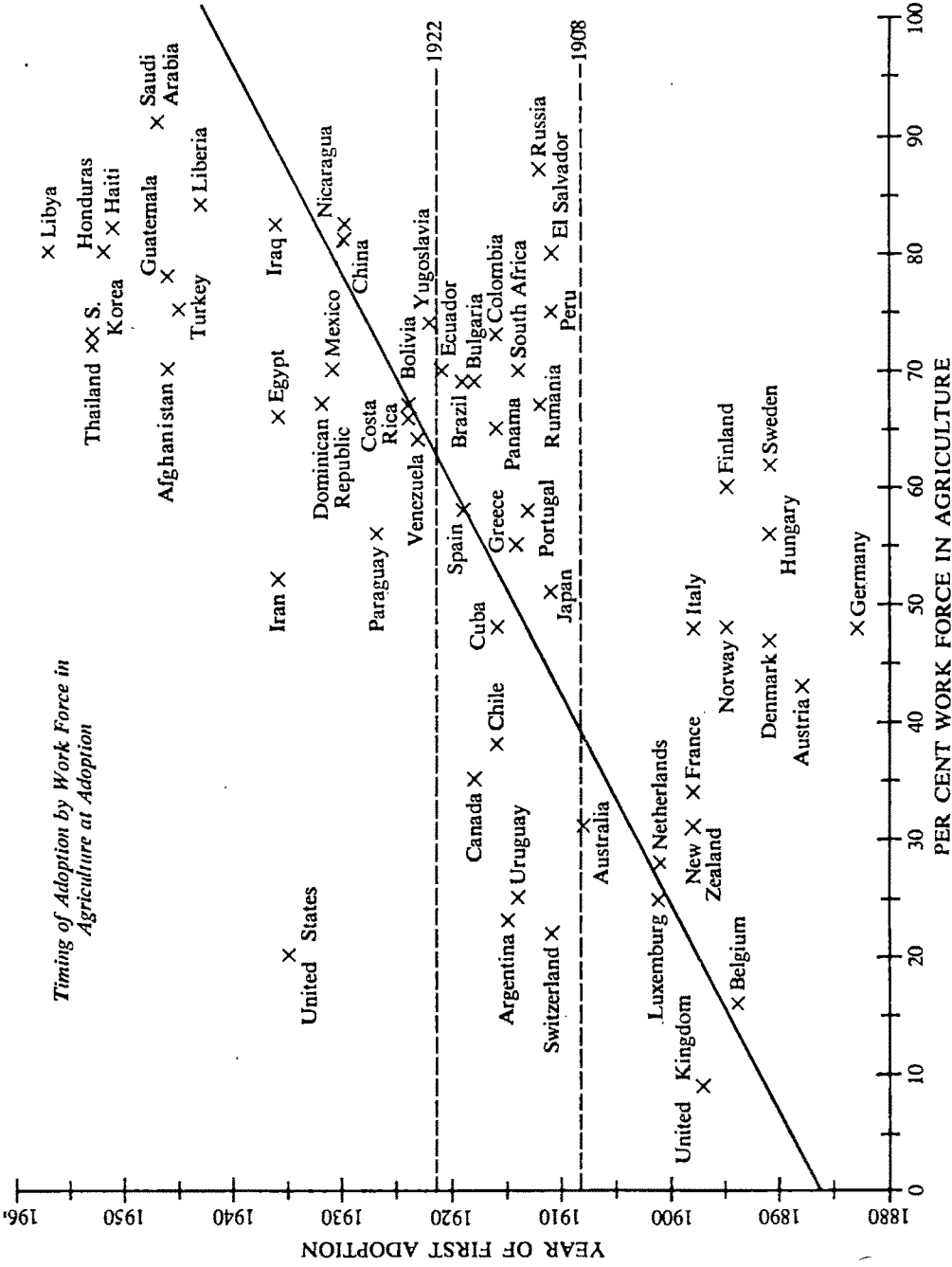


Figure 1

regard to the types of modern technology selected by developing countries.<sup>61</sup>

Aside from the question of the types of programs or technology that are selected, the pattern of social security adoption in Figure 1 may reflect a tendency toward a larger role of the state in society in later-developing countries. Dahrendorf and Gerschenkron have noted this tendency among European countries,<sup>62</sup> and it may be present in broader frames of comparison as well. Though some developing countries have had considerable experience with mutual benefit associations and conventional insurance, the introduction of social security by the state appears to come far earlier relative to the development of these private arrangements among later adopters than it did in Europe.<sup>63</sup>

The pattern of adoption may also be due in part to special characteristics of social security as a public policy. To the extent that it is financed by taxing employees who are covered by the programs, it has the unusual feature of being an easy form of taxation, since citizens may feel that they are not really paying taxes when they make social security payments, but rather that they are purchasing insurance. In addition, there is often an interval of many years between the payment of social security taxes and the retirement or illness for which the contributor is covered. Social security is therefore an easy form of taxation which produces large amounts of capital that are invested at the discretion of the social security administration. In some countries, this capital is loaned to the national government at low interest rates, or is distributed on a patronage basis to finance a variety of private or public projects.<sup>64</sup> It is easy to see that a government in a developing

country might be tempted to adopt social security even if the social and economic need for a program was more limited than it was at the time of adoption among the earliest adopters.

Another feature of social security that may account for this pattern of adoption is its tendency to weaken labor movements.<sup>65</sup> The use of social security in this co-optive way at earlier stages of development may be explained in part by the tendency for labor movements to appear at earlier stages of modernization in later developing countries.

#### Diffusion among the Earliest Adopters

In addition to the overall pattern of adoption reflected in Figure 1, three sub-patterns merit close attention. We will therefore present separate analyses of the earliest adopters, a middle group of adopters, and the later adopters.

Perhaps the most striking of these subpatterns is that among the earliest adopters. Within Europe, the first appearance of social security did not occur in the most advanced countries, but rather in 1883 in Germany, a later developer within the European context. The most advanced country—the United Kingdom—did not adopt until 1897. An examination of the lower part of Figure 1 reveals that though three countries did adopt at lower levels of modernization than Germany, there is a moderate but consistent tendency among the nations which adopted up until 1901 to do so at successively *higher* levels of modernization. The correlation for this group of countries is  $-.49$ , with the negative sign reflecting the tendency toward adoption by successively more advanced countries. It might be objected that this statistical finding depends largely on the fact that Germany was the first adopter and the United Kingdom was a later adopter for reasons perhaps unrelated to their level of modernization at adoption. However, the removal of these two countries leaves the correlation virtually unchanged. Similar patterns appear in the correla-

<sup>61</sup> Harry Johnson, ed., *Economic Nationalism in Old and New States* (Chicago: University of Chicago Press, 1967), pp. 1ff.

<sup>62</sup> See Alexander Gerschenkron, "Economic Backwardness in Historical Perspective," in *The Progress of the Underdeveloped Countries*, ed. Bert F. Hoselitz (Chicago: University of Chicago Press, 1952), pp. 3-29 and Ralf Dahrendorf, *Society and Democracy in Germany* (Garden City: Doubleday and Co., 1967).

<sup>63</sup> On the basis of data made available to the senior author by the Ministry of Labor in Peru, the earliest date of foundation of the mutual benefit associations which have received government recognition in that country is around 1900, only a few years before the first adoption of social security. Although there may have been other associations which were founded much earlier, this suggests at least tentatively that these associations had a far more limited development prior to the appearance of social security than in Europe.

<sup>64</sup> Robert J. Lampman, "The Investment of Social Security Reserves and Development Problems: The Philippines as a Case History," in *The Role of Social Security in Economic Development*, ed. Kassalow, pp. 92-93.

<sup>65</sup> The importance of social security in weakening labor movements has been suggested in Gaston V. Rimlinger, "Social Security and Industrialization: The Western Experience, with Possible Lessons for the Less Developed Nations," in Kassalow, *The Role of Social Security in Economic Development*, pp. 135 and 143 and in David Chaplin, *The Peruvian Industrial Labor Force* (Princeton: Princeton University Press, 1967), p. 78. The remarkable history of resistance to the adoption of national unemployment insurance in the United States by the American Federation of Labor also illustrates the importance of the threat posed to labor movements by social security. See Grant McConnell, *Private Power and American Democracy* (New York: Alfred A. Knopf, 1966), p. 85.

tions for the other measures of modernization.<sup>66</sup>

Within this earlier group of adopters, there is thus a moderate tendency toward diffusion *up* a hierarchy, a type of diffusion that has received scant attention in diffusion research.<sup>67</sup> Yet if diffusion research is to avoid treating modernization as involving only innovations that emanate from the most advanced centers to the rest of the world, patterns of diffusion such as this must be given close attention.

How can this pattern of diffusion be explained? Rimlinger has argued that a principal variable affecting the timing of adoption of social security among early adopters was the extent of resistance to welfare programs due to the development of liberal ideology that stressed the importance of self-reliance on the part of the poor.<sup>68</sup> If one chooses to emphasize power relationships rather than ideology, a parallel argument can be made in terms of the extent to which industrialization was led by a capitalist entrepreneurial class which opposed state intervention in welfare, as opposed to an entrepreneurial state eager to win the political loyalty of the working class. The weaker development of liberalism and capitalism in countries such as Germany, Austria, and Hungary meant that there was less resistance to overcome, and that adoption came earlier in these nations. In terms of the idea of the co-optive use of social security referred to earlier, it may be argued—particularly with reference to Germany—that there was less resistance to using social security to secure the loyalty of the working class.

Two major explanations might be suggested in interpreting these relationships. On the one hand it might be argued that because an independent bourgeoisie played a larger role in industrialization in the earlier industrializers of Europe, the strength of liberal ideology (and capitalist practice) was therefore greater in these countries.<sup>69</sup> Alternatively, it might be argued that such *pre-modern* characteristics as the strong tradition of

a centralized state and of state paternalism in Central and Eastern Europe caused both later economic growth and earlier adoption of social security. It is beyond the scope of this research to test these alternative explanations. The crucial point is that regardless of which is correct, they both provide a rationale for the inverse relationship between the timing of economic growth and the timing of social security adoption among these countries.

#### Diffusion in the Middle Group of Adopters

Turning now to the cases of adoption in 1908 or later (there were no adoptions between 1901 and 1908), it can be seen in the upper part of Figure 1 that the general tendency toward adoption at lower levels of modernization among later adopters is present among these countries, though the correlation is somewhat weaker (.49) than among all 59 countries. A careful examination of Figure 1 suggests a partial explanation: instead of a consistent pattern of adoption by successively less modern nations, one finds that during the fifteen years after 1908, social security was adopted nearly simultaneously in highly advanced and relatively backward countries, and was then gradually adopted by successively less modern countries over the following several decades.

In the period from 1908 to 1922, the dominant pattern is not one of diffusion either up or down a hierarchy. The pattern appears instead to be one of spatial diffusion. Figure 2 provides a preliminary test of this hypothesis by illustrating the spatial distribution of adoption broken down into four time periods: 1883 to 1891; 1892 to 1901; 1908 to 1922; and 1923 and later.<sup>70</sup> The cases of adoption in 1901 and earlier are subdivided in order to illustrate more clearly the pattern of spatial diffusion within Europe. In examining this map, one should remember from the earlier discussion that while spatial diffusion may operate along simple lines of proximity, the relevant dimension of closeness may involve not only physical proximity, but proximity along dominant lines of communication.

Figure 2 shows that in addition to involving diffusion up a hierarchy, the pattern of adoption in Europe up to 1901 was also spatially ordered. The earlier adopters in this period were closer to the center of innovation—Germany—and the later adopters within this time period were in Western Europe. This spatial ordering is consistent with the earlier findings, and probably emerged because the timing of economic growth

<sup>66</sup> The correlations for work force in industry and real income for these countries are .34 and .22 respectively, with slight increases if Germany and the United Kingdom are removed.

<sup>67</sup> A close analogue to this pattern of diffusion may be found in social psychological discussions of marginal individuals as innovators. See Everett M. Rogers, *Diffusion of Innovations* (New York: The Free Press, 1962), chapter 7. In "The Diffusion of Innovations . . .," p. 883, Walker notes that Mississippi was the first state in the United States to adopt a general sales tax. This is not surprising, in light of the fact that this is a regressive tax. However, Walker does not report the pattern of adoption for this innovation following its introduction in Mississippi.

<sup>68</sup> Rimlinger, *Welfare Policy and Industrialization*, pp. 35ff and 93–98.

<sup>69</sup> See the Gerschenkron and Dahrendorf references cited in footnote 62 above.

<sup>70</sup> Berry and Neils, "Location, Size, and Shape of Cities as Influenced by Environmental Factors," p. 298, have used a somewhat similar mapping procedure to illustrate the spatial diffusion of street cars in the United States.

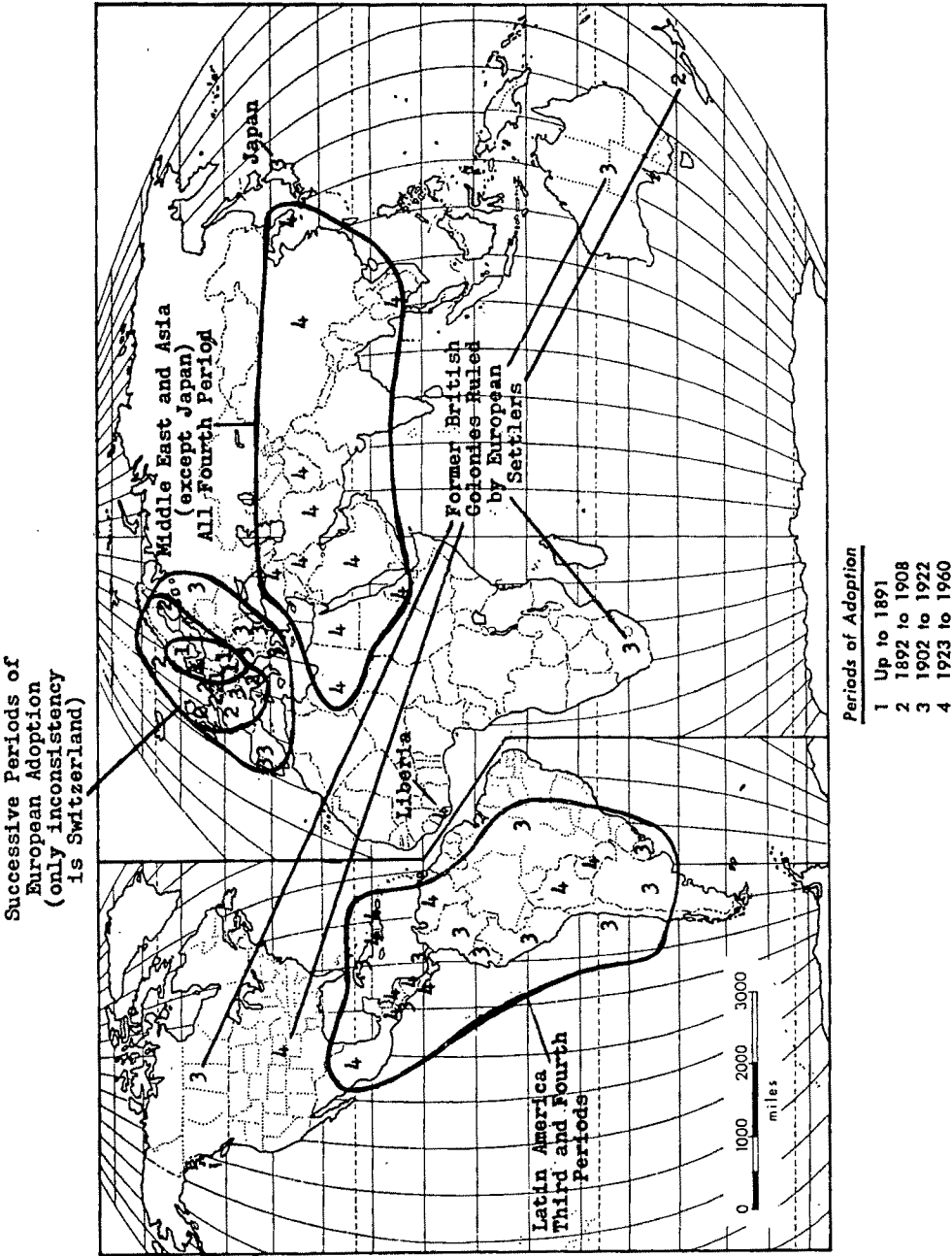


Figure 2. Timing and Spatial Distribution of Adoption

and the distribution of liberalism that emerged more strongly among the early modernizers also allowed a regular spatial pattern.

An understanding of the pattern of adoption for the period 1908 to 1922 requires an examination of both the levels of modernization at adoption and the spatial distribution of adoption. It can be seen in Figure 1 that several of the countries that adopted in this period did so at relatively high levels of modernization—following a pattern that might be considered to be a continuation of the pattern of adoption among successively more advanced countries from the earlier period. This included not only an additional European country—Switzerland—but also two former British Colonies—Canada and Australia—and the most advanced Latin American countries—Argentina, Chile, and Uruguay.

The surprising feature of this period is that a number of much less modern countries adopted at almost exactly the same time. These adoptions may be due to a regional effect. They included all of the remaining countries in Europe—Greece, Spain, Portugal, Rumania, Bulgaria, Yugoslavia, and Russia. These countries apparently adopted in part because of their proximity to the earlier European adopters, even though some of them were at extremely low levels of modernization at adoption. A similar regional effect is present in Latin America, where countries adopted during this period at a wide range of levels of modernization during a relatively small number of years, with no clear hierarchical ordering of adoption. The remaining adopters during this period were an additional former British colony ruled by European settlers—South Africa—and Japan, the most advanced country of Asia.

#### Diffusion among the Late Adopters

The remaining countries to be considered are those which adopted social security after 1922. These include all of the countries of the Middle East and Asia except Japan; the remaining countries of Latin America; and Liberia and the United States. The late adoption in the United States may be interpreted as an extreme case of the impact of liberal ideology (or capitalist practice) that was discussed earlier. For the rest of the countries, regional effects are obviously present in the sense that particular regions of the world are involved. An examination of the upper right-hand corner of Figure 1 suggests, however, that the underlying effect may again be diffusion, this time *down* a hierarchy of modernization. If work force in agriculture is used as a measure of modernization, the correlation between level of modernization at adoption and timing of adoption is .49 among these countries. For work force in industry and real income, it is  $-.55$  and  $-.40$ ,

respectively.<sup>71</sup> It is noteworthy that the size of these correlations has been reduced by two extreme outliers—the United States and, for the correlation with income, Libya—for which ambiguities about the appropriate scoring were resolved in a way that tended to weaken the confirmation of the hypothesis. If the opposite decisions had been made, these correlations would have been considerably higher.<sup>72</sup>

Regardless of which correlations one accepts, a clear pattern of diffusion down a hierarchy of modernization is present. It should be emphasized, however, that this apparent diffusion pattern does not preclude the possibility of a prerequisites effect. One interpretation of the timing of adoption for most of the ten countries in the upper right hand corner of Figure 1 is that in terms of the pattern of hierarchical diffusion, social security finally diffused down to them. On the other hand, most of them experienced some degree of modernization in the decades preceding their adoption, and it might alternatively be argued that there is, indeed, a minimum level below which it is unlikely for adoption to occur, and that their adoption was delayed until they reached that level. The fact that adoption has not occurred in any country with more than 91 per cent of its work force in agriculture would appear to give some support to this conclusion.

#### Conclusion

This research has sought to analyze the patterns of social security adoption among nations. An examination of the overall pattern of adoption provided substantial support for the hypothesis that hierarchical diffusion is present, with later adopters tending to adopt at far lower levels of modernization than the early adopters. This relationship was interpreted as being in part an aspect of the tendency toward a larger role of the state in later-developing countries, and in part due to

<sup>71</sup> These correlations are significant at the .01, .01, and .20 levels respectively. The correlations presented in the following paragraph are all significant at the .01 level.

<sup>72</sup> The first involves ambiguity with regard to the year of adoption by the United States, which might be scored around 1920 because of the extensive development of work injury programs at the state level by that date (see Appendix). Though the date that was used for the United States does not make much difference in the correlations presented earlier, it places the United States in the group of late adopters and has a considerable effect on the correlations within this group. The other outlier is Libya in the correlation involving real income. Libya adopted at a far higher level of income than would be expected because it was already a major exporter of oil at the time of adoption. It might be argued that real income is a particularly misleading indicator of development for Libya.

certain characteristics of social security as a policy.

Within this overall pattern, three striking sub-patterns clearly emerged. Among the earliest adopters, social security actually diffuses *up* a hierarchy of nations rather than *down* a hierarchy. In the middle group of adopters, a pattern of spatial diffusion is present in which social security is rapidly diffused among countries at widely differing levels of modernization. Finally, a combination of hierarchical diffusion and a prerequisites explanation appear to be the most satisfactory means of accounting for the pattern of adoption among the latest adopters.

Apart from contributing to an understanding of the patterns of social security development, it is hoped that this research may help to suggest two innovations that might play a useful role in comparative political research. First, it is clearly time that comparative political analysis devoted more attention to the rôle of diffusion in political change. Though a few studies have considered it, most comparative research devotes no attention to it at all, thereby neglecting an important explanatory factor and running the risk of reaching misleading conclusions regarding the importance of causal factors that exist within each unit of analysis, due to Galton's problem. A useful starting point in an attempt to give more attention to diffusion might be to identify types of research that lend themselves to a diffusion perspective. The increasing concern in recent literature with the timing of modernization and with the rate, timing, and sequence of different aspects of modernization was referred to earlier. This would appear to be one of the areas of hypotheses in which explicit attention to diffusion might yield particularly interesting results.

With regard to alternative patterns of diffusion, it has been shown that one of the most interesting phases of the diffusion of social security was one in which it tended to be adopted by progressively more advanced countries, involving a pattern of diffusion up a hierarchy. It is obvious that a great many of the modernizing changes considered in studies of development do diffuse from more advanced centers to more backward regions. Yet, particularly in the area of political innovations, some of the most interesting new aspects of development originate not at the top of a developmental hierarchy, but part way down it. This is obviously the case for the Chinese and Cuban models of economic and political development, and must surely be the case for many other political innovations as well. Students of innovation would do well to seek out these alternative patterns of diffusion along with the more conventional types of diffusion from the most advanced to the less advanced centers.

## APPENDIX

Deciding upon and collecting data for a meaningful statistical universe is a perennial problem in cross-national research. The universe studied in the present research consists of all nations listed in *Social Security Programs Throughout the World, 1971* which were autonomous with regard to domestic policy and were not socialist at the time of adoption.<sup>73</sup> Since a test of the prerequisite hypothesis would be less meaningful to the extent that adoption is imposed (as opposed to simply diffused) from outside, autonomy was required. On this basis, 65 countries were excluded which presently have social security but which were not autonomous with regard to domestic policy at the time of adoption, thereby eliminating all of Black Africa except Liberia and a number of countries in Asia and the Middle East. This criterion permitted the inclusion of certain countries—Hungary, Finland, and some Commonwealth nations—which were not formally independent at adoption, but which were autonomous with regard to domestic policy.

Countries which were socialist at the time of adoption were excluded because social security involves very different issues in socialist countries than in capitalist countries and including socialist countries might distort the analysis.<sup>74</sup> In practice, only one country proved to have been socialist at the time of adoption—Albania. It was therefore excluded from the analysis. With the exclusion of the dependent units and Albania, 59 countries remain in the universe of analysis.

The four principal variables used in the analysis are the year of the first adoption of social security, the percentage of the work force in agriculture, the percentage of the work force in industry, and real income per capita in the year of adoption. Since these variables cover three-quarters of a century, the problems of finding adequate data are obviously serious. Fortunately, however, the earlier adopters tend to be those countries for which more complete historical data are available, so that it was possible to find complete data on work force in agriculture and industry for all countries except China and Luxembourg. The data were far less complete for real income, with information having been available for only 36 of the 59 countries.

<sup>73</sup> United States Department of Health, Education, and Welfare, *Social Security Programs Throughout the World, 1971*. The only nations not reporting to the Social Security Administration were Guinea, Fiji, Kuwait, Lesotho, Maldives Islands, Nepal, North Korea, and Southern Yemen.

<sup>74</sup> This point is made by Vladimir Rys, "Some Current Problems of Social Security in the World," *Bulletin of the International Social Security Association*, 21 (1968), 432-442, at p. 439.

The source for the year of first adoption is *Social Security Programs Throughout the World, 1971*. The definition of social security used in this source for determining first adoption is paraphrased in the discussion of the development of social security in the text. Exhaustive historical research on social security development in the 59 countries considered here would doubtless raise questions about the appropriate identification of the first law in a few cases which could result in small changes in the date of first adoption. It is clear, however, that these changes would generally be so small as to have virtually no effect on the findings reported in this research.

A problem of validity may arise in connection with adoption in federal systems, however. For example, although it was not until 1936 that national social security legislation was passed in the United States, 45 states had some form of work-injury insurance by 1920. While the authors chose to use the 1936 date, it should be noted that some of the relationships presented in the research would be stronger if the 1920 date had been used. An exhaustive investigation of legislation in sub-national units in other countries with federal systems might raise similar questions about their year of first adoption.

In estimating the values for the three indicators of level of development at the time of adoption, we were fortunate to be able to rely on the work of two economists, Colin Clark and Simon Kuznets.<sup>75</sup> Though Clark and Kuznets rarely provided data for the exact year of adoption, they generally provided data at ten-year intervals, so that linear interpolation between their dates generally provided fairly reliable estimates. If Clark and Kuznets differed, we relied on Kuznets, since his work is more recent and in many cases involved an updating of Clark. When the three Kuznets publications differed, the more recent

one was again chosen. A smaller number of the estimates were based on the unpublished 1971 version of Arthur S. Banks's Cross-National Time-Series Data Archive, which was kindly supplied by Professor Banks. In a few cases in which the time series in the Banks data fell short by a few years of the year of adoption, linear extrapolations were used. For certain countries, other sources were used, generally based on census data for a year close to the year of adoption.<sup>76</sup>

Clark, Kuznets, and Banks define agriculture as including agriculture proper, fishing, forestry, trapping, and nomadic grazing. Though slight variations in the definition of industry exist even within the data presented by individual authors, it generally included not only industry itself, but also mining, construction, transportation, and communication. Because of the definition of industry used by these authors, the percentage in industry is higher than might be expected if one is accustomed to thinking in terms of strictly industrial activities. However, these other activities also lead to opportunities for worker organization and create for workers many of the same problems and needs as industrial employment. Hence, in terms of the concerns of the present analysis, the particular activities included in this definition do not pose a problem.

Real income per capita is gross national product per capita expressed in international units which correspond to the average value of dollars in the United States over the period 1925 to 1934. Most of the income data are from Clark, who includes in the definition of income the value of farm products which never enter the market and are consumed in farm households. When the data were obtained from a source other than Clark, they were converted so as to be roughly equivalent to 1930 dollars.

<sup>75</sup> Colin Clark, *The Conditions of Economic Progress* (London: Macmillan & Co., 1957); Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations II: Industrial Distribution of National Production and Labor Force," *Economic Development and Cultural Change*, supplement to Vol. V, No. 4 (July, 1957); Kuznets, *Modern Economic Growth: Rate Structure and Spread* (New Haven: Yale University Press, 1966); and Kuznets, *The Economic Growth of Nations* (Cambridge: Harvard University Press, 1971).

<sup>76</sup> Other sources include Stanley Braithwaite, "Real Income Levels in Latin America," *The Review of Income and Wealth*, Series 14, No. 2 (June, 1968), p. 146-47; Everett E. Hagan and Ali Hawrylyshin, "Analysis of World Income and Growth, 1955-1965," *Economic Development and Cultural Change*, Vol. IV, No. 1 (November, 1955); *Statesman's Yearbook*, various years; Irene B. Taeuber, *The Population of Japan* (Princeton, N.J.: Princeton University Press, 1958); and United Nations, *Demographic Yearbook*, various years.

# Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations\*

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At the height of the student movement in the United States it was being freely predicted that an unbridgeable gulf was forming between the generations. Differences over political ends and especially over the means to reach those ends reached epidemic proportions. The defensive reaction of the middle-age "establishment" seemed merely to speed the widening of the chasm.<sup>1</sup> Nor were the differences confined strictly to the ranks of committed college radicals, as the movement expanded to include a wide array of sympathetic supporters on the campuses.<sup>2</sup> Some observers also saw strong signs of generational change among working-class youth.<sup>3</sup> And at a more general level the counterculture seemed likely to diffuse into all segments of the rising generation, thereby helping create and sustain a sociopolitical cleavage between the generations.

With the softening of young voices and the alteration of the public agenda, the predictions of *politically* conflicted generations have diminished. The failure of the McGovern campaign to realize fully the youth vote potential, visibly expanded by the franchise extension, was seen by many as the swan song of the youth movement and age-based politics. What had appeared to be unresolvable conflict a few years previously, now seemed to be more like tolerable tension. If vivid differences in life styles still marked the generations, these differences did not appear to have their sequels in the political sphere.

\* The research reported on here was supported by the National Science Foundation and the Ford Foundation. We would also like to thank Greg Markus and Gina Sapiro for their participation and assistance and Philip Converse and Ronald Inglehart for their comments on an earlier version of this paper presented at the meetings of the American Political Science Association, New Orleans, 1973.

<sup>1</sup> In addition to the more spectacular evidence of generational cleavage, a good deal of survey data has been offered. Perhaps the best known, partly because of its three-part showing on CBS television, is Daniel Yankelovich, Inc., *Generations Apart: A Study of the Generation Gap Conducted for CBS News* (New York: Columbia Broadcasting System, 1969).

<sup>2</sup> For example, Milton Mankoff and Richard Flacks, "The Changing Social Base of the American Student Movement," *The Annals*, 395 (May, 1971), 54-67.

<sup>3</sup> Richard Flacks, "Strategies for Radical Social Change," *Social Policy*, 1 (March-April, 1971), 7-14.

Of course, some observers never regarded the intergenerational differences as constituting what came to be called the generation gap.<sup>4</sup> The apparent cleavage was said to be an illusion, a function of the vociferous few, something which would dissolve as the young passed through another life stage, or as a phenomenon not really very different from past illustrations of age-graded conflict. On the other hand, some observers maintain that the gap still exists, that basically different kinds of political orientations and frameworks have been adopted by the young, and that the youthful cohorts baptized into politics from the mid-1960s onward constitute a new political generation. Still others argue that there has been selective continuity and change, that in some respects the rising generation has evolved a different set of preferences and modes of behavior, but that in other respects it echoes very faithfully the generation of its parents.

Complicating even more these divergent perspectives is the possibility that older generations may be in a state of flux also. While a good deal of the social science literature—especially in the area of political socialization—holds that change is relatively rare after adolescence,<sup>5</sup> there are signs that older people were not immune to the same forces affecting the young in the recent past, and that they were not completely impervious to the lessons which the young were trying to broadcast. The more dramatic forms of youthful expression may have simply overshadowed movements which were also at work among older people.

Any attempt to sort out these conflicting views on change and continuity can profit by using longitudinal materials. It would be helpful to

<sup>4</sup> A very useful discussion of different interpretations of the generation gap, from which the following discussion borrows, is Vern L. Bengtson, "The Generation Gap: A Review and Typology of Social Psychological Perspectives," *Youth and Society*, 2 (September, 1970), 7-32.

<sup>5</sup> Two works stand out for their emphasis on change throughout life—Stanton Wheeler and Orville Brim, *Socialization After Childhood: Two Essays* (New York: Wiley, 1966); and Theodore Newcomb, Kathryn E. Roenig, Richard P. Flacks, and Donald P. Warwick, *Persistence and Change: Bennington College and Its Students after Twenty-Five Years* (New York: Wiley, 1967).



■ know what the parental and filial generations were like both before and after the onset of a radically changed political climate beginning around the middle of the past decade. At the simplest level this would help resolve the question of the absolute and relative size of the so-called generation gap at different points in time, thereby shedding light on propositions about age-based political cleavages and on propositions about the changing political character of the American citizenry. More fundamentally, however, such materials would enable us to go beyond the generation-gap formulation. In particular, we could (1) specify the domains of continuity and change; (2) detect the residues and trace elements of the historical period on each generation; (3) establish the degree to which one's stage in the life cycle prompted change and continuity; and (4) define as precisely as possible those political traits which might constitute a more or less permanent schism between the generations.

In order to gain a better understanding of these dynamic elements of political behavior, and in particular to investigate change and continuity in early and middle adulthood, we undertook a panel study of two age generations. Beginning with a representative national sample of high school seniors and their parents in 1965, we reinterviewed a large proportion of these respondents during the first part of 1973.<sup>6</sup> This study design allows us to investigate questions of continuity and change both across and within generations more adequately than is usually the case. In this paper we will present the initial results from this study by comparing the two-wave aggregate response patterns for both generations. Since this study design is unusual, and since many patterns of development could conceivably be observed, we shall begin by discussing the major models of continuity and change which we are likely to encounter, along with the interpretations appropriate to these models.

### Models of Continuity and Change

In comparing generations with each other and with themselves over time, one must be alert to four types of phenomena: continuity over time and three kinds of discontinuity arising, respectively, from generational effects, life cycle effects, and period effects.<sup>7</sup> It will be helpful to sketch in

diagrammatic terms some configurations which would describe these various processes at work among our two generations. Figure 1a, for example, depicts a near-perfect continuity model. Scarcely any difference marks the generations in 1965, and neither shows any change by 1973.

Life-cycle effects are ordinarily interpreted as movements by the young which, as they pass through time, bring them into line with the older generation (or at least to the point at which the older generation was when it was in that age bracket). This interpretation rests on the assumption that certain kinds of change are endemic to the life course. These changes stem from shifting responsibilities, opportunities, and needs which accompany the aging process. Many of these changes are held to transpire as people move through young adulthood and into the middle years. But life cycle effects may also be a function of movements among older people which would increase (or conceivably decrease) the distance between them and the young. A conventional life-cycle effects model is shown in Figure 1b, in which the filial generation moves toward the older one, and in which change in the older generation has stopped. Implicit here is the assumption that subsequent shifting by the young generation would only serve to bring it even closer to the older one.

An ideal type of generational difference model is shown in Figure 1c. Here the two stand apart in 1965 and maintain that division in 1973. Sustained cleavages of this order are what people usually have in mind when speaking of a *true* generation gap. Generation effects derive from age cohorts undergoing a shared community of experiences under roughly similar circumstances at pivotal, impressionable points (usually before adulthood) in the life cycle. Differential experiences within a generation can lead to generation units, distinctive clusters of like-minded people.<sup>8</sup>

The third major indicator of discontinuity is displayed in its ideal form in Figure 1d. Period

<sup>6</sup> The most complete report of the original study is M. Kent Jennings and Richard G. Niemi, *The Political Character of Adolescence* (Princeton: Princeton University Press, 1974).

<sup>7</sup> A comprehensive treatment of these topics is found in Matilda White Riley, Marilyn Johnson, and Anne Foner, *Aging and Society, Vol. III, A Sociology of Age Stratification* (New York: Russell Sage Foundation, 1972). See especially Chapter 2, and a shorter

version by Matilda White Riley, "Aging and Cohort Succession: Interpretations and Misinterpretations," *Public Opinion Quarterly*, 37 (Spring, 1973), 35-49.

<sup>8</sup> The classic formulation of the generational concept remains that of Karl Mannheim, "The Problem of Generations," in Mannheim, *Essays in the Sociology of Knowledge* (New York: Oxford, 1952), pp. 276-320. Herbert Hyman anticipates some current analytic problems in *Political Socialization* (New York: The Free Press, 1959), chapter 6. One of the best empirical applications of the political generation concept is David Butler and Donald Stokes, *Political Change in Britain* (New York: St. Martins, 1971). For a provocative cross-national application see Ronald Inglehart, "The Silent Revolution in Europe: Intergenerational Change in Post-Industrial Societies," *American Political Science Review*, 65 (December, 1971), 991-1017.

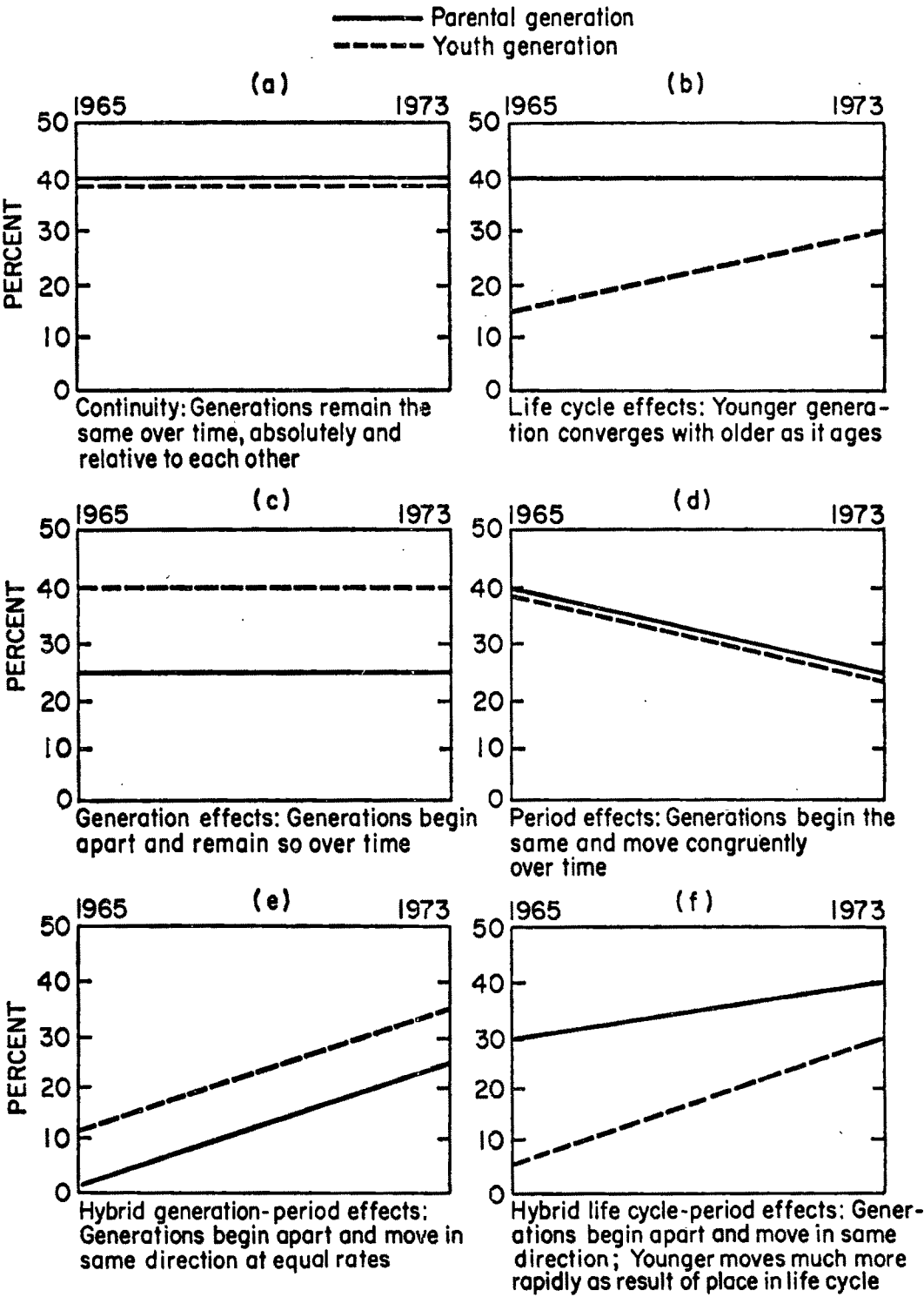


Figure 1. Models of Continuity and Change

effects work their will on each generation, reflecting the important events and trends of the time. They are often referred to as *Zeitgeist* effects. True period effects have a roughly common impact on all or most segments of society. Illustratively, certain elements of a war, an economic depression, a unique regime, a technological innovation, or mass cultural movement leave their mark on the entire society, even though other elements touch population segments in unique ways.

It would be difficult to interpret Figure 1d as illustrating anything other than period effects, given the great life-cycle and experiential differences in the two generations. Of course period effects, like the other processes, are not usually so easily identified in the real world. Rather, the three factors of age, date of birth, and historical periods often work simultaneously and in varying combinations. One very likely illustration of this is contained in Figure 1e, which depicts a hybrid, generation/period effects model. The gap between the generations in 1965 remains in 1973, but each generation has moved at a corresponding rate over time.

Another hybrid model, life cycle plus period effects, is demonstrated in Figure 1f. In this instance the generations are set apart in 1965, and both move with the times. But the younger generation, because of its still impressionable years, changes more rapidly and begins to converge with the older.

One can imagine other consequent patterns based on complex relationships among aging, generations, and periods. For example, curvilinear life-cycle development would add to the array of patterns we might expect to find, thereby adding to the difficulty of interpreting changes observed in the real world. Even some of the relatively "clean" models already presented are susceptible to alternative interpretations. One must certainly take into account substance and theory in assessing over-time patterns. Nonetheless, the processes we have outlined are among the most prominent patterns to be expected and will at least serve as points of departure.

The foregoing types of change and stability deal with aggregates rather than individuals. It is known that aggregate stability within a generation may disguise enormous individual level movement; and apparent large movement at the aggregate level may be a function of a minority undergoing sizable shifts. Moreover, the *reasons* for change and stability often lie largely untapped in an aggregate analysis. Any good understanding of political generations must ultimately be able to draw on what is occurring at the micro level.

Nevertheless, there is much to be said for a more aggregated approach also. In the final analysis political leaders and lay persons alike deal in

gross, average, grouped modes of thought. For example, while it is vitally important to know the why and how much of individual changes which have led to a marked decline in political trust over the past few years, the politically significant event is that it declined in the aggregate and especially so in some identifiable segments of the aggregate.

Furthermore, generational discontinuities are no less real simply because there are compositional factors "explaining" them. It is often speculated, for example, that differences between young and old arise because the young are much better educated. Controlling for education washes out the generation gulf. Such explanations no more reduce the age cleavage as such than do those which say that the young will become more like the old in due time. Clearly, we want to know whether the generational cleavage is temporary or permanent and whether it rests on a true difference in "communities of shared experiences" or has other bases. But in terms of some vital functions of the system the point is whether sequential generations are more like or unlike—regardless of the reasons.

#### Study Design and Panel Composition

This panel study began with a representative cross-section sample of 1669 high school seniors and their parents, interviewed in the spring of 1965. Eight years later, in the late winter and early spring of 1973, an attempt was made to reinterview all youths and one of their parents. Reinterviews were completed with 1119 out of an original pool of 1669 youths, and with 1118 of a possible 1562 parents.<sup>9</sup> Mail questionnaires were received from 230 youths and 61 parents who were inaccessible to the interviewers.

The study design allows for a multiplicity of analyses across generations and across time. For the initial presentation of the data we will simply compare the aggregate changes in each generation on some of the political orientations first tapped in 1965. Detailed comparisons between those retained in the panel versus those not retained, based on 1965 characteristics, indicate the presence of very little bias in the panel subset. For example, differences in the distributions on social and political characteristics rarely exceeded five per cent.

It is worth dwelling on the life course and historical corollaries of the two biologic generations that we have studied. First, and most obviously, there is a sharp difference in their place in the life cycle. Three-fourths of the parents were between 40 and 54 years of age in 1965 and, inexorably, were between 48 and 62 by 1973. By definition, all

<sup>9</sup> The difference between 1562 and 1669 stems from the fact that we were unable to interview a parent in 107 instances in 1965.

had a child at least 25 or 26 years of age; a majority by 1973 had seen their last child through high school. Their place in the life cycle contrasts sharply with the youth sample, all of whom were born *after* the end of World War II and represent the first installment of the post-war baby boom. Even by 1973 one-fourth of the youth sample had not been married, and among those who had children most of the children were still pre-schoolers.

Accompanying this discontinuity is the generational one. All but a handful of the parents were born before the crash of 1929, and most were old enough to remember FDR's first election. World War II touched them very personally. They began their families in the Cold War era, and they saw at least one of their children finish high school in 1965, shortly before the Great Society began to dissolve in riots and demonstrations.

The contrast offered by the political history of their offspring is well known. Most of them have only the dimmest recollection of Eisenhower's first election. Kennedy's 1960 victory is probably the first one recalled with precision. They entered high school with the civil rights movement, and about one-half left college in the midst of turmoil and discontent.

In addition to being distinguished by current age and date of birth the two generations also differ on where the life course has taken them over the eight-year period covered by our observations. Whereas the personal lives of the parents underwent relatively little alteration (one of the major ones being the leave-taking of a child) the same was scarcely true of the young adults. The period between high school graduation and the mid-twenties is obviously one of major mobility, new endeavors, and role changes.<sup>10</sup>

As noted above, apparent age-related differences can be artifacts of compositional differences. For example, rather than being derivative of "shared communities of experiences," the deviance of a generation may simply be due to a change in its composition, say by race and eth-

nicity, migration, skill level, educational attainment, or selective mortality.<sup>11</sup> At this stage, we have considered the most obvious compositional difference between the two generations, *viz.*, educational attainment. Not uncommonly, the educational disparity has been cited as the cause of the putative generation gap. Therefore, for all of the analysis to follow we have also examined the results for each of three subgroups in the junior generation: those who had no collegiate schooling after finishing high school (40 per cent); those who matriculated but did not receive a four-year degree (26 per cent); and those with at least a college degree (34 per cent). There are some absolutely large attitudinal and behavioral differences across the three groups, not only in 1973 *after* most of the educational achievements, but in 1965 as well, *before* the achievements.

For the present, however, the central point is that the three educational subgroups tended to move in tandem between 1965 and 1973. Thus regardless of whether the drift was down, up, or stable, the direction of the drift tended to be very much the same for all three. This being the case, most of what we report below concerning the young generation (in terms of direction at least) is not a function of one subgroup performing at odds with another nor of wildly disproportionate contributions from the college educated. While not completely solving the composition problem by any means, these results do tell us that when we observe what appear to be either generational, life-cycle, or period effects, we can be reasonably sure that all three educational strata are sharing in these processes.

### The Salience of Public Affairs and Politics

As will be demonstrated momentarily, the *objects* of political interest change substantially over relatively short periods of the life cycle. But what about the *level* of political interest? It has been argued that the rising generation—being highly educated and coming of age in exceptionally politicized times—has already surpassed the parental generation in its concern with political matters. Judging from previous adult data, however, overall political interest appears to grow at a moderate rate well into the middle years. (There is some controversy about whether it tails off among the very old.<sup>12</sup>) If political interest rises

<sup>10</sup> The time lapse happens to embrace what have been called the most crucial age years for creating a distinctive, self-conscious political generation. There is nothing magic in these figures, but Mannheim put the span at 17–25. Another scholar has recently built an elaborate biosocial rationale for 18–26 as the span wherein "political-cultural consciousness" takes firm hold and wherein, if the psycho-historical conditions are appropriate, a new political generation may be born. See T. Allen Lambert, "Generations and Change: Toward a Theory of Generations as a Force in Historical Process," *Youth and Society*, 4 (September, 1972), 21–46. At a more general level, Erik H. Erikson's work on identity crisis singles out late adolescence and early adulthood as a potentially important period for political character formation. See his *Identity: Youth and Crisis* (New York: Norton, 1968).

<sup>11</sup> Although this explanation seems simple enough, the reality is more complicated. For example, there is the question of whether certain compositional factors, education being a prominent one, have the same equivalency of measurement over time. This is one of the drawbacks to standardization as a statistical way of checking for compositional effects.

<sup>12</sup> Recent work indicates that the widely perceived disengagement of older people is in part an artifact of

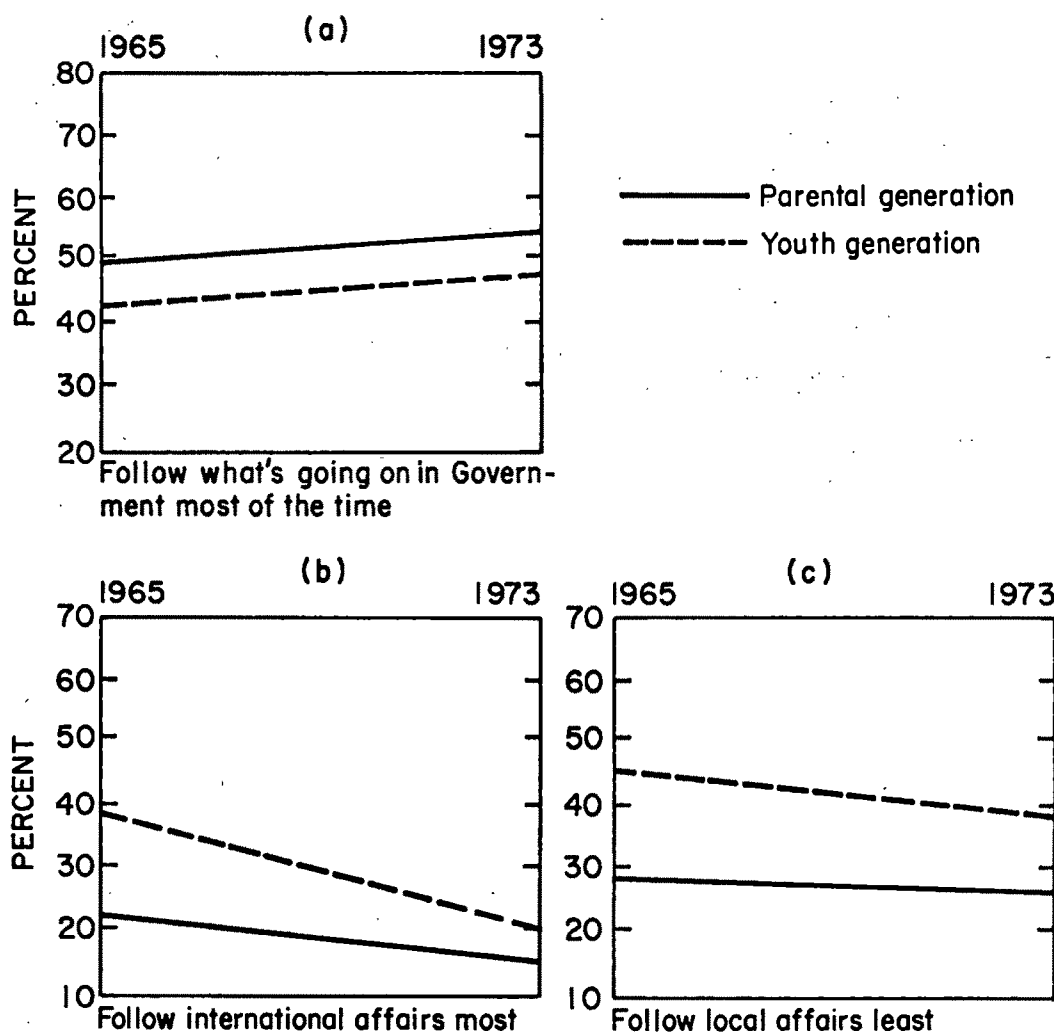


Figure 2. Salience of Public Affairs

with age, both the parent and young adult generations should have changed to reasonably similar degrees over the eight-year span covered by our study.

That this is what happened can be observed from Figure 2a. The percentages signify the proportions of respondents at each time period indicating that they follow what is going on in government "most of the time." In interpreting this development it is essential that we have been able to

draw upon previous data about life-cycle development, because the parallel lines of change could be interpreted as indicative of period effects. Here we have an instance, however, in which life-cycle effects do not occur solely among young adults, but continue throughout most if not all of adult life. Thus a pattern that might reflect period effects is more likely to be a result of normal life-cycle changes. While the young adults have nearly reached the level of interest expressed by their parents in 1965, we expect that they will surpass this level by the time they reach a comparable age because of their greater overall education.

If it is true that the general salience of public affairs changes only modestly but at a similar rate for the two generations, it is clearly not true for the salience of particular arenas of politics. We have advanced elsewhere the idea that individuals

socioeconomic composition. See Norval Glenn, "Aging, Disengagement, and Opinionation," *Public Opinion Quarterly*, 33 (Spring, 1969), 17-33; Norval Glenn and Michael Grimes, "Aging, Voting, and Political Interest," *American Sociological Review*, 33 (August, 1968), 563-75; and Sidney Verba and Norman Nie, *Participation in America* (New York: Harper and Row, 1972), chapter 9.

develop salience maps of public affairs and politics.<sup>13</sup> One such mapping operation arranges public affairs according to geopolitical domains, specifically the arenas of international affairs, national affairs, state affairs, and local affairs. The relative emphasis placed on these various levels may be conceptualized as representing one's degree of cosmopolitanism or of its mirror image, localism.

From the gross indicators used at this stage, it is obvious that the geo-political orientations of the young have changed considerably, whereas those of the middle-age have shifted only slightly. Figure 2b shows the proportions selecting international affairs as the domain to which they pay most (first-rank) attention. Since there is only slight movement among the older generation, we may rule out large-scale period effects.

Is there something peculiar about this generation which causes the precipitous drop, or is this restriction of scope a normal life course development? A generational interpretation is suspect, because this young generation has been more exposed to international stimuli, including travel, than any previous one. Rather, the press of other domains becomes the political reality with which people must deal. For a student sitting in the high-school classroom, the romance and intrigue of the international scene hold great attraction. But in the "adult world," there are more localized institutions, officials, and forms which must be dealt with; and news must be monitored which has more potential personal consequence. The pattern displayed here also fits cross-sectional data from national surveys taken in 1966 and 1968. The sizable dip thus seems to be very much a function of alteration in the life stage.<sup>14</sup>

If the young as high school seniors were heavily oriented to international affairs as their first loyalty, they were equally oriented to local affairs as their last (fourth-rank) domain of interest. Figure 2c reveals that parents held steady but that their offspring showed a modest drop in placing local affairs as their least favored domain. As young adults begin to settle into a community and feel its impact on their lives, they begin a slow gravitation toward the local arena. That the gap between the young and old still persists, though narrowed, suggests that further penetration into the life cycle will be necessary before they achieve union with the parental generation.

Now let us turn to another set of rather general

measures, this time assessing media behavior in regard to politics. Here the problem is complicated by long-term trends in media usage, as we shall see in a moment. First, judging solely from previous cross-sectional data, we would expect newspaper readership and television viewing to rise considerably among the younger adults. In contrast, use of the radio and magazines could be expected to increase little, if any, judging by the relatively small differences between parents and youths in 1965. Superimposed on these life-cycle expectations, however, are probable long-term changes in media usage. These seem to be working primarily in the direction of increasing reliance on television and decreasing reliance on radio and, to some extent, newspapers.

Data on usage of all four media are presented in Figure 3. The results are not entirely as expected. Television viewing does rise dramatically in the filial generation. But newspaper readership is almost constant over this eight year period and remains well below parental levels, even though parental usage itself has declined slightly. Altered patterns of daily time use and something as simple as having one's own television set are life-stage developments related to the sharp gain in television watching. Hence the first generation raised on television assumes its rightful place. Even within the parental generation there is a modest movement away from newspaper readership and toward television viewing, but this movement is overshadowed by the shift just discussed.

The declines in magazine readership and in radio listening are perhaps not surprising, but they suggest still further the increasing reliance being placed on television. While magazine readership was quite high among the seniors in 1965, there was still room for marginal advances to achieve parental levels at that time (taking into account the greater education of the young). But instead of gaining slightly, magazine readership dropped very noticeably among the young adults. The decrease in radio usage is perhaps less surprising but no less significant. Certainly the radio was no longer the primary source of political news even by 1965, but the level of usage was still quite high, perhaps owing in large part to captive audiences such as car drivers and to news coverage picked up as an incidental part of entertainment listening. For whatever reason, however, it is clear that minimum levels of radio usage for political news had not been reached even by 1965.

Though it seems almost contradictory that overall interest should rise while attention via at least some of the mass media declines, there may be a very simple explanation. The declines for the printed media and radio may be compensated for by the increases in television viewing. (Unfortunately, we do not know the absolute amount of

<sup>13</sup> Jennings and Niemi, *Adolescence*, Chapter 10.

<sup>14</sup> Data from 1966 are presented in Jennings and Niemi. A period effect may have accelerated the movement in the young cohort, however. Disengagement from Vietnam and the rise of severe domestic issues are secular forces which probably contributed disproportionately to the declining internationalism of the current young.

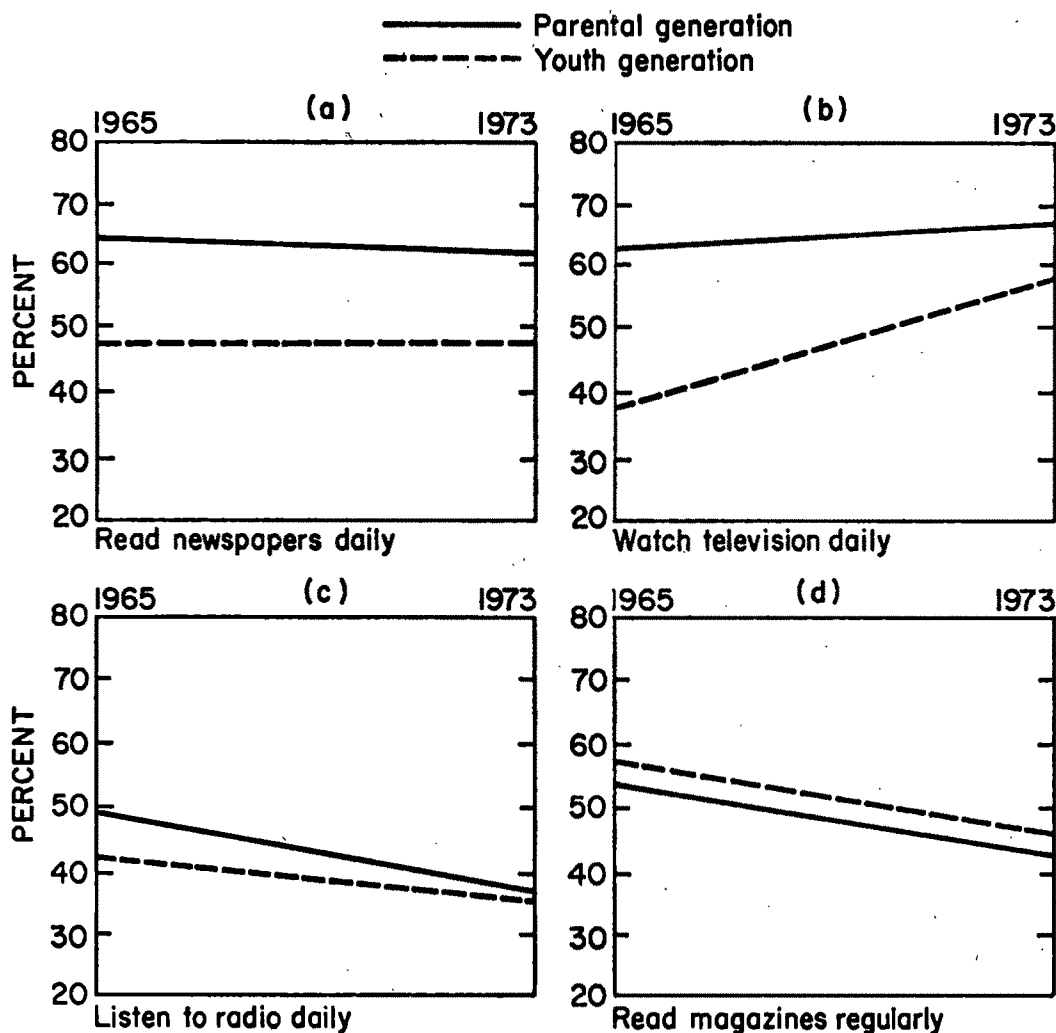


Figure 3. Following Public Affairs Through Mass Media Usage

daily time devoted by our respondents to watching news and public affairs on TV.) Thus there may be no anomaly between rising professed interest in politics and lack of increase in three of the media.

Overall, both the relative and absolute rise in the use of television to keep informed about public affairs, coupled with a fall or no gain for other media describes a strong period effect. The continued lag in newspaper usage by the younger generation also suggests a net generation effect, and the increased use of television a life cycle effect.

#### Partisanship and Electoral Behavior

Virtually all previous work on partisanship leads to one clear expectation about the eight-year period of the panel. Partisanship among the

parents should change very little if at all. Even in unsettled times, such as realigning periods, it is the young who are peculiarly susceptible to the currents of partisan sentiment flowing largely in one direction or the other. Parents, having had many years in which to develop and nurture a partisan attachment, should be highly resistant to short-term ebbs and tides in party fortunes. That this is true is borne out nicely by the data in Figure 4a, which differs from previous figures in showing the entire partisan distribution for parents in both years. The slight decline in the proportion of strong partisans may slightly contradict the life cycle processes that have been evident in earlier years. But the great similarity of the overall distributions in the two years unequivocally supports the view that partisanship among the middle-age adults is quite stable.

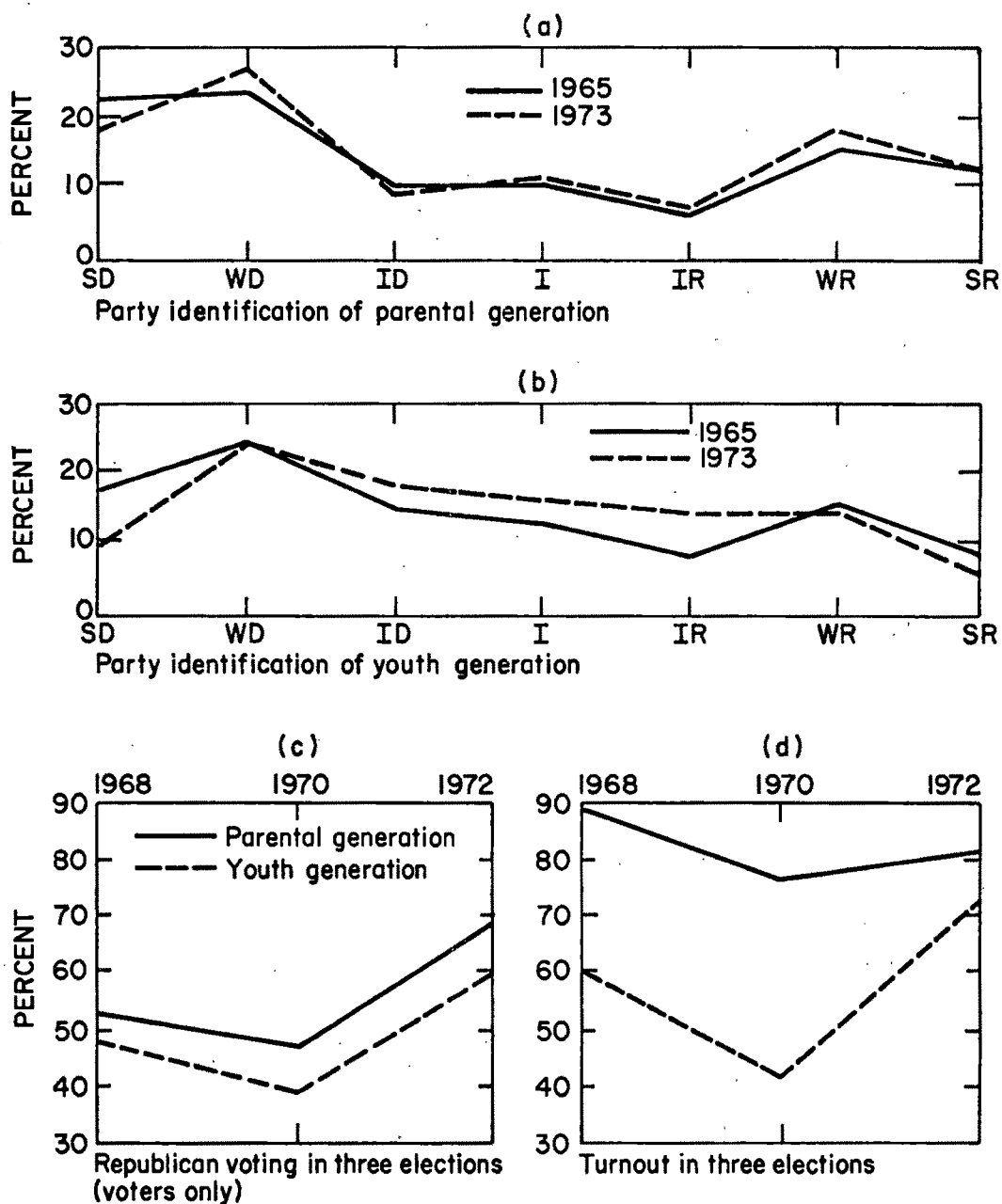


Figure 4. Partisanship and Voting, 1965-1973

More change in partisanship should be apparent among the younger generation, but exactly what changes should be expected are not altogether clear. On the one hand, the generation is maturing at a time marked by a significant rise in the proportion claiming to be free of allegiance to either of the major parties. Thus the proportion

of Independents might be expected to rise above what it was in 1965.

Working against this possibility, however, are two factors. First, these young adults are several years into the adult life cycle. All have been able to vote in at least one congressional and one presidential election, and many have been able to vote



n two presidential elections. Past studies indicate that partisan feelings begin to jell fairly quickly after the individual enters the active electorate.<sup>15</sup> If this force is operative, we would expect the proportion of Independents to have already declined slightly. Second, compared with other orientations, the transmission of party attachments across generations is carried out quite successfully except, possibly, in realignment periods. Though the question of whether the present time constitutes such a period has been repeatedly raised, opinion is far from unanimous that it is. Hence it would not be surprising if the offspring, after flirting with independence while in high school and perhaps in college, returned to the partisanship of their parents in fairly large numbers.

These two countervailing tendencies should at least keep the proportion of Independents among the young adults from rising much, if at all. With these thoughts as a backdrop, the change in the partisan distribution of the filial generation between 1965 and 1973 is startling (Figure 4b). The proportion of Independents, already very high in 1965, rose another 12 per cent, to include almost half of the sample. At the same time, the proportion of strong identifiers was cut nearly in half. Such an increase in the proportion of Independents at a time in the life cycle when we would ordinarily have expected the beginning of a long term decline, provides a compelling argument for a generation effect.

What will the future development of partisanship be for this generation? Although there may be a decline in the proportion of Independents in the future, the rate of decline probably will not bring this proportion down to levels observed in previous generations for some years, if ever. The decline of political trust which we shall observe later, along with negative implications about parties and the party system generated by disclosures through the Watergate scandal, would very likely support this feeling of independence. Moreover, the degree of independence from parties that many of these respondents have felt for several years is also likely to make them more resistant to future changes. On the other hand, sustained psychological attachment to the rather abstract concept of "independence" seems intuitively more difficult than attachment to the more concrete entity of a political party.

Generation effects also emerge when considering voting behavior. Of equal or greater importance, however, are the short-term period effects.

While the younger generation consistently voted more Democratic by a small margin (an average of 7 per cent), the movement from one year to another followed very much the same pattern in both generations. A look at the reported vote in the presidential election of 1968, the congressional election of 1970, and the presidential election of 1972 demonstrates this very nicely (Figure 4c). Thus both generations manifest signs of Nixon's hairline victory in 1968, the traditional "decline" in the off-year election, and a spectacular "surge" represented by Nixon's landslide of 1972. These parallel movements occur despite the intense Democratic attempts to woo the young in 1972 and despite the much greater proportion of Independents among the young adults. Especially given the differential composition of the two generations, Figure 4c appears to be a good example of a hybrid, generation/period effects model.

Another aspect of electoral behavior is turnout. The life cycle pattern of initial low turnout rates, which then grow rapidly and level off, has been observed so frequently that it would be surprising if it were not found in our panels. As Figure 4d shows, parental turnout—the figures for which are inflated because of the middle-aged character of the sample—is high for all three elections. The reduced turnout in 1972 compared to 1968 is consistent with the lower national balloting in the later year.

In contrast, the younger generation began its voting history with a turnout markedly below that for the elder generation. This is so even though the youth sample includes the better educated three-fourths of the total cohort (i.e., all were at least high-school graduates). Predictably, turnout decreased in the offyear election, but in an exaggerated fashion. Finally, in 1972, while the nation's overall turnout was dropping from its 1968 level, voting in the young cohort rose by 12 per cent, reflecting that cohort's increasing engagement in the political world and the development of the voting habit. By 1972 the large 1968 difference between the two generations had been reduced severely. In its voting record this young generation registers the same slow start and rapid growth that has been observed almost universally in the past. Life-cycle effects are clearly at work in this process, as they are in the much less volatile performance of the senior generation.

A final facet of partisanship to consider is the respondents' understanding of the differences between the parties. Unlike partisanship, knowledge of presumed party differences is not widespread among young children.<sup>16</sup> And while such knowl-

<sup>15</sup> Philip E. Converse, "Of Time and Partisan Stability," *Comparative Political Studies*, 2 (July, 1969), 139-71.

<sup>16</sup> Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), chapter 4; and

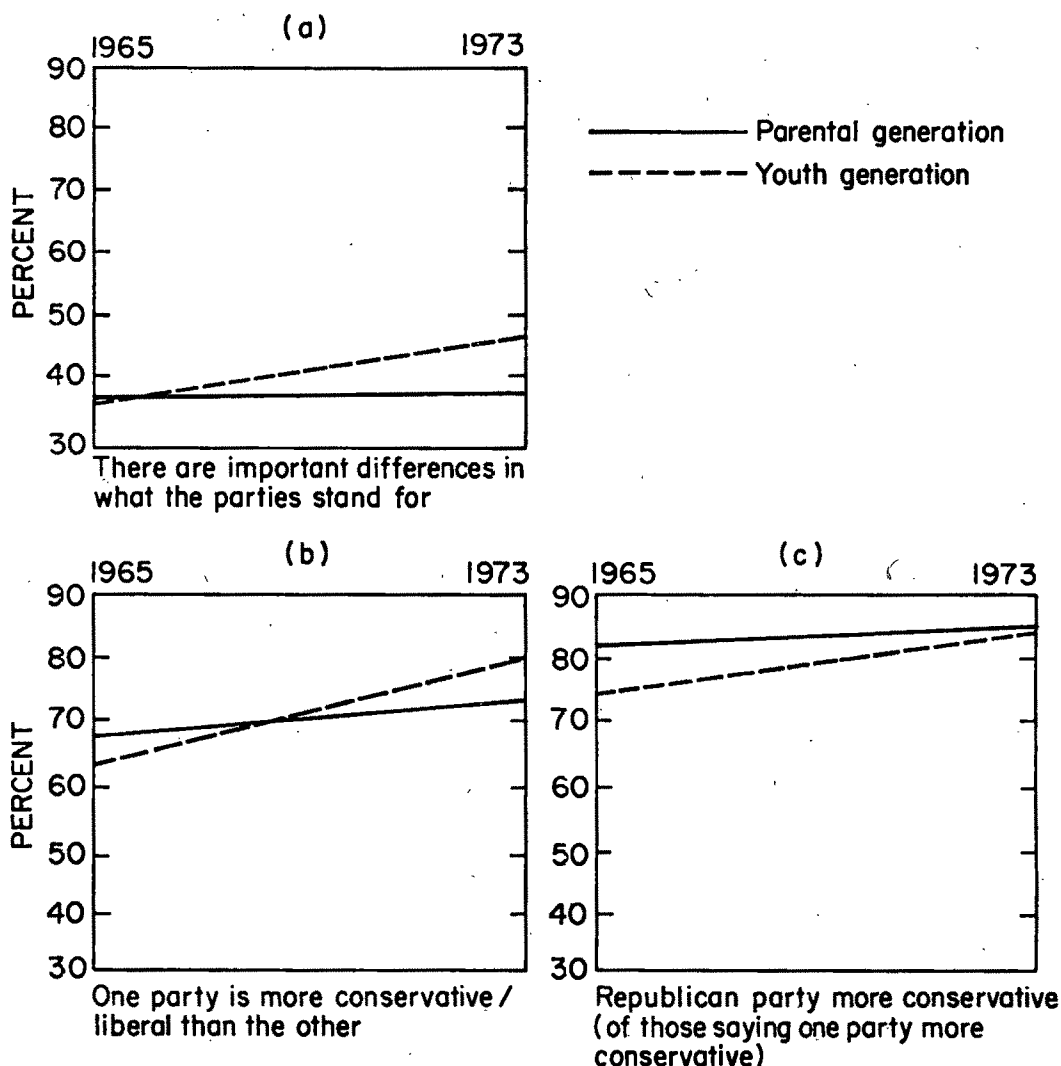


Figure 5. Perceptions of Political Parties

edge grows rapidly during the high school years, high school graduates are still slightly less knowledgeable than their parents. We would expect this knowledge to expand somewhat in the early years of adulthood. In contrast, parental views would change very little on the basis of life-cycle developments. However, images of the parties have certainly changed during the past couple of decades, and it is possible that some adult movement will be observed in the 1965 to 1973 period.

Based on responses to three questions about party differences, more change occurred among the young than among their elders (Figure 5). The

youths' understanding of political party differences rose on each of the three measures. Rather surprisingly, given its greater proportion of Independents, the filial generation in 1973 ascribed *more* differences to the parties and was at least as aware of the liberal and conservative tags as was the senior cohort.

More so than with other measures we have used, it is unclear to what extent generational effects account for the patterns observed. Our results seem to show primarily how fast young adults learned the typical views of political party differences. It is impossible to say however, how much this was due to the particular circumstances of the late '60s and early '70s. That the young not only caught up with but also surpassed in some respects their elders suggests the operation

both life cycle and generational processes at work. The complexities of information acquisition deserve more attention, and we now turn more directly to that topic.

### Political Information

Part of what guides an individual's processing and evaluation of political events is the storehouse of factual information accumulated over the years. While perhaps not as crucial as goals, values, and attitudes, an awareness of current and past political history and a comprehension of the machinery of government undoubtedly make for difference in individual political behavior. Six questions to which there were right and wrong answers were asked of the respondents at both points in time. Although these questions by no means exhaust all types of factual information, they cover a good range of history, geography, and political institutions. The workings of a variety of dynamic processes can be seen in the differential patterns based on answers to the six questions.

Parents are unlikely to have changed very much at all with regard to the questions that we have used. On the one hand, they should not really have forgotten significant historical phenomena that occurred during their lifetimes. On the other hand, for questions concerning facts and figures about governmental machinery, we presume that if parents were to forget what book learning they may have had, they had already done so by 1965. Surely the older generation would have increased its knowledge of individuals and events primarily of recent significance (such as Vietnam), but on the questions we asked very little change ought to be observed among the parents.

In contrast, young people ought to show a definite decline in one area, namely, the technical operations of government. Eight years after high school graduation fewer of the young adults are likely to answer correctly such questions, just as they have probably forgotten specific names, dates, and places from their history books. Less clearcut is what is likely to take place in the younger generation's knowledge of major historical events of the recent past. Young people obviously cannot relive these events, so that we would rarely expect dramatic increases in their knowledge of them.<sup>17</sup> They may, however, come to know more about such events as they enter the

adult world where much of the population assumes that one knows about such things and where considerable political interaction presupposes knowledge of those forces shaping the contemporary world. Thus youthful awareness of recent historical events and personalities should rise, though not very steeply.

The data for the six questions are rich in their variety and rewarding in the extent to which they fit these theoretical expectations. As can be seen in Figure 6, parents were remarkably stable on every question. A saturation point seems to be reached by the middle years. Over this eight year period the greatest change in parental response was a 3 per cent gain in the number who were able to indicate correctly the number of Supreme Court justices. Given the amount of attention devoted to the Supreme Court during this time period, so very slight a climb strongly suggests that older adults' knowledge of many features of governmental structure and processes is firmly fixed.

In contrast to the stability of parental responses is the movement in both directions observed in the young adults' answers. As expected, the declines come in their knowledge of governmental structure. We would expect some further decline in the percentage able to answer these questions in the future. The percentage will probably remain higher than for the parents, however, because of the young adults' higher education level.

Most interesting to us are the increases in young people's knowledge of historical events and current personalities. Two features are important—that there is an increase and that the rise is relatively small. Nearly three in ten youths are still ignorant of the partisan era in which their parents matured. Despite the small gain registered, there continued to be a true and substantial generational difference. Only where a political personality continues to occupy the stage (e.g., Tito) did familiarity grow by more than five to six per cent.

The development of political knowledge reveals both life cycle and generational patterns from which we can tentatively generalize beyond the specific questions asked. Information held by the new adult about forms and processes of governmental operations appears to have a deteriorating quality. Knowledge of facts and figures may rise slowly over generations as the level of education rises; but in each generation we can probably witness a cycle of expanded technical knowledge toward the end of formal schooling, coupled with the trailing off of the ability to recall this information as one leaves school. In the aggregate this drop probably occurs relatively quickly at first and then slowly as a more or less steady state is approached. Here a life-cycle pattern seems most significant in understanding the learning process.

<sup>17</sup> As E. E. Schattschneider says: "What people think about public affairs depends to a great extent on when they were born. Since life is short and history is endless, what anybody remembers of the past illuminates only a brief segment of the whole story." See his *Two Hundred Million Americans in Search of a Government* (New York: Holt, Rinehart and Winston, 1969), p. 83.

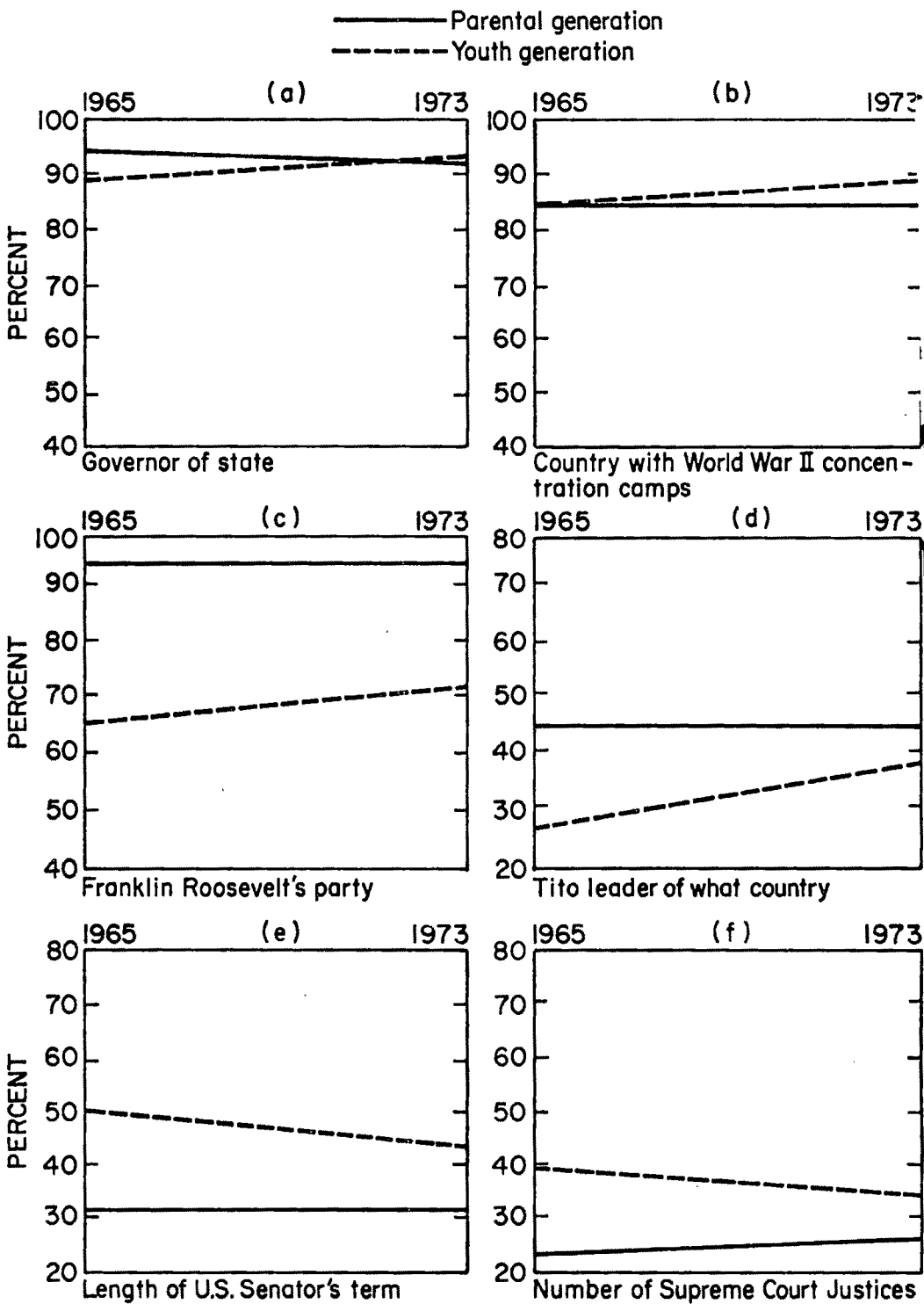


Figure 6. Factual Knowledge About Politics (Per cent Giving Correct Answer)

A sharply different picture emerges for events and personalities prominent in the present or recent past. Because of their recent entry into the adult world of politics and increasing exposure to politically-relevant media content, the rising generation improves on its awareness. On the other hand, the parental generation has reached a saturation point: those who will learn have learned. (Strictly speaking, of course, this needs individual level analysis for support.) Set off against these life cycle effects are generational effects, as exemplified by the discrepancy over the New Deal era. This type of discrepancy, and the variety of images, experiences, and emotions attached to it, is likely to stand as a permanent part of the political landscape.

### National Images

If *Zeitgeist* means anything it should mean that individuals within a political system are sensitive to and affected by the critical problems of the time. It would be absurd, for example, to think that the traumatic events of the Civil War, World Wars I and II, and the Vietnam War were not perceived by people experiencing them as tests of the nation's ability to survive either physically, morally, or both. Similarly, the various economic panics and depressions have not passed unnoticed among those experiencing them. Broad social-political ills and gains also help set off major and minor political periods. To the extent that they are recognized and experienced, these events and movements form the citizenry's image of the political periods through which they are passing. They constitute, in short, one barometer of the "spirit of the times."

We have tapped this spirit by asking our respondents what things they are least proud of as Americans. Answers to this free-response question were coded under a great number of specific categories, which in turn fall under a few general rubrics. By observing the marginal distributions of these rubrics for each generation we can catch the kinds of events and processes which struck them as constituting national negative images. These images may be taken as one indicator of a political era.

Political historians are already referring to the 1960s as the civil rights decade. Beginning with the sit-ins of the late 'fifties and early 'sixties, and culminating with riots, strong legislation, favorable court decisions, and administrative enforcement, the civil rights movement centered primarily on the struggle for Black equality. The question at hand is whether this sense of the times is shared by our two generations, and whether it is shared to the same degree. According to our data, the judgment of the political historians is matched by the images of the mass public. We have taken

the total number of responses referring to civil rights and percentaged them against the total number of responses of all kinds in answer to the question about the "least proud" aspects of being an American.<sup>18</sup> In 1965 both generations placed extraordinary emphasis on the civil rights area (Figure 7a). Responses of this type more than doubled those in other areas. And the great majority of these responses dealt specifically with white-black problems, with only a scant few being overtly hostile to the movement.

The relative and absolute focus on civil rights as of 1965 strongly suggests a period effect at full tide. But without another observation point, that developmental state would be difficult to document. By using exactly the same coding scheme on the 1973 replies (except for adding new categories) we are able to replicate the 1965 procedures. As Figure 7a shows, there was a staggering drop in the salience of civil rights on the part of both generations. The attrition was slightly sharper for the younger generation, virtually removing the modest edge it possessed in 1965. Aside from this small difference, the two generations moved almost in parallel, pointing toward a decisive *Zeitgeist* phenomenon.<sup>19</sup>

Exactly what the long-range consequences of the absorption with civil rights in the mid-sixties would be on the two generations is beyond the scope of this paper. So also is the equally significant question of the consequences of its eclipse by early 1973. It is worthwhile, however, to offer one vivid piece of fall-out from the civil rights movement which shows concretely the behavioral consequences of the historical period.

In both 1965 and 1973 our respondents were asked if they had any close friends of the opposite race. Blacks of both generations responded much more in the affirmative, and exhibited little change over time. Not so among the whites. While noticeably less than half of each generation claimed close black friends at either point in time, the trend was definitely up for each over the eight-year span (Figure 7b). Other explanations are possible, but it is difficult to account for this in anything other than period effects, flowing directly out of the civil rights movement. This explanation is all the more convincing because movement among the parents parallels (in fact, slightly exceeds) that for the youths. If it were only the "compositional" effects derivative of the younger generation being better educated, the shift should

<sup>18</sup> Similar patterns emerge using respondents rather than responses as the percentage base.

<sup>19</sup> One might challenge data of this type on the grounds that people simply report "what is in the news" at the time. To a great extent that is precisely the point, but only when that "news" is continuously repeated and reinforced by other phenomena—as was clearly the case for civil rights.

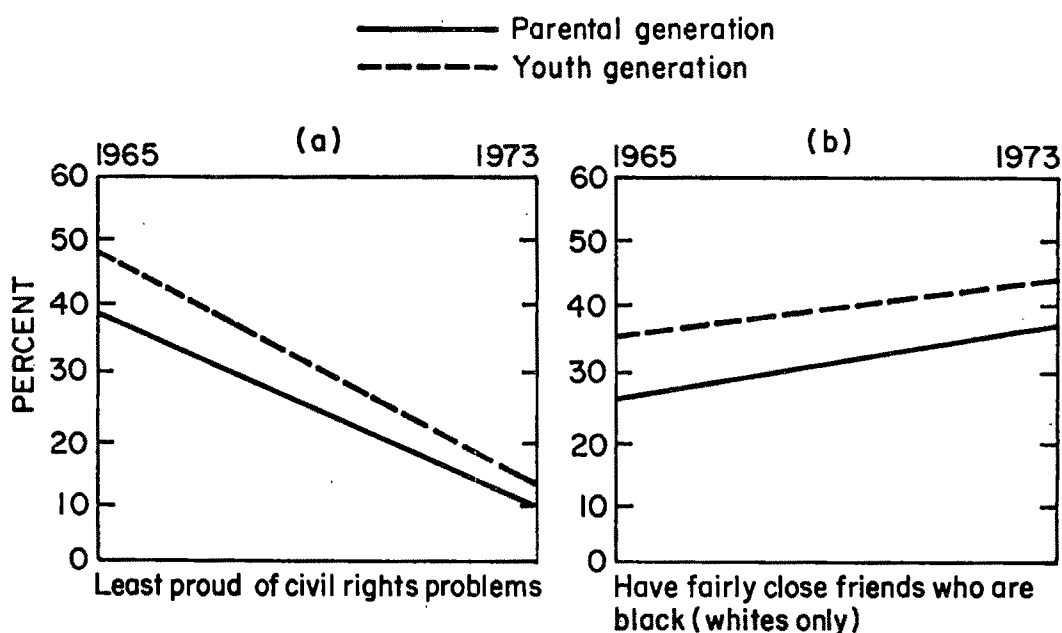


Figure 7. Salience of Civil Rights and Black-White Friendship

be less among the parents. That the filial generation still holds an edge over the older does point, however, toward a continuing generation gap in an absolute sense. Parents by 1973 had reached just about the same level the young had reached as of 1965. Overall, the pattern suggests strong period effects leavened by generational ones. The edge for the young would seem to be very much a function of being socialized in an environment more sensitive to racial injustice and strife.

Returning to the original question about negative national images as an indicator of the spirit of the times, it is significant that 1965-1973 comparisons for each generation reveal that they moved in tandem for every major substantive area. For example, perceptions of national shortcomings in the domain of moral, ethical, and religious conduct rose from 9 per cent to 18 per cent among the middle-aged, and from 5 per cent to 14 per cent among the young—the same absolute increase for each. Similarly, references to failings of the domestic political system climbed from 16 per cent to 31 per cent among the old, and from 14 per cent to 29 per cent among the young, again an absolute increase of equal magnitude. What we may be capturing in the latter category are the strong, rising sentiments that the domestic political system is failing. Because the field work was completed well before the shattering developments and spinoffs of the Watergate affair, the 15 per cent climb probably understates what will ultimately be a pronounced period effect. In this sense the increased references to failings of the

political system are of a piece with the tide of responses to the political trust items to be reported next.

#### Political Trust and Cynicism

Of all recent changes in mass public attitudes, those regarding political trust have been perhaps the most widely discussed. Cohort and cross-section analysis have shown remarkable drops in the level of trust accorded the national government.<sup>20</sup> Our unique contribution will be to trace that pattern for two distinct generations over an extended time-frame.

We have relatively well-developed theoretical expectations about the course of development of political trust. Two kinds of processes are expected jointly to structure the patterns of change for the older and younger generations. On the one hand, strong period effects are likely to be found. Trust in the government has been declining for a number of years, particularly since 1966. Thus the level of trust shown by the parent and young adult samples should decline markedly; conversely, levels of cynicism should rise.

In addition, a life-cycle change should also be observed. It has been shown repeatedly that young

<sup>20</sup> See especially Arthur H. Miller, "Political Issues and Trust in Government: 1964-1970," *American Political Science Review*, 68 (September, 1974), 951-972; and Arthur H. Miller, Thad A. Brown, and Alden S. Raine, "Social Conflict and Political Estrangement, 1958-1972" (paper presented at the 1973 Midwest Political Science Association Convention, Chicago).

children are more idealistic about political authority than older persons. This is true even of the late '60s and early '70s when the extremely positive views of earlier years were no longer so evident. By late adolescence, idealistic views decline, but in 1965 we still found the young generation to be much more positive about the government than were their parents. In the years since then we would expect their cynicism to have grown because of their encounters with the "real" world of politics, quite apart from any period effects.

Combining these two expectations, we should find growing cynicism among both generations, but a faster rate of change among the young adults.<sup>21</sup> Strong confirmation of these expectations is observed in Figure 8. The percentages indicate the proportion giving cynical replies to each of five questions about the government in Washington. In each case exceptionally strong *Zeitgeist* effects are seen in the growth of cynicism among both parents and young adults. One seldom sees in longitudinal data the precipitous slopes especially prominent in Figures 8a, 8b, and 8c.

But the young have changed more, drawing closer to, and in two cases overtaking, parental levels. The depth of cynicism that this represents can be seen by the fact that in 1973 the young adults are consistently more cynical than were their parents in 1965, usually by a moderately wide margin. In more normal times we would have expected the younger set to change by a relatively small amount, perhaps approaching the degree of cynicism held by parents in 1965. Under the impetus of events beginning in the late 60s, however, this generation was catapulted well beyond what probably occurred in most preceding cohorts.

Despite the very rapid change among the young, for three of the five questions they still remain less cynical than their parents. This suggests that there is room for yet a further increment in the cynicism of the younger population arising solely from life-cycle effects. That likelihood is also supported by data from adult cross-section samples showing change well into adulthood among individuals as well educated as our young adults. Moreover, we would speculate that having reached this degree of cynicism, the young would be unlikely to re-embrace attitudes of trust even when the political atmosphere becomes less charged. The greater than usual advance of cynicism among the young adults would then keep the level of cynicism quite high even if the young are replaced over the next

few decades by new, less cynical cohorts. We may be witnessing not only period and life-cycle effects, but also the makings of generational effects.

As political trust in the national government has declined dramatically in absolute terms, the relative place of the national government in the three-tiered U.S. system has also declined. In contrast to absolute trust, however, there are also marked age-related contrasts. These contrasts suggest the additional workings of life cycle effects.

Our evidence comes from questions about the level of government—national, state, or local—in which the respondents have the most faith and confidence and the least faith and confidence. In 1965 a majority of each generation vested more confidence in the national government, but there was a chasm between the overriding majority point of view of the younger group versus the more moderate margin among the older (Figure 9a). Based only on those figures, and making no allowance for period and life-cycle changes, one might well have predicted a continuing generational cleavage. What happened in the eight years approached landslide proportions among the young. Both generations recorded a decline, thereby fitting our presumption of period effects. But the decline was momentous among the young, bringing them into virtual congruence with their elders.

Strictly speaking it is difficult to distinguish between life-cycle and generation effects in accounting for the exaggerated movement of the filial generation. Evidence from other work suggests a similar dip at that age range among previous cohorts.<sup>22</sup> This would support a period/life-course hybrid model. On the other hand, as noted earlier, period effects often fall unequally on the generations. Because of their still impressionable years it is conceivable that the secular trend is falling especially hard on the young. Nor should we rule out the possibility that while period effects account for virtually all of the shift among the parents, a combination of period, generational, and life-cycle processes is operating on the young adults.

Although this is a good example of the complexities of trying to unravel the threads of change and continuity, of one thing we can be sure: for both generations trust in the federal government declined relative to other levels of government as well as absolutely. The main beneficiary of that decline was the local government and, to a much smaller extent, the state government. Figure 9b shows that local government gained slightly in the senior generation and very substantially in the younger. Again, there is presumptive evidence for

<sup>21</sup> The relationship between age and cynicism among adults tends to be curvilinear and somewhat inconsistent over time. Moreover, race and social class complicate the age trends. See Miller, Brown, and Raine; and Jennings and Niemi, *Adolescence*, chapter 10.

<sup>22</sup> Jennings and Niemi, chapter 10.

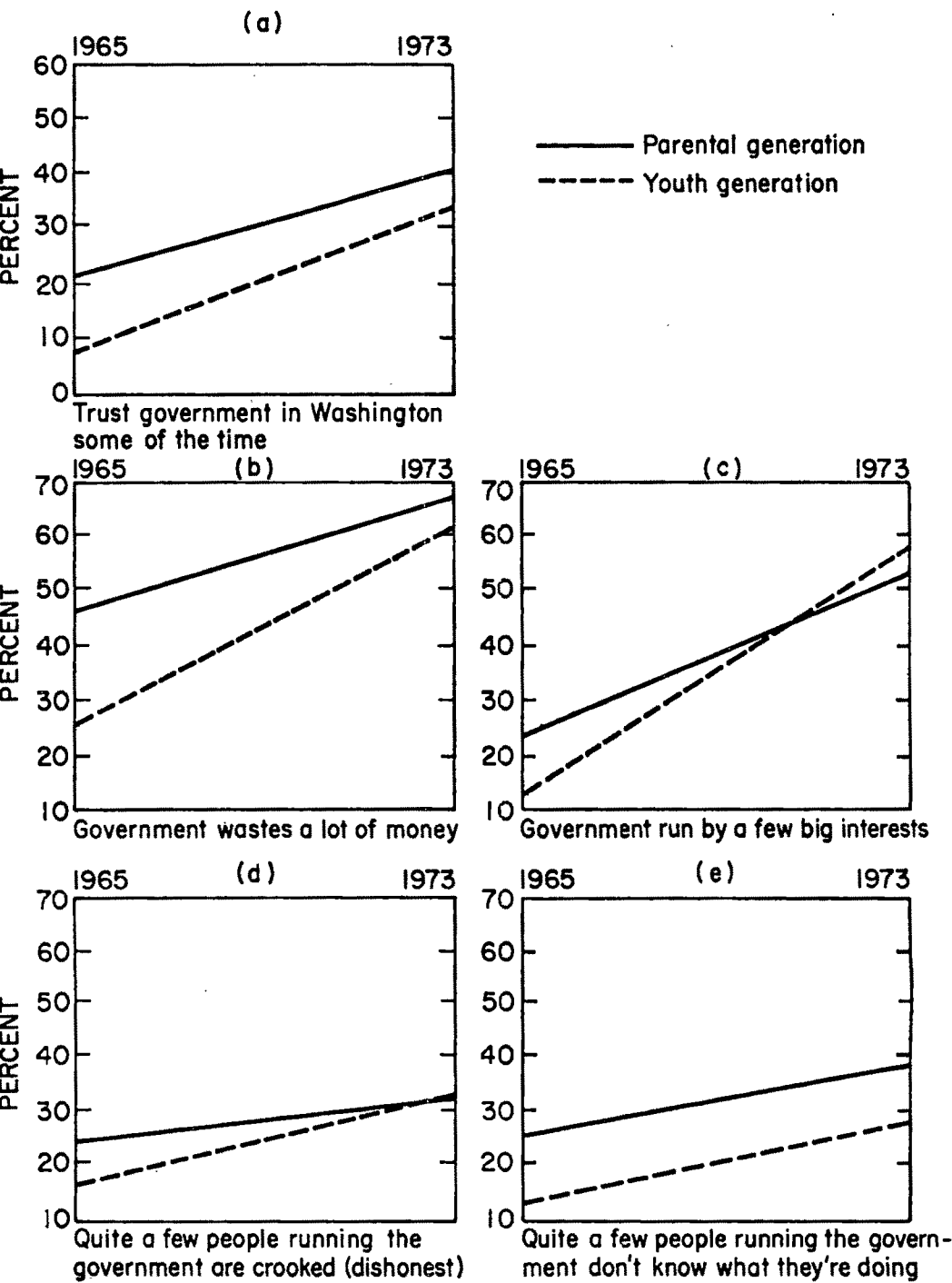


Figure 8. Political Trust and Cynicism



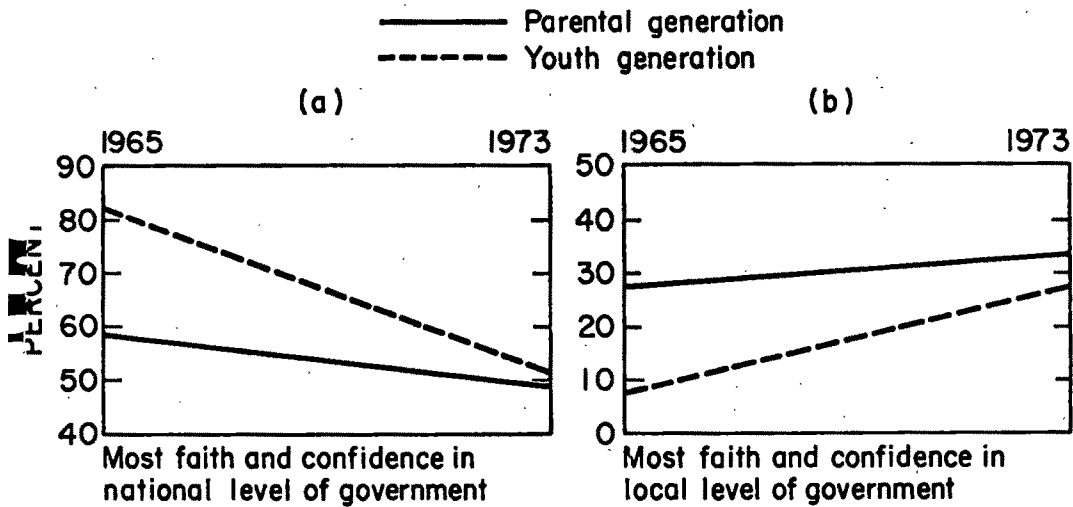


Figure 9. Faith and Confidence in Levels of Government

a secular shift affecting both, and either life-cycle and/or generational processes heavily affecting the younger. Regardless of the processes, the two generations are now in much closer proximity than they were eight years previously.

#### Opinions on Political Issues

Much of the work on age and generational conflict has centered on the diversity of views on issues of the day. Age breaks in the popular polls often reflect sizable differences between young and old. Divisions of opinion on material, economic issues have traditionally been based much more on divisions of social class than of age. But claims to prestige and recognition, differences of views on life styles, competing moral structures, conflicts over means to achieve ends, and sometimes polarization over basic changes in the political order vary often with age.<sup>23</sup>

We draw on judgments made about four issues in comparing our two generations over time. The resulting patterns illustrate very nicely the diverse nature of issue continuity and change.<sup>24</sup> Emerging as a classic example of generational cleavage is the issue of prayers in the public schools. Despite court rulings that seemed to be unfavorable to the use of prayers, this position is agreed to by only a minority within each generation at both points in time (Figure 10a). More interesting, there has been scarcely any change in either generation,

with the younger group maintaining its moderate edge over the older. Given this continuing difference, it seems best to interpret the result as generationally inspired. The older generation was socialized in a time when religious interests were clearly more prominent than now and before the courts had drawn even sharper lines on the separation of church and state. Only a marked dip later on in life would bring the junior generation into accord with the older.

Another issue involving religion and the state yields a similar pattern of movement, although the opinions expressed are considerably different from those on the prayers in school issue. Figure 10b demonstrates that support for allowing speeches against churches and religion has remained very high among both generations. And the visible gap between the two generations remains, suggesting again a more or less permanent schism. Beyond that, there is evidence of a growing division. The increasing secularization of society and perhaps the residues of the free-speech movement have pushed the younger generation almost to a point of unanimity on the issue of freedom to oppose organized religion publicly.

Although prayers in school is a continuing issue which occasionally blazes into life, the third issue at hand has burned with a sustained flame over the eight-year period. To say that the federal government's role in integrating the schools has been a volatile issue and one which has seen some shifting of sides is to state the obvious. During the mid-sixties, at what may well have been the height of the civil rights movement, opinion among our respondents was very much on the side of a positive federal role in integration. Consistent with the theme of being socialized during a more ra-

<sup>23</sup> On this point see Anne Foner, "The Polity," in Riley, Johnson, and Foner, *Aging*, pp. 115-159.

<sup>24</sup> It should be stressed that the issues for which we have longitudinal data do not include (almost by definition) issues of more recent vintage. Initial inspection of the marginals on some issues included in the 1973 wave reveals some striking differences between the generations.

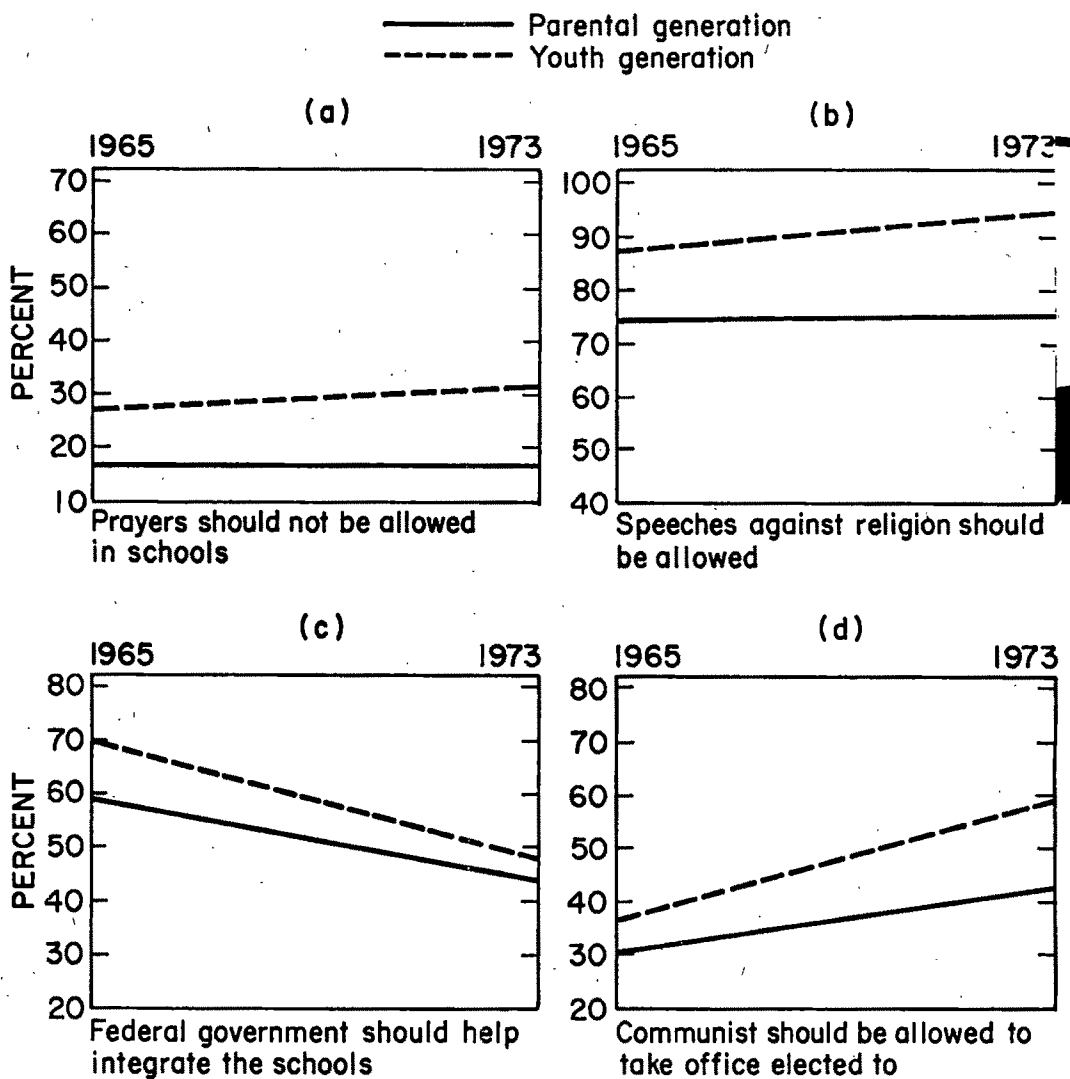


Figure 10. Opinions on Political Issues

cially liberal *Zeitgeist*, the upcoming generation was more positively disposed than the older (Figure 10c).

By 1973 a downturn had set in. Slightly under one-half of each generation favored a strong federal role, and the former edge of the young adults had come close to disappearing. So strong was the decline in youth support that by 1973 they were well below the point occupied by their elders in 1965. There are any number of reasons for the fall—the busing controversy certainly being amongst them. From the long-term perspective, however, the key point is the evidence of a strong secular pull operating on both generations. In contrast to the previous issue where essentially no period effect could be observed, this one shows it

in abundance, with perhaps an extra kicker effect on the young in particular.

While the secular trend has been away from the liberal position (*as of 1965*) on the school integration issue, it has been toward liberalism on an issue reflecting the waning years of the Cold War and anti-communism. About one-third within each generation agreed in 1965 that a Communist should be allowed to take an office rightfully won. By 1973 the figure had climbed appreciably for each generation, especially so for the younger one (Figure 10d). Since there is no plausible reason to suspect that Americans become less anti-Communist as a function of passing through middle age, period effects would seem to explain the increasing liberalism of each generation. As the

plots reveal, however, the very modest excess liberalism of the young adults in 1965 has grown to a net difference of 15 per cent. It would be difficult to construct a life-course explanation for the enlarging gap. Rather, it would seem that the era in which the young have been socialized has led them to be more receptive to the secular trends.

As is true of most of the other domains covered in this paper, no single model adequately describes the issue positioning of the two generations over time. That the youth cohort remains more liberal after an eight-year interim suggests the formation of a standing generational contrast along a liberal-conservative dimension. And thus far it is difficult to see any life-cycle effects at work. On the other hand, there are two marked instances of secular shifts overlaid on the generational differences. One (federal role in school integration) seems to be drawing the generations together, while the other (Communists taking office) separates them still further.

#### Concluding Remarks

We began this discussion by noting the controversy and ambiguity surrounding the question of conflict between the generations. It is now time to take stock of where the generations stand in relation to each other in 1973 as compared with 1965. Much additional work must be done before definitive statements can be made, but simply on the basis of what we have seen thus far a clear pattern seems to be emerging.

The flow of the two generations over time has, if anything, worked to bring them closer together now than they were eight years earlier. Only in certain issue areas and in regard to partisanship were the generations noticeably pulling apart. To the extent that differences increased, they consisted of the rising generation's having emerged with slightly to moderately more liberal political views, greater independence of partisanship, and higher Democratic voting behavior. Of the other orientations covered (and others not reported in this paper), the pattern is either one of little change over time or of visible convergence. Considering that three-fifths of our young generation attended college, that approximately one-half of the males served in the military, that the country was in an uproar during much of the eight-year period, and that open efforts were made to pit the young and the middle-aged against each other, one can only marvel that the gulf did not widen rather than narrow.

Part of the explanation lies in the sorts of indicators we have used. Rather than restricting ourselves to matters of affect and preference we have also introduced measures of participation and cognition. To the extent that the generation gap hypothesis rests purely on contrasting issue preferences, the examination of these other dimensions probably reduces the likelihood of establishing a gap phenomenon.

More fundamentally, however, other processes can be seen either drawing the generations together or, less often, pulling them apart. Clearly, life-cycle effects were working primarily—but with significant exceptions—to hold the parents on a plane while drawing their offspring toward them. We saw this most vividly in the cognitive and participative domains. Period effects in some instances, most notably regarding political trust and civil rights, prompted parallel shifts in both generations. And there were, indeed, some visible signs of lasting generational effects as in information holding and certain issues of our time. Some evidence also clearly suggests nascent generation effects which may not be fully realized for some time.

On balance the forces acting to establish convergence seem to outweigh those creating divergence. True, there are forces drawing the age generations apart, and we shall address these in another place. But what stands out as we watch these two generations over time are the strong vectors acting to bring the generations in line. These come, in substantial part, from the life-space changes (e.g., marriage, family, employment, property ownership, organizational life) accompanying the aging process among the young. Those forces, while perhaps weaker now than in the past, are still massive shapers of behavior and belief. Complementing this source of convergence are the forces of contemporary history (e.g., economic conditions, war, technology, entertainment, the arts) which touch the lives of young and middle-aged alike. If the middle-aged were unresponsive to these effects, differences between the generations would persist or increase. While malleability is higher among the young, there is graphic evidence in our materials that change occurs in the middle years also. The net result of life-space changes among the young and of historical forces operating on each generation is a smoothing out of intergenerational antagonisms, a smoothing out accomplished even over the eight turbulent years covered by our observations.

# Hobbes's Doctrine of Method\*

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The rise of modern political science is usually associated with the rise of modern natural science and scientific method. It is often noted that Hobbes was the first modern thinker to apply the new science of nature to the study of politics and, therefore, that Hobbes was the first to argue for the use of scientific method in the study of politics. It is generally agreed that Hobbes's teaching is somehow unified by his novel use of a comprehensive method. However, with respect to the two basic parts of his teaching—his doctrine of nature and his doctrine of man and society—the character of this unity has been the subject of important controversy.

According to one interpretation, it is thought that because of Hobbes's novel application of the new scientific method, his mechanistic, antimetaphysical doctrine of matter in motion is the sole foundation for his doctrine of man and politics. Implicit in this interpretation is the understanding that if Hobbes's political teaching is governed by the new scientific method, it cannot but be physicalist and a doctrine of moral relativism. According to another interpretation, it is thought that Hobbes's doctrine of man and politics must be simply independent from and prior to his materialistic, antimetaphysical doctrine of nature. Implicit in this interpretation is the understanding that since Hobbes's political teaching is moral and so neither physicalist nor relativistic, the methodological unity of Hobbes's teaching must be superficial at best.<sup>1</sup>

These interpretations obtain in spite of Hobbes's

\* The preparation of this study was aided by a Fellowship Research Grant from the Earhart Foundation. I would like to thank the Trustees of Earhart Foundation for their gracious and generous financial assistance.

<sup>1</sup> These two interpretations are not necessarily clearly stated in the Hobbes literature. However, the distinction between them is clearly at the root of the issue of the autonomy of Hobbes's theory of obligation from his empirical psychology. The two extremes are best represented by Leslie Stephens, *Hobbes* (London: Macmillan and Co., 1904), p. 73, on the one hand and A. E. Taylor, "The Ethical Doctrine of Hobbes," *Philosophy*, 13 (October, 1938), 406-24, on the other. See also M. M. Goldsmith, *Hobbes's Science of Politics* (New York: Columbia University Press, 1966), pp. 228, 242; Stuart M. Brown, Jr., "The Taylor Thesis: Some Objections," in *Hobbes Studies*, ed. K. C. Brown (Cambridge: Harvard University Press, 1965), pp. 57-71; and the literature generated by Howard Warrender, *The Political Philosophy of Hobbes: His Theory of Obligation* (Oxford: Clarendon Press, 1957). The dichotomy between these two interpretations, and the question of the "scientific" versus the "moral" nature of the doctrine of man are

apparently clear statement that the study of man as matter in motion (moral philosophy) does not "so adhere" to the study of natural right and civic duty (civil philosophy) "that they may be severed."<sup>2</sup> The persistence of the opposing interpretations must be attributed to Hobbes's insistence upon the value and necessity of the new scientific method. For although he argues that "moral philosophy" and "civil philosophy" may be severed and that the first principles of "civil philosophy" can be had by commonsensical introspection, he criticizes classical political philosophy for its unmethodical, unscientific uncertainty and indemonstrability.<sup>3</sup> According to Hobbes, the basic defect of the ancients was not a failure of commonsensical introspection, but rather a want of true method. Yet if the tradition failed for want of systematic, scientific method, and if Hobbes's political teaching is founded on common sense, it is hard to see in what the substance of Hobbes's method consists. It is not surprising that Hobbes's triumphal celebration of scientific method has engendered controversy over the unity of his teaching about man and nature.

It is my intention to suggest a new light in which to reconsider Hobbes's doctrine of method and the role it plays in the whole of his teaching about man and nature. I shall argue that Hobbes's teaching is comprehensive and unified because it is governed by the doctrine of method. The unifying role of the doctrine of method can be understood only as a function of Hobbes's intention. Hobbes believed that the theoretical reflection about politics was an essential characteristic of human nature. His intention was to reform the previously defective understanding of the relationship between this theoretical reflection and politi-

usually imposed upon Hobbes exegesis by a preinterpretive (Kantian-positivist) understanding of man and nature. Those who would seem to occupy a middle position between the two interpretations and between the question of "science" versus "morality" presuppose the distinction fashioned by the two extremes. See R. S. Peters, *Hobbes* (Baltimore: Penguin Books, 1967), pp. 43-74, 151-55, 159-62, 165-67; J. W. N. Watkins, *Hobbes's System of Ideas* (London: Hutchinson, 1965), pp. 75-81. In contrast to these errors, see Leo Strauss, *The Political Philosophy of Hobbes* (Chicago: The University of Chicago Press, 1952), pp. 1-5; *Natural Right and History* (Chicago: The University of Chicago Press, 1953), pp. 169-177.

<sup>2</sup> *The English Works of Thomas Hobbes of Malmesbury*, ed. Sir William Molesworth, 11 vols. (London: John Bohn, 1839-45), hereafter EW, I, 73.

<sup>3</sup> EW, I, 8, 73; *Leviathan*, ed. C. B. Macpherson (Baltimore: Penguin Books, 1971), Intro., pp. 81-83.

cal practice. In the light of this intention, the doctrine of method will be shown to consist in a new rhetoric that links the resolution of the human problem to the resolution of the problem of nature facilitated by the new science of nature. Illumination of the grounds of this new rhetoric will show why the doctrine of method is less than transparent. This in turn will show the degree to which Hobbes's reformation of the traditional relationship between practice and theory stands within that very tradition.

### Theory and Practice

Hobbes presents his teaching about politics as the fourth and last part of a presentation of the elements of philosophy, the parts and order of which are determined by method which "ought to be kept in all sorts of philosophy" and which determines "that such things as . . . are to be taught last cannot be demonstrated, till such as are propounded to be first treated of, be fully understood."<sup>4</sup> Hobbes insists that philosophy properly understood is a unified whole whose parts differ in name "because of the diversity of the matter about which they are conversant." These parts relate to the unified core of philosophy as the branches of a tree relate to the trunk, or as the seas relate to the whole of the ocean: "For treating of figures, it is called geometry, of motion, physick, of natural right, morals; put together they make up philosophy. Just as the British, the Atlantic, and the Indian seas, being diversely christened from the diversity of their shores, do notwithstanding all together make up the ocean."<sup>5</sup> According to the images of the tree and the ocean, the proper understanding of method ensures the unity of philosophy. With respect to practical affairs, the proper use of method will provide a "true and certain rule of our actions by which we might know whether that which we undertake be just or unjust."<sup>6</sup>

For Hobbes, method properly understood unites the study of human action and the study of all the things that are so as to abolish the traditional disharmony of theory and practice. It is this successful unity that illuminates the scope of Hobbes's self-proclaimed reformation of all prior teachings.<sup>7</sup> Hobbes's criticism of the ancients is directed not at an abstraction from the problems of political practice, but rather at a failure to re-

solve the problems of political practice. Hobbes does not criticize the ancients for their concern for the opinions of the vulgar; rather, he criticizes the tradition of political philosophy for merely mirroring the opinions of the vulgar. The reason for this defect was the "want of clear and exact method," which resulted in the persistence of "warring factions of philosophers," each with its own opinion of the human good and bad. Because the ancient philosophers mirrored the manifold opinions of the vulgar, they were unable to persuade the multitude as such to accept their manifold philosophic opinions.<sup>8</sup> Classical political philosophy was defective because of its practical inefficacy; for want of method its teachings were manifold and problematic, but diverse problematic teachings are worse than useless in practical affairs.

Since method unites the whole of philosophy so as to harmonize theory and practice, and since the defect of the ancients was want of true method, we must conclude that, for Hobbes, the practical inefficacy of the ancients was caused by their ignorance of the proper relationship between theory and practice, i.e., by their ignorance of the true unity of philosophy as such.<sup>9</sup>

In framing his criticism of the ancient tradition in terms of the problem of theory and practice or practical opinion, Hobbes agrees with the tradition that philosophy is linked to common opinion in an important way. For the ancients, man's practical concerns and speech led to his theoretical concerns and speech. The question of the good and the bad as it arose from practical concerns led to the question of the human good as such, and the question of the human good led to the question of the nature of human being as such. Insofar as the articulation of the human good was sought from the understanding of the whatness or being of man, it was ultimately dependent upon the understanding of the permanent or eternal structure of being in general, the whatness or nature of nature.<sup>10</sup>

<sup>4</sup> *De Cive*, *Ep. Ded.*, EW, II, i-vii; EW, I, 1-2, 8-10; EW, IV, 1.

<sup>5</sup> While the ancient geometers acquitted themselves well, the ancient moral philosophers failed to discharge their theoretical duties. For "were the nature of human actions as distinctly known as the nature of quantity in geometrical figures, the strength of *avarice* and *ambition*, which is sustained by the erroneous opinions of the vulgar as touching the nature of right and wrong, would presently faint and languish; and mankind should enjoy such an immortal peace. . . ." EW, II, iv.

<sup>10</sup> ἀρχικωτάτη δὲ τῶν ἐπιστημῶν, καὶ μᾶλλον ἀρχικὴ τῆς ὑπηρετούσης, ἡ γνωρίζουσα τίνος ἐνέκεν ἐστὶ πρακτικόν ἐκαστον τοῦτο δ' ἐστὶ τὰ γὰρ δὲν ἐκαστον, ὅλως δὲ τὸ ἄριστον ἐν τῇ φύσει πάσης.

Aristotle *Metaphysics* 982a19-b10; cf. *Nic. Eth.* 1094a19-27, 1096a11-1096b9, 1098a21-1098b8, 1141-

<sup>4</sup> EW, I, 87-88.

<sup>5</sup> *De Cive*, *Ep. Ded.*, EW, II, iii-iv.

<sup>6</sup> EW, I, 9.

<sup>7</sup> EW, I, ix; VII, 471; Strauss, *The Political Philosophy of Hobbes*, pp. 1-2; Goldsmith, *Hobbes's Science of Politics*, pp. 228-229; cf. Quentin Skinner, "The Ideological Context of Hobbes' Political Thought," *Historical Journal*, 9 (1966), 286-317; and Harvey C. Mansfield, Jr., "Hobbes and the Science of Indirect Government," *The American Political Science Review*, 65 (March, 1971), 97.

For the ancients, every prephilosophic opinion of the human good depended upon some understanding of those virtues which constitute a complete or whole human being. That is, the unarticulated theoretical content of common speech pointed to the whatness of human nature as the model for the perfection of human nature. Because common opinions were political and exclusive, they pointed to a perfection of the whole of human nature, and the perfection of this whole was manifested in the perfection of a part. Hence, just as the vulgar presumed natural superiority of some men, so the classical political philosophers took as thematic the problem of the natural superiority of some men over others, or the perfection of the whole in the light of the perfection of a part. Since the theoretical articulation of the structure of being as such was related to the prior, irreducible problems of common speech, the question of being was the metaphysical question of parts and wholes. The problem inherent in common opinion, the problem of how some men could be defective, incomplete, or less than men, was reflected in the metaphysical problem of intermediate wholes, or how wholes could be wholes by virtue of their participation in some larger whole.<sup>11</sup> Yet the relationship between the intelligible parts and wholes of nature remained controversial in classical metaphysics. Since the completion of practical speech depended upon the completeness of theoretical speech, the problematic nature of theoretical speech coupled with the serious urgency of practical concerns produced what Hobbes judged to be a disastrous disharmony between theory and practice. For Hobbes, the traditional ascent from practice to theory simply exacerbated the original practical problems.

When we consider the images of the tree and the seas and the criticism of the disharmony of classical philosophy, we see that Hobbes attacked neither the scope of classical thought nor its conception of the origin of theoretical speech in practice or common sense. Hobbes's teaching about method represents not a revision of a discrete teaching about man, but rather a revision of the comprehensive teaching about man and nature.

We shall see that it was the aim of Hobbes's methodological revolution to preserve the traditional ascent from practice to theory, but to do so in a way that eliminated the problematic consequences of this ascent. Insofar as method unites the parts of philosophy, the teaching about method illuminates the whole of which man is a part, and in so doing, method provides the way to a demonstrable political science, a true and certain rule of human action. Hobbes was correct to recognize the connection between traditional moral doctrines, traditional metaphysics, and common opinion, and he was doubly correct to find the locus of the political problem in the problem of natural superiority. Hobbes agreed with the ancients in two important respects. First, the question of the nature of nature springs from the prior question of the human good. Second, the question of the human good is rooted (first and foremost) in vulgar or common opinion. It should come as no surprise, therefore, that Hobbes's teaching begins with and is grounded upon a careful and comprehensive account of the sources and nature of human speech.

#### Human Nature and the Ascent From Practice to Theory

In the *Leviathan*, Hobbes presents his analysis of human speech as a part of an analysis of man's cognitive powers in general, and he discusses these powers so as to distinguish between man and the brutes. The most general classification of cognitive powers is formed by the distinction between single imaginations and "trains" of imaginations. Single imaginations are twofold. One kind, which Hobbes calls simple imaginations, represents a whole object given in sense. Hobbes calls this mode of cognition the "sense and memory of things," and he says that it is shared by man and the brutes. The other kind of single imaginations is compound imaginations, or the result of conceiving, which produces fictions of the mind such as centaurs. Unlike simple imaginations, the conceiving of compounded imaginations is "understanding" and, as such, is unique to man. For "that understanding which is peculiar to man, is the understanding not only his will; but his conceptions and thoughts, by the sequel and contexture of the names of things into Affirmations, Negations, and other forms of speech." The facility for compounding imaginations, understanding or the conceiving of "fictions of the mind," is the product of speech and is peculiar to man and denied to the brutes.<sup>12</sup>

Trains of imaginations are twofold. One kind is unregulated, where there is no "passionate

a9-20; Plato *Republic* 504b1-541b5. For the ancients the distinction between theory and practice, speech and deed, depended upon the thematic connection between them. See Joseph Owens, *The Doctrine of Being in the Aristotelian Metaphysics* (Toronto: Pontifical Institute of Medieval Studies, 1963), p. 167.

<sup>11</sup> See Plato *Republic* 514a-520d; *Meno* 70a1-4, 71b-c1, 72a6-d1, 86e1-87d8, *Apology* 37e2-38a10; Aristotle *Metaphysics* 1023b13-1024a10; *Nic. Eth.* 1097a15-1098b7, 1102a5-1102a28, 1141a9-1141b14, 1177a13-1179a33; *Politics* 1252b28-1253a1, 1253a7-19, 1261a17-25, 1274b39-1275a5, 1276b16-1277b33, 1324a23-33.

<sup>12</sup> *Leviathan*, chap. 2, pp. 89, 93-94; chap. 3, pp. 94-99; cf. EW, I, 399.

ought to govern and direct those that follow to self, as the end and scope of some desire or other passion."<sup>13</sup> Conversely, regulated trains of imaginations or thoughts are governed by some directing desire or design. As a general classification, trains of imaginations are common to both man and the brutes. That is, mental discourse, in the most general sense, is shared by man and the brutes. But regulated trains of thoughts or imaginations are also twofold. One kind is "when of an effect imagined, we seek the causes, or means that produce it: and this is common to Man and Beast." The other kind is "when imagining any thing whatsoever, we seek all the possible effects that can by it be produced; that is to say, we imagine what we can do with it, when we have it."<sup>14</sup> This distinction between regulated thought from effect to a possible cause and regulated thought from a cause to a possible effect is described by Hobbes as the distinction between mere prudence and science. The possibility of science is grounded on the possibility of speech and reason, which are "attained by industry in the apt imposing of names." Hobbes argues, then, that while prudence is common to man and beast, science is unique to man.<sup>15</sup> In the *Leviathan*, the distinctly human capacities of speech and reason, which produce conceptions or fictions of the mind and science, constitute an essential difference between man and the brutes.

In his earlier writings, Hobbes is very careful to point out that man differs essentially from the brutes because of speech and reason and that speech and reason are always manifested as the problematic articulation of the good and the bad. In the *De Homine* Hobbes argues that the speech-like communication of the animals is not, in fact, speech because "animals do not know that words are constituted by the will of man for the purpose of signification," and because any possible signification that does occur when the animals call is "forced out," not by will, but "out of the necessity of nature."<sup>16</sup> Man is different from the brutes because of reason and speech, and this difference reflects the difference between instinct and will.

Speech and reason facilitate the willful articulation of need and desire, and the nature of human reason and speech is primordially practical. In

the *De Homine* and in the early presentation of his political science in the *De Cive*, Hobbes is very explicit about the meaning of the difference between man and the brutes. There Hobbes argues that the natural capacity of human speech results in the human opining about the good and the bad. It is for this reason that human society is fundamentally different from the society of bees or ants or any other brute (mute) creature. "In those creatures living only by sense and appetite, their consent of minds is so durable, as there is no need of anything more to secure it, and by consequence to preserve peace among them, than barely their natural inclination." The brutes, devoid of speech and reason, can see no defect "in the administration of their commonwealths," nor can they "distinguish between injury and harm."<sup>17</sup> The distinction between animal instinct and rational human will consists in the human opining about the good and the bad. To be human is to be rational, and to be rational is to be the speaker of opinion. This opinion is political and problematic, however, because in seeking the good it produces disagreements, conflict, and harm. When we consider the *De Homine*, the *De Cive*, and the *Leviathan*, we see that, for Hobbes, reflection about the nature of human speech reveals the incorrigible priority of practical speech or opinion. But opinion is inherently controversial. The difference between man and the brutes is that man is the rational and political animal.<sup>18</sup>

Hobbes's analysis of man as the speaking, rational, political animal suggests an essential difference between man and the brutes. It also suggests that a necessary characteristic of this essential humanity is a moral-political problematic that is in need of solution or resolution by the natural gift of articulate reason. It suggests the need, therefore, for a resolution of the metaphysical question of the whatness of human being as the ground for the articulation of human excellence or perfection. Hobbes's analysis of man's essential nature suggests the metaphysical ascent from practice to theory. However, that work which combines the description of man as the rational, political animal with a thematic account of the scope of man's cognitive powers subverts the possibility of the metaphysical ascent from practice to theory. In the *Leviathan*, the rational power of understanding or conceiving is limited to the production of fictions of the mind, and that reason which illuminates the nature of things is identified with a narrow understanding of science as the demonstrable knowledge of cause and effect.

<sup>13</sup> *De Cive*, V, EW, II, 66-67.

<sup>14</sup> *De Homine*, X, 3, LW, II, 90-92, Gert, *Man and Citizen*, pp. 39-41; *Leviathan*, chap. 3, p. 98; chap. 4, pp. 100-102; chap. 5, p. 115-18; chap. 6.

<sup>15</sup> *Leviathan*, chap. 3, p. 95; EW, IV, 14-15.

<sup>16</sup> *Ibid.*, chap. 3, p. 96.

<sup>17</sup> *Ibid.*, chap. 3, pp. 97-99; chap. 5, pp. 110-118; EW, IV, 28-29; cf. *De Homine*, X, 1, *Thomae Hobbes, Malmesburiensis, Opera Philosophica Quae Latine Scripsit Omnia*, ed. Sir William Molesworth, 5 vols. (London: John Bohn, 1839-1845), hereafter LW, II, 89.

<sup>18</sup> *De Homine*, X, 1, LW, II, 88-89. I have used the translation in *Man and Citizen*, ed. Bernard Gert (New York: Anchor Books, 1972), pp. 37-38.

In the thematic discussion of speech and reason in the *Leviathan*, Hobbes argues that speech makes possible the "registration of the consequences of our thoughts," the signification of conceptions one to another, and the signification of desires and fears. He refines this to a fourfold list of commodities: registration of what we find to be the cause of things present or past, and what things present will produce an effect, showing others what knowledge we have attained, making known our wills for mutual help, and, finally, the pleasing or delighting of ourselves. The virtue of speech is truth, and the means to this virtue is right definition, "so that in right definition of names lies the first use of speech; which is the acquisition of science."<sup>19</sup> As he continues, however, the first use of reason becomes the only use of reason and hence the sole perfection of reason. So reason is defined as the adding and subtracting (of words or numbers) that is the "conceiving of the consequences of the names of all the parts, to the name of the whole; or from the names of the whole and one part to the name of the other part."<sup>20</sup> This reason is perfected by the "apt imposing of names" by "getting a good and orderly method in proceeding from the elements, which are names, to assertions made by connexion of one of them to another. . . ." The aim of this proceeding is to "come to a knowledge of all the consequences of names appertaining to the subject in hand; and that it is men call science."<sup>21</sup> The scope of the knowledge produced by science is, however, strictly limited to the knowledge of cause and effect. For Hobbes then argues that

whereas sense and memory are but knowledge of fact [and common to man and beast], which is a thing past and irrevocable, science is the knowledge of consequences, and dependence of one fact upon another: by which what we can presently do, we know how to do something else when we will, or the like, another time: Because when we see how any thing comes about, upon what causes, and by what manner; when the like causes come into our power we see how to produce the like effect.<sup>22</sup>

In the *Leviathan*, the scope of human reason is identified with science understood as the knowledge of cause and effect. That mode of reason which would illuminate the potentially metaphysical question of the whatness of things is said to produce fictions of the mind or, as nonfictional, is dismissed as "nothing else but sense and memory," and, as such, is no part of the uniquely human capacity of reason.<sup>23</sup> The *Leviathan* admits the practical origins of human reason and then

denies to reason the self-reflective capacity to resolve or account for the problems that are equiprimordial with the practical origins of reason. In the *Leviathan*, the ends and perfection of reason are based upon abstraction from the origins of speech and reason. Hence the question of the whatness of things, which, by means of the metaphysical ascent from practice to theory, would account for the problems illuminated by practical speech, is assimilated to the question of the "how" or material causes of things revealed by the knowledge of cause and effect.

Although the *Leviathan* suggests a teaching about the essential nature or being of man, it subverts the speech necessary for the complete articulation of human nature or being. The account of reason and speech in the *Leviathan* supports the interpretation that construes Hobbes's doctrine of method as identical with his mechanistic, materialistic science of nature, which admits as the only intelligible question the "how" of cause and effect. In the *Leviathan*, method is presented as the beginning of discourse with proper definition or the apt imposition of names for the purpose of seeking knowledge of cause and effect. It is on the basis of this understanding of method that, in the *Leviathan*, Hobbes presents a debunking attack on the "absurdity" of scholastic or traditional metaphysics.<sup>24</sup>

But there is something curiously weak about Hobbes's attack on traditional metaphysics. If method is the beginning of discourse with agreed-upon definitions, then surely the practitioners of traditional metaphysics were proper users of method. In fact, it is obvious that Hobbes's criticism is directed not at the failure to begin with definitions, but rather at the failure to begin with certain *specific* definitions, those of body and accident.<sup>25</sup> But the discussion of speech and reason in the *Leviathan* provides no thematic account of proper and defective definitions, or, specifically, of the proper and defective notions of body and accident. The *Leviathan* provides no account of why the perfection of reason must be identified with the perfection of science as the knowledge of cause and effect. The account of speech, reason, and method in the *Leviathan* is too weak to ground an attack upon traditional metaphysics and, therefore, shows a disproportion between the teaching of the *Leviathan* and its rhetorical or polemical content.<sup>26</sup> Moreover, with respect to the founda-

<sup>19</sup> *Ibid.*, chap. 1, pp. 86-87; chap. 4, pp. 107-108; chap. 5, pp. 112-115; chap. 8, pp. 146-147.

<sup>20</sup> *Ibid.*, chap. 5, pp. 114-115; see EW, I, 55-62.

<sup>21</sup> In chapter 34 of the *Leviathan*, Hobbes provides no thematic discussion of body and thus no defense or discussion of why "substance" is identical to "body" or why the universe is simply "the Aggregate of all Bodies." Hobbes appeals to the "general Acceptation"

<sup>19</sup> *Leviathan*, chap. 4, pp. 101-102, 104-106.

<sup>20</sup> *Ibid.*, chap. 5, p. 110.

<sup>21</sup> *Ibid.*, chap. 5, p. 115.

<sup>22</sup> *Ibid.*

<sup>23</sup> *Ibid.*, chap. 7, p. 131; chap. 9, pp. 147-148.



sions of a methodological political science, there is no account of the relationship between the definitions of proper method and the maxims of introspective common sense. It would seem, however, that the explication of this relationship is crucial to the very possibility of a new demonstrable political science. In sum, the *Leviathan* provides little or no information about the method which accounts for the superiority of Hobbes's science of politics over all prior teachings. The *Leviathan*, however, does not present the only Hobbesian account of reason, speech, and method. Whereas the *Leviathan* appears to debunk the rational and metaphysical question of the whatness of things, the account of method in the *De Corpore* depends upon the distinction between the questions of the "how" and the "what" and maintains the integrity of both. There is a rhetorical veil over the positive teaching of the *Leviathan*, and this veil is not immediately apparent.

### The Doctrine of Method

Hobbes begins the *De Corpore* with a discussion of the nature of philosophy, reason, and method. Philosophy "is such knowledge of effects or appearances, as we acquire by true ratiocination from the knowledge we have first of their causes or generation: And again, of such causes or generations as may be from knowing first their effects."<sup>27</sup> Hobbes follows this definition with a remark about the distinction between sense and memory, on the one hand, and philosophy on the other. Sense and memory are not to be thought of as philosophy because they are not "gotten by ratiocination," that is, they are not the product of regulated thoughts and words or speech. In an echo of the *Leviathan*, Hobbes debunks the cognitive status of prudence, which, being but memory, "is not to be esteemed philosophy." In the *De Corpore*, however, Hobbes uses the term "philosophy" rather than "science" for the ratiocinative knowledge of cause and effect. "Science" (*scientia*) is used for the more general notion of knowledge as such. Also, whereas sense and memory are described as *cognitiones* simply, *philosophia* is described as *cognitiones* governed by

of the word *body* in order to refute the fabulous religious dogma of incorporeal substance; there is no serious consideration of the problems of Aristotle's *Metaphysics*. In chapter 46 Hobbes clearly states his rhetorical intention. He attacks the "school Divinity," which transforms Aristotle's *Metaphysics* into "Books of supernatural philosophy," in order to prevent men from using its "vain philosophy" as the ground for appeal from government and obedience. It is rarely noted that in this chapter Hobbes suggests a difference between Aristotle, who feared the fate of Socrates, and the fabulous "Aristotelity" of the schools. *Leviathan*, chap. 34, pp. 428–429; chap. 46, pp. 688–689, 691–692. See pp. 1348–1349 and nn. 56–60, *infra*.

<sup>27</sup> EW, I, 3; cf. EW, I, 387.

*ratiocinatio*.<sup>28</sup> In order to understand the difference between "philosophy" and prudence or mere sense and memory, it is necessary to understand the nature of human reason or ratiocination.

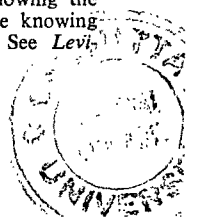
The discussion of ratiocination in the *De Corpore* reveals not simply a twofold cognitive distinction between prudence and philosophy, but a threefold cognitive distinction between prudence, ratiocinative knowledge, and philosophy as a part or kind of ratiocinative knowledge. The analysis of ratiocination in the *De Corpore* explicates the uniquely human capacity of conceiving or understanding and shows it to be linked to the phenomenon of human will. But unlike the *Leviathan*, the *De Corpore* does not limit the scope of reason or intelligibility to fictive imagination and the knowledge of cause and effect. The distinction between "*scientia*" and the specific term "philosophy" is introduced to expand the range of rational cognitive activity beyond the narrow limits of the *Leviathan*.<sup>29</sup>

Hobbes describes ratiocination as the conceptual computation or the collecting of the sum of many things added together or subtracted. Things so added or subtracted are things considered, computed, reasoned, or reckoned. Hobbes gives a revealing example of such ratiocination when he describes a man who sees something from afar and has the idea of "body." Coming closer, the new idea of "animated" is added to "body," and then, in similar fashion, the idea "rational" is added to those of "body" and "animated." Finally, the man looking "fully and distinctly" upon the object, he "conceives all that he has seen as one thing." "*Denique quando totam rem ut unam plene jam et distincte visam concipit. . .*"<sup>30</sup> The last idea is compounded of the former ideas, "which are put together in the mind in the same order in which these three single names, body, animated, rational, are in speech compounded into this one name, body-animated-rational, or man." The function of ratiocination is the articulation of the properties of things, which are the

<sup>28</sup> LW, I, 2–3. In describing the end of knowledge, Hobbes remarks, "*Scientia propter potentiam; Theorema (quod apud Geometras proprietatis investigatio est) propter problemata, id est propter artem construendi; omnis denique speculatio, actionis vel operis alicujus gratia instituta est.*" LW, I, 6. Given the prior composition and publication of the *Leviathan*, this usage, consistent throughout the *De Corpore*, cannot be attributed to an "earlier stage" of Hobbes's scientific development.

<sup>29</sup> Both terms are used in the *Leviathan*. They are not differentiated, however, by an analysis of ratiocination that articulates the possibility of knowing the whatness of things, which is not simply the knowing of cause and effect or mere "prudence." See *Leviathan*, chap. 9, p. 149; chap. 46, p. 682.

<sup>30</sup> LW, I, 3–4.



facilities or powers of bodies, which make us distinguish them from one another; that is to say, conceive one body to be equal or unequal, like or unlike, as in the example above, when coming near enough to any body, we perceive the motion and going of the same, we distinguish it thereby from a tree, a column, and other fixed bodies; and so that motion or going is the property thereof, as being proper to living creatures, and a faculty by which they make us distinguish them from the bodies.<sup>31</sup>

From these remarks about ratiocination it is clear that the most general function of human reason is to provide answers to the question of what a thing is rather than to the question of its material causes or how a thing is. Sense and memory simply are not ratiocination or rational and they are, therefore, not to be confused with "philosophy" which is. But there can be ratiocination from sense and memory which is not "philosophy" or the knowledge of cause and effect, and which is the articulation of the whatness or being of particulars. All "philosophy" is reason, but not all reason is "philosophy." As such, then, reason is the whole of intelligibility of which "philosophy," or knowledge of cause and effect, is a part. Indeed, in the sequel Hobbes argues for the logical priority of the question of "what" over the question of "how."

Hobbes is concerned to delimit the scope of "philosophy" or knowledge of cause and effect. The matter of which it treats is "every body of which we can conceive any generation, and which may, by consideration thereof, compare with other bodies or which is capable of composition and resolution; that is to say, every body of whose generation or properties we can have any knowledge."<sup>32</sup> This limit may be deduced from the definition of "philosophy" whose "profession it is to search out the properties of bodies from their generation, or their generation from their properties, and therefore, where there is no generation or property, there is no philosophy." This limit excludes theology, because of God "there is nothing neither to divide nor compound, nor any generation to be conceived"; it also excludes natural and political history, which are not the products of ratiocination. The important point to note here is that the rejection of God as a subject of "philosophy" depends upon the knowledge, general and negative as it may be, of the whatness of God, His eternity, ungeneratedness, and incomprehensibility. As such, the articulation of God's nature represents the limit of our powers of philosophizing. But, as the limiting condition of "philosophy", the general, negative knowledge of God is included in the intelligible scope of ratio-

cination.<sup>33</sup> History is excluded from the scope of "philosophy" because its subject matter does not consist of ratiocinated whatnesses but is simply the product of experience or sense and memory. The exclusion of history is determined, therefore, by the intelligible distinction between rationally articulated whatnesses and the unarticulated brute facts of nature. The scope of "philosophy" is determined by ratiocination, which is itself not identical with "philosophy" or the knowledge of cause and effect, and the question of "what" is, therefore, logically prior to the causal question of "how." The question of the properties of things is not identical to or reducible to the question of material causes or generation.

The autonomy and priority of the "what" question over the "philosophic" or "how" question suggests the possibility of the metaphysical ascent from practice to theory. It suggests that the problems generated by the theoretical content of practical speech may be resolved by a theoretical account of the whatness or being of man and nature. Hobbes's remarks about the ends of knowledge are, therefore, of especial importance.

Hobbes speaks first of the end or scope of "philosophy," which "is that we may use to our benefit of effects formerly seen; or that, by application of bodies to one another, we may produce the like effects of those we conceive in our mind, as far forth as matter, strength, and industry will permit, for the commodity of human life."<sup>34</sup> He then remarks that "the end of knowledge (*scientia*) is power," and that the scope of all speculation is "the performing of some action, or thing to be done." He argues that the utility of "philosophy," especially of natural philosophy and geometry, is best understood by "reckoning up the chief commodities of which mankind is capable." Thus the "end" and "utility" of "philosophy" are the same, that is, the "commodity of human life"; for "the inward glory and triumph of mind that a man may have for the mastering of some difficult and doubtful matter, or for the discovery of some hidden truth, is not worth so much pains as the study of philosophy requires. . . ." <sup>35</sup> Insofar as the end of "philosophy" is some human good, that good is identified with the useful. Hobbes speaks of the end of knowledge or speculation,

<sup>31</sup> The articulation of whatness and hence the objects of ratiocination can include, for Hobbes, negative properties. See EW, I, 27. Unless the existence of God is simply denied, the implication is that God is corporeal and finite. Hobbes repeats Bacon's Promethean heresy. EW, I, 98-101, 117-19, 411-12; cf. Bacon, *De Sapientia. Veterum*, XI; *Essays*, XVI, XVII; "The Advancement of Learning," *Works*, ed. Spedding, Ellis, and Heath, 14 vols. (London: 1857-1874), III, 295-319. See pp. 1343-1344, *infra*.

<sup>34</sup> EW, I, 7.

<sup>35</sup> *Ibid.*

<sup>31</sup> EW, I, 3-6; LW, I, 5; cf. EW, IV, 24-27.

<sup>32</sup> EW, I, 10.

that is, of the rational whole of which "philosophy" is a part, in terms of the Baconian equation of knowledge and power or human "commodity." This end is inherent in the very structure of human reason, which, to be reason, must be regulated by some desire or need. This end of utility encompasses reason and "philosophy," as whole and part, and, therefore, the questions of "what" and "how." The articulation of whatness, or beings or properties, is bounded and determined, then, by the structure and end of human reason, which is human utility or the good for man.<sup>36</sup>

The end and scope of man's reason are co-determined by the articulation of the whatness of things and the conquest of man over nature. Thus, when men answer the question of the whatness of any thing in nature, the whatness articulated is the product of the ability of the thing to be sensed and then compounded by reason and the possibilities that make up its utile characteristics as the object of "philosophic" knowledge. For Hobbes, to be intelligible is to be the object of conceptual compounding and subtracting and to be the object of human use. Therefore, the intelligible entities illuminated by the answers to the questions of "what" and "how" must be characterized in some way by purposiveness or teleology. In the light of the scope and end of reason, which combine the questions of what and how, intelligible natural entities can be fully intelligible only in relation to the human good, which is articulated in terms of perfection and defect, or, to speak generally, wholes and parts.

We recall that, for Hobbes, theoretical speech springs from the prior question of the human good. Since all men are opiners about human excellence and hence about the whatness of man, the seeds of reason and knowledge as such are the prereflective moral opinions that constitute the problem of the public good.<sup>37</sup> Since the end of all knowledge is the human good, both the beginning and the end of reason and knowledge must be rooted in practical speech.

We recognize here the elements of the traditional ascent from practice to theory, but we see also the elements of Hobbes's reinterpretation of that ascent. The theoretical question of the whatness of things arises from practical discourse, but the being of nature is illuminated and constituted by the perfection of man's ability to complete himself through the conquest of nature by science.

<sup>36</sup> "For the Thoughts, are to the Desires, as Scouts, and Spies, to range abroad, and find the way to the things Desired: All Stediness of the minds motion, and all quickness of the same, proceeding from thence," *Leviathan*, chap. 8, p. 139.

<sup>37</sup> *De Cive*, V, EW, II, 66-67; *De Homine* X, 3, LW, II, 90-92. Man is, therefore, the speaking, rational, political animal. Cf. Aristotle *Politics* 1252b28-1253a20.

Science does not subvert the intelligibility of the question of the whatness of things; indeed the new science presupposes this question and its relation to practical discourse. Instead of debunking the very notion of final cause, Hobbes argues that "a final cause has no place but in such things as have sense and will; and this also I shall prove hereafter to be an efficient cause."<sup>38</sup> Man's study of the parts or the whole of nature is not the contemplation of the model of being, but is, rather, the study of how these parts become intelligible parts of an intelligible whole by means of human scientific power. Instead of natural teleology as the standard for morality, moral teleology is the standard of the intelligibility of nature or the whole of man and nature.

For the ancients, the teleology of nature was connected to the human good, or the teleology of man, because the proper understanding of the relationship between the parts and wholes of the intelligible beings of nature, or beings as such, provided an answer to the practical and prior question of the whatness of man and hence of the perfection of man. This ascent is metaphysical because the beings so illuminated are the eternal beings that provide stable guidance for the ephemeral, changeable speakers who seek out the whatness of man and nature. Thus both with respect to the practical content of prior common speech, and the way of metaphysical ascent itself, the metaphysical question is the question of whole and part. For the ancients, the strict resolution of the problems of practical discourse depended upon the resolution of the theoretical problem of natural, intelligible parts and wholes. Classical teleology was natural because the unstable moral context of speech was to be stabilized by the eternal teaching of theoretical speech. To the extent that natural teleology was problematic, then the understanding of the human good was problematic. In respect of the different requisites of practical and philosophic life, and in respect of the dependence of practical, political doctrine upon a problematic metaphysics of nature, the traditional ascent from practice to theory resulted in the disharmony of practice and theory.

As we have seen, Hobbes's intention was to preserve the traditional ascent from practice to theory, but to do so in a way that eliminated the disharmony of practice and theory. His intention was to replace the problematic metaphysics of nature with a putatively unproblematic conquest of man over nature, and the vehicle for this replacement was the doctrine of method. Within the

<sup>38</sup> EW, I, 131-132. So " . . . the knowledge of the essence of anything is the cause of the knowledge of the thing itself; for, if I first know that a thing is rational, I know from thence, that the same is man; but this is no other than an efficient cause."

framework of the ascent from practice to theory, the practical, yet moral-theoretical question of the whatness of man was to be answered and resolved by the metaphysics of man as the conqueror of nature, which identifies man as the locus of the intelligibility of nature.

It is the conquest of nature by human science that permits the metaphysical ascent from practice to theory. For classical metaphysics, the whatnesses or wholes of nature were morally and theoretically intelligible by virtue of their eternal and unchanging natures: hence their status as eternal standards of guidance for the ephemeral and changeable circumstances of practical life. These beings were universals because of their eternity in contrast to the noneternal particulars of which they were models. In this sense philosophy was coextensive with political philosophy, and the question of natural right was *the* philosophic question.<sup>39</sup> For Hobbes, the eternal whatness or being of nature resolves the problems of practical discourse because being is intelligible in the light of the fulfillment or completion of human needs and desires. The metaphysical relation of eternal universal to noneternal particular exists because of the possible corruptibility of any particular, and yet the eternity of intelligible whatness or being depends upon the eternal possibility of at least one particular; the eternity of the intelligible whatness of nature consists in and depends upon the eternity of the human species. The new relationship of practice and theory produces an audacious scientific humanism: the world is an intelligible, eternal whole because man is the creature who can create his own eternity. It is no wonder, then, that Hobbes criticizes Socrates for trying to bring philosophy down from the heavens.<sup>40</sup>

<sup>39</sup> So for the ancients although man was not the highest thing, the contemplation of the highest things was always mediated by the problems of the human things. See Aristotle *Nic. Eth.* 1141a21–1141b14; n. 10, *supra*.

<sup>40</sup> *De Cive*, Preface, EW, II, x. Hobbes argues that the most "profitable part of natural science" is the science of man's body. The new science of nature aims to fulfill the desire for eternal life denied man by a jealous God. For Hobbes, as for Bacon and Descartes, medicine is the real queen of the sciences. EW, I, viii; cf. Descartes, *Discours de la Méthode*, Sixième Partie, in *Oeuvres et Lettres*, ed. A. Bridoux (Paris, 1952), pp. 168–169; Bacon, *De Augmentis* IV: 3; *Novum Organum* II: 1. The fact that knowledge of the infinite is a function of human imagination and is limited by human imagination would be affected, of course, by the possible eternity of human imagination. Hobbes never argues that man is necessarily a finite or ephemeral being, but rather argues that knowledge of what is infinite can never be attained by a finite inquirer. The question of the infinite is not the question of the beginning or eternity of the world, but of the ability of man to conquer natural decay or the Baconian equation of knowledge and power. See EW, I, 7, 93–

In the light of the intended unproblematic ascent from practice to theory, the task of the doctrine of method is twofold. First, it must facilitate the articulation of nature so that nature will be intelligible *solely* in the light of human conquest. Method must harmonize the fact that, because of the natural structure of human speech and reason, nature conquered and articulated by man is theoretically and morally intelligible, with the requirement that nature conquered and articulated must not by itself be a model for human being or perfection. Since Hobbes teaches that the origin, structure, and end of speech and reason are practical, and that practical speech generates the practical-theoretical problem of parts and wholes, the doctrine of method must facilitate the illumination of nature in such a way that abstracts from the problem of parts and wholes. Nature conquered must be theoretically and morally intelligible, but it must be so in a way that tempts no attempt to secure from that intelligibility a solution to the problem of parts and wholes.

The second task of the doctrine of method is prescribed by the first. Since nature conquered must be stripped of its character as a model for human being, the *unproblematic* ascent from practice to theory depends upon the reformation of the *sources* of moral and theoretical discourse. The second task of the doctrine of method is, then, the reformation of common speech. However, because of the natural structure of speech and reason, this reformation can be accomplished only by means of a new methodological rhetoric.

Hobbes begins his thematic discussion of method with the crucial distinction between two kinds of knowledge, knowledge *that* something is and knowledge of *how* something is. He says that

in knowledge by *sense*, the *whole* object is more known, than any *part* thereof; as when we see a man, the conception or whole idea of that man is first or more known, than the particular ideas of his being figurate, animate, and rational; that is, we first see the whole man, and take notice of his being, before we observe him in those other particulars. And therefore, in any knowledge of the *ὅτι*, or *that* anything is, the beginning of our search is from the whole idea; and contrarily, in our knowledge of the *διότι*, or the causes of anything, that is, in the sciences, we have more knowledge of the causes of the parts than of the whole. For the causes of the whole [in knowledge *τοῦ διότι*] is compounded of the causes of the parts; but it is necessary that we

101, 411–412; cf. Bacon, *De Sapientia Veterum*, XI. Hobbes's science of nature does not result in a radical materialism. "First philosophy" does not exhaust the intelligible illumination of nature and does not result in a *materia prima* as the separable and ontologically real ground beneath a putative phenomenal reality. EW, I, 118. Hobbes unabashedly speaks of the accidents of "*Politique Bodies*"; surely such bodies are not merely corporeal entities. See n. 45, *infra*.

now the things that are to be compounded before we now the whole compound.<sup>41</sup>

Hobbes's terminological distinctions are a proximal guide to the interpretation of this important passage. Earlier in the *De Corpore*, sense and memory were described as unmodified *cognitiones* in contrast to "philosophy." The difference between the knowledge had by the brutes and that had by man was said to consist in ratiocination, which is *conceptual* computation and compounding. In the *De Corpore*, ratiocination as conception was *not* characterized as mere fictive imagination, and, more importantly, ratiocination proved to be first and foremost the knowledge of the whatness of things. As Hobbes introduced the term "*scientia*" to the dyad of prudence and "philosophy," so he introduced the ratiocinative knowledge of the whatness of things to the dyad of sense and memory and knowledge of cause and effect.<sup>42</sup> In the present context, the *cognitione autem sensuum*, which informs knowledge *τῷ ὄντι*, is characterized as conceiving: "ut cum videmus hominem, prius notus, seu notior est *conceptus*, sive idea illa tota hominis, quam particulares ideae figurati, animati, rationalis. . . ."<sup>43</sup> The use of *conceptus* in the explanation of knowledge *τῷ ὄντι*, in conjunction with the obvious choice of example, points to the prior discussion of ratiocination. In the light of this prior discussion, with its distinction and connection between sense and memory simply and the conceiving of the whatness of things, the discussion of knowledge *τῷ ὄντι* must be seen not as the explication of brute facticity, but as the explication of the articulation of the whatness of things.

In speaking of our knowledge *that* a thing *is*, Hobbes uses a term, *τὸ ὄντι*, that points to the traditional account of substance. Thus he also points to the traditional distinctions between first and second substance and between *τὸδε τι* (thisness) and *τί ἐστι* (whatness), and so to the way these concepts were related in the rich and complex problematic of substance. At the same time, however, Hobbes provides no comprehensive account or critique of the traditional problematic of substance. We must note the presence of a remarkably subtle and circumspect rhetoric. Hobbes speaks of knowledge *τῷ ὄντι*, of the mere fact that things are, and yet directs us to his understanding of the question of the whatness of things. He indicates the way this understanding stands within the metaphysical tradition, and yet he abstracts from the problems generated by the tradition.

In the light of the logical priority of the question of the whatness of things, the knowledge of

causes (*τὸ διότι*) is itself a mode of the knowledge of the whatness of things, though distinguished from the knowledge of the whatness of things simply. The distinction drawn here is not merely between the "what" and the "how," but is itself a division of the "what." This is clearly evidenced by the fact that the descriptions of both kinds of knowledge speak of wholes and parts, or the categories which govern conceiving or the articulation of the whatness of things. The crucial point, then, is the precise nature of these wholes and parts.<sup>44</sup>

From the perspective of the "that/what" simply, or common sense, every particular is a whole, e.g., a man. Thus with respect to our knowledge of "that/what" that is the rational extension of common sense, we are more concerned with, or "know more," things as they appear to us in common sense. When we reason about things or the parts of things, we reason about them as things toward which we comport ourselves in the everyday world. Thus when we speak of man in practical, moral discourse we speak of man as we encounter him in practical life and not as matter in motion. In the latter case man would not differ from elephants or trees, and this difference is essential for moral and practical discourse.<sup>45</sup> In the light of the practical origin and end of human reason, however, the question of the "that/what" simply is characterized by the moral and theoretical content of common speech. Since human reason is first characterized by opinion about the good and the bad and since this opinion is political and problematic, man's rational nature produces "diverse opinions concerning him who hath the supreme power." This diversity causes the fact that "those persons, who by some are looked on as best are by others esteemed to be the worst of all men."<sup>46</sup> The opinions of common speech articulate prereflective notions about those virtues which constitute human perfection. Because

<sup>41</sup> We are again reminded of the possibility of ratiocination from sense and memory, which is not simply reducible to reasoning about cause and effect, and of the logical priority of the question of the whatness of things.

<sup>42</sup> "Homo enim non modo *corpus naturale* est, sed etiam civitatis, id est (ut ita loquar) *corporis politici* pars. Quamobrem considerandus erat tum ut homo tum ut civis; id est, ultima physicae cum principiis politicae conjungenda erant, difficillima cum facillimis." *De Homine*, Ep. Ded., LW, II, see also *Leviathan*, Intro., pp. 81–83; EW, I, 73–75.

<sup>43</sup> *De Cive*, VII, EW, II, 94. So among men there is, because of speech, a "contestation of honor and preferment," and "man scarce esteems anything good, which hath not somewhat of eminence in the enjoyment, more than that which others do possess," and some men suppose themselves "wiser than the others" and hence claim to rule. *De Cive*, V, EW, II, 66–67; see *Leviathan*, chap. 13, pp. 184–185; chap. 17, pp. 225 ff.

<sup>44</sup> EW, I, 66–67. Hobbes's emphasis.

<sup>45</sup> See pp. 1341–1343, *supra*.

<sup>46</sup> LW, I, 59.

these opinions are political and exclusive, they claim to illuminate the perfection of the whole of man in the light of the perfection of a part. Thus the tendency of common speech is the articulation of the whatness of man that teaches natural superiority, or completeness and defect, and poses for theoretical resolution the problem of intermediate wholes, where parts can be wholes and wholes must be wholes by virtue of their participation in some larger whole.

In this light Hobbes's remarks in the immediate sequel are important. Of the knowledge we have of the causes of things (*τὸ διότι*) he argues that "by parts I do not here mean parts of the thing itself, but parts of its nature as, by the parts of man, I do not understand his head, or his shoulders, his arms, etc., but his figure, quantity, motion, sense, reason, and the like; which accidents being compounded or put together constitute the whole nature of man, but not the man himself."<sup>47</sup> The import of these remarks is that wholes or whatnesses, when studied from the perspective of knowledge *τὸ διότι*, are constituted of parts abstracted from wholes or whatnesses as they appear in common sense. As such they are determinate and differentiable like the wholes of common sense or practical speech, but they cannot be intermediate wholes. The problem of part and whole as it arises from common speech (parts as wholes and wholes as parts) is eliminated from wholes that are intelligible in the light of common (moral) speech. When we speak of wholes as the subject of knowledge *τὸ διότι*, the knowledge of any particular is identical with the knowledge of any other like particular. The problem of the superiority or ontological completeness of any part or whole is simply unintelligible from the perspective of knowledge *τὸ διότι*.

Hobbes concludes his discussion of parts and wholes by remarking that

this is the meaning of that common saying, namely, that some things are more known to us, others more known to nature; for I do *not* think that they, which so distinguish, mean that something is known to nature, which is known to no man; and therefore, by those things, that are more known to us, we are to understand things we take note of by our senses, and by more known to nature, those we acquire the knowledge of by reason; for in this sense it is, that the whole, that is, those things that have universal names (which, for brevity's sake I call universal) are much more known to us than the parts, that is, such things as have names less universal (which I therefore call *singular*); and the causes of the parts are more known to nature than the cause of the whole; that is, universals than singulars.<sup>48</sup>

Hobbes again emphasizes that knowledge *τὸ διότι* originates in common sense and ratiocination and

adds that, with respect to the knowledge of cause and effect (*τὸ διότι*), the part is prior in importance to any phenomenal whole. Hence, in the knowledge of cause and effect we can abstract from things as they appear in common sense and see these wholes as the product of universal cause or, for Hobbes, matter in motion. As such the determinate and differentiable wholes are merely the inescapable phenomenal appearances that mediate the reality of universal matter in motion. Thus the beings articulated by the knowledge of cause and effect have twofold natures. First, they are determinate, differentiable wholes that are intelligible in the light of common sense and common (moral) speech.<sup>49</sup> Second, they are mere appearances or modes of universal cause and so susceptible to human use and conquest.<sup>50</sup> Wholes in this latter sense would have no inherent nature that would hinder their being subjected to human use, save for the limitations of matter.

It is the miracle of Hobbes's doctrine of method that the twofold nature of the wholes of knowledge *τὸ διότι* links the morally intelligible whatnesses of common sense and practical speech to the beings that, according to the new science, are merely the objects of human utility.<sup>51</sup> The new science combines the illumination of the things of nature (including the human things) as morally determinate whatnesses with the reduction of those things for human use. The new science, then, facilitates the human conquest of nature whereby human being becomes the model for being as such. That is, insofar as the beings of knowledge *τὸ διότι* are determinate wholes that originate in common sense, they become morally intelligible

<sup>49</sup> The objects of universal cause must always be determinate, differentiable phenomenal wholes particularized "according to some certain accidents or accident" lest "all agents, seeing that all bodies are alike, would produce like effects in all patients." EW, I, 121.

<sup>50</sup> The principles of first philosophy are "the first principles by which we know the *διότι* of things." The metaphysics of first philosophy, or natural philosophy universal, which teaches the nature of motion as universal cause, is, therefore, determined by method and the distinction between knowledge *τὸ διότι* and *τὸ διότι*. The order of study that proceeds from first philosophy or the study of universal cause is from first philosophy to geometry, and then to mechanics or the study of the effects of one body upon another, then to physics, and finally to moral philosophy, which studies man as "the subject of physical contemplation." EW, I, 69-73, 87-88.

<sup>51</sup> See n. 22, *supra*; *Leviathan*, chap. 5, p. 115. Cf. EW, VII, 183-184. So far is the knowledge of cause and effect from being the sole knowledge of the phenomena of nature that physics is, of itself, indemonstrable. Only when causes are in our power is the knowledge of cause and effect demonstrable with respect to the phenomena of nature. It is for this reason that *both* knowledge from cause to effect and knowledge from effect to cause are parts of philosophy or science rather than science and prudence respectively.

<sup>47</sup> EW, I, 67.

<sup>48</sup> EW, I, 67-68.

whatnesses by means of their being the objects of human use. They are morally intelligible in the light of the human needs and desires, articulated in common speech, that they complete or fulfill. The most important point for Hobbes is that the whatnesses illuminated by science answer the question of the "what" as it arises from practical discourse, but eliminate the problem of intermediate wholes. With this teaching about natural parts and wholes, Hobbes completes the first task of the doctrine of method.

It may be objected that this interpretation ignores the ambiguity of Hobbes's remarks about the nature of parts. They might seem to refer as much to his definition of knowledge *τῷ ὅτι* as to knowledge *τῷ διότι*. In fact, this ambiguity strengthens rather than weakens the present analysis of Hobbes's teaching about method. If his remarks refer also to knowledge *τῷ ὅτι*, then it should be argued that Hobbes presents a new or different picture of common speech than has hitherto been seen. In fact, this is what Hobbes does. The theoretical perspective of knowledge *τῷ ὅτι*, originating as it does in the moral context of common speech, illuminates the possibility of intermediate wholes, which is not the case for the theoretical perspective of knowledge *τῷ διότι*. Thus the difference between wholes illuminated by the two perspectives consists in the possibility of intermediate wholes. Yet the ascent from practice to theory requires the theoretical explication of intermediate wholes; it requires the theoretical resolution of the problem inherent in the theoretical content of common speech. Thus, the conquest of nature facilitated by the new science of cause and effect makes possible the *unproblematic* ascent from practice to theory only if the perspectives of knowledge *τῷ ὅτι* and knowledge *τῷ διότι* can be harmonized. This harmony accounts for the superiority of method over classical philosophy and it is produced by the methodological reformation of common speech. Before we consider this reformation of common speech, the second task of the doctrine of method, we must consider Hobbes's remarks about body and accident and examine his apparent nominalism. This digression is important because it introduces Hobbes's philosophical—or perhaps we should say scientific—rhetoric, which proves to be the fundament of the reformation of common speech and of the doctrine of method as such.

#### Body, Accident, and Names

Hobbes's discussion of body and accident in the *De Corpore* provides the account that is missing in the *Leviathan*. The discussion of body and accident is part of the explication of "first philosophy." Since "first philosophy" is itself the

product of method,<sup>52</sup> the distinction between body and accident presupposes the teaching about wholes and parts that is central to the doctrine of method. As a part of the discussion of "first philosophy," the distinction between body and accident is presented from the perspective of the knowledge of cause and effect. Therefore, Hobbes speaks of accidents as the vehicles for generation and change in contrast to bodies, which are ungenerated and changeless. In order for generation and change to exist, accidents cannot be material or "in" corporeal particulars lest "an accident would be a body also."<sup>53</sup> Hobbes is careful to argue that particular accidents "do perish continually" and then argues strangely that "bodies are things and not generated," while "accidents are generated and not things."<sup>54</sup> Unless we are willing to ascribe to Hobbes the opinion that non-things can be generated, we must conclude that by "things" he means here simple corporeal particulars abstracted from any determinate character or form. Surely bodies with determinate forms are things, and surely they are generated and destroyed. The reason for the distinction between body and accident is that accidents are complex, while body as body is not. This complexity is composed of the difference between particular and universal. Particular accidents perish continually, but properties or "whole natures" do not. Likewise, insofar as accidents are changeable and generated, they are the subject of knowledge of cause and effect; but the doctrine of method teaches that the knowledge of cause and effect, generation and change, does not exhaust our rational knowledge of things. Thus Hobbes argues in the sequel that "that accident for which we give a certain name to any body, or the accident which dominates its subject, is commonly called the *essence* thereof, as rationality is the essence of man; whiteness of any white thing, and extension the essence of body. And the same essence, *in as much* as it is generated, is called the form."<sup>55</sup> Form may change while matter does not, but form in this context is a mode of "essence" considered from the perspective of generation, or the knowledge *τῷ διότι*. We are again reminded that the metaphysical relation of eternal universals to noneternal particulars exists because of the corruptibility of any particular. That these universals are, in Hobbes's scheme, themselves generated by means of scientific conquest poses no problem of circularity; the generation of the morally intelligible eternal is the miracle of the new science of nature.

<sup>52</sup> EW, I, 70–73, 87–88.

<sup>53</sup> EW, I, 104.

<sup>54</sup> EW, I, 117.

<sup>55</sup> *Ibid.* Eadem essentia quatenus generata, forma dicitur. LW, I, 104.



Although the distinction between body and accident is an extreme embarrassment to anyone who would have Hobbes a nominalist or materialist, it is true that Hobbes often sounds like a nominalist when he discusses universal names. In his attacks upon the universals of the schools, however, Hobbes presents no arguments why universals as such are impossible. He argues against the schools that unless the copulation of names takes place between names of the same kinds of things (bodies, accidents, phantasms, and names themselves) the result is "incoherence and false propositions."<sup>56</sup> Yet he provides no account of the kinds of things and hence of what names in fact refer to what kinds of things. Indeed the account of the kinds of things is to be found in the explication of method, which is profoundly antinominalist. Again, Hobbes argues like a nominalist that it is an error to say that "the idea of *anything* (conception) is universal; as if there could be in the mind an image of a man, which were not the image of some one man, but a man simply, which is impossible; for every idea is one, and of one thing. . . ." But in arguing so, he implies a materialistic atomism (every idea is of one thing) that is nowhere defended in his writings.<sup>57</sup> Hobbes's attack against the universals and metaphysics of the schools is a rhetorical polemic.

None of Hobbes's arguments aim at refuting the possibility of universals, but in the *De Corpore* his discussion of names reflects the understanding of universals, or intelligible whatnesses, articulated by his doctrine of method. Hobbes states that

a common name, being the name of many things severally taken, but not collectively all together, (as man is not the name of all mankind, but of every one, as of Peter, John, and the rest severally) is therefore called a universal name; and therefore this word universal is never the name of anything existent in nature, nor of any idea or phantasm formed in the mind, but always the name of some word or name; so that when a living creature, a stone, a spirit, or any other thing, is said to be *universal*, it is not to be understood that *any* man, stone, etc., was or can be universal, but only that these words, *living creature*, *stone*, etc., are universal names, that is, names common to many things; and the conceptions answering to them in our mind, are the images and phantasms of several living creatures, or other things.<sup>58</sup>

The meaning of this remark is twofold. First, it clearly limits the level of abstraction allowed to

our rational articulation of the things of nature. That is, there is no such thing in nature as the universal abstracted from body. When we use universal names, we always refer to wholes encountered as particulars. There is no speech about whatnesses that is not necessarily related to particular whatnesses. Thus, though accidents can never be identified with bodies or corporeal subjects, the abuse of abstract names springs from the belief that accidents can be completely abstracted or "separated from *all* bodies."<sup>59</sup> Since the beings of nature are morally intelligible by being subservient to man's self-perfection, the integrity of the eternal universal depends upon the existence of at least one human being. Second, this argument does not deny that universal names denote intelligible whatnesses that may be eternal while particulars perish, but only denies that the whatness articulated by a universal name can of itself have the nature of a particular. That is, the universal is not itself a particular thing and cannot, therefore, exist as a model for any particular thing with respect to its particularity. Thus the relationship between the intelligible universal and the particular is not teleological in the problematic sense of natural superiority. The difference between universal and particular, or whole and part, is exhausted by the distinction between the sempiternal and the ephemeral. Hobbes asserts that the most profitable part of the new science of nature is the science of man's body. In the light of the self-perfection promised by the new science, eternity is at least a possibility for man as such, if not for each and every human particular.<sup>60</sup>

Hobbes's attack against the metaphysics of the schools is a rhetorical polemic and not a consistent or carefully reasoned argument. Indeed, Hobbes's attack veils the metaphysics of the doctrine of method. This rhetorical veil points to the second task of the doctrine of method, which requires the use of rhetoric; it also points to the fact that, for the sake of presentability, the doc-

<sup>59</sup> EW, I, 33. Except for the most extreme and vulgar "Platonism," this remark would hardly be a scandal to the classical tradition. For Christianity it is an anathema.

<sup>60</sup> Hobbes's nominalism is at best very inconsistent. While the discussion of common and universal names in the *De Corpore* is consistent with the metaphysics of the doctrine of method, Hobbes's nominalism is much more radical in explicitly religious or political contexts. Hobbes's nominalism is a necessary component of his methodological rhetoric, which aims at the reformation of common speech and veils the comprehensive teaching about method. See pp. 1349-1351, *infra*. Watkins has noted the equivocal character of Hobbes's "nominalism" and even hints that practical or moral intentions may govern this nominalism. The point is, however, not whether or not Hobbes maintained a "Humpty-Dumpty theory of truth and falsity" but rather that Hobbes was a new kind of King's man. Watkins, *Hobbes's System of Ideas*, pp. 144-50, 158.

<sup>56</sup> EW, I, 57-61. See *Leviathan*, chap. 4, p. 108; chap. 5, pp. 113-15; chap. 9, pp. 146-47.

<sup>57</sup> *Ibid.* This is the case whether this atomism is "epistemological" or "ontological." In either case Hobbes's argument begs the question of the kinds of things that exist, which question is, to repeat, comprehended by his teaching about method.

<sup>58</sup> EW, I, 19-20.



■trine of method itself must consist in a new scientific rhetoric.

#### Method as Rhetoric

For Hobbes, the identification of final cause with human will as efficient cause and the grounding of a morally intelligible natural whole on human conquest did not of themselves remedy the fundamental defect of classical natural and political philosophy. Unlike Descartes and Bacon, Hobbes believed the scientific project to be only partially completed with the possibility of man's conquest over the natural world. For if Hobbes agreed that knowledge is power, he also believed that his modern predecessors were but dimly aware of the true nature of power. It is in the science of natural justice that the full understanding of power is to be found.<sup>61</sup> This should not be surprising; since the coherence of the whole of man and nature depends upon the dominance of human desire or practice over nature, that which guarantees the coherence of desires is the key to the proper and complete reformation of science and philosophy.

The first task of the doctrine of method was to establish that nature could be theoretically and morally intelligible only in the light of the human conquest of nature. The guiding intention of this task was to subvert the possibility that an articulated but indifferent nature, or nature in itself, might serve as the model for resolving the practical-theoretical problems inherent in the natural structure of speech and reason. Although the metaphysical question of the whatness of things is answered in terms of human possibilities, this question does arise from the imperatives of practical, moral discourse. Because of the practical origin and structure of speech and reason, the newly central question of the whatness of man is bounded by the articulations of common speech. This fact is subversive of the unproblematic ascent from practice to theory because Hobbes admits that the problem of natural superiority is inherent in common speech.<sup>62</sup>

For Hobbes the true superiority of modern over ancient science depends not simply upon the scientific conquest of nature by man, but also

upon the methodological resolution of the problematic relation between practice and theory. This requires what we have called the second task of the doctrine of method. Since practice is the beginning and end of human reason, the harmonious relationship of practice and theory, and hence the successful completion of practical speech by the scientific conquest of nature, depends upon the methodological reformation of common speech. The reason why scientific conquest is a sufficient teaching about nature is not that it provides answers to the problems engendered by common speech but that the methodological reformation of common speech eliminates the controversial problems of common speech. Thus the desires articulated by common, moral speech and completed by the new science of nature can be rendered unproblematic by Hobbes's new science of natural justice or sovereign political power. Although Hobbes claims that the first principles of his new political science can be discovered by simple introspection, we see that the new political science is a methodological science because it depends upon the second task of the doctrine of method.

The science of natural justice is presented, of course, in the *Leviathan*. In the *Leviathan*, the methodological reformation of common speech is resplendently simple and is contained in the assertion of natural equality. It is from the assertion of natural equality as the teaching of reformed common sense that the doctrines of natural justice and sovereignty are derived.<sup>63</sup> From this assertion it results that with respect to the being of man, part is equal to part and whole is equal to part. Nothing can be a property of the universal "man" that is not a property of *every individual* man; there can be no problem of intermediate wholes or natural superiority. The revised common speech of the *Leviathan* is rooted in the opinion of natural human equality, which only the fool refuses to acknowledge "in his heart." This equality makes it possible for Hobbes in the *Leviathan* to view the study of man and justice in terms of the science of cause and effect. The argument from the universal cause of fear is based on the theoretical claim of the commonsensical self-evidence of human equality.<sup>64</sup> The new political science, based on the assertion of human equality and on the trustworthy passion of fear, provides a simple and certain rule of action, whereby men's con-

<sup>61</sup> Hobbes insists that because of his new political science the "Science of Naturall Justice is the onely Science necessary for Sovereigns." *Leviathan*, chap. 31, p. 407. Hobbes was not a simple conventionalist, for the power and scope of his new political science depended on the possible harmony of nature and convention. Thus the positive precepts of civil law are "of equal extent" to or are contained by the laws of nature, to which men are obliged *in foro interno* in the prepolitical natural condition. *Leviathan*, chap. 14, pp. 189-192; chap. 15, pp. 201-206, 215; chap. 26, p. 314.

<sup>62</sup> See nn. 17, 18, 37, 46, *supra*.

<sup>63</sup> *Leviathan*, chap. 11, pp. 160-61; chap. 13, pp. 183-88; chaps. 14, 17, 18; *De Cive*, III, EW, II, 38-39; EW, IV, 102-105.

<sup>64</sup> *Leviathan*, chap. 15, pp. 203-204. So, too, Hobbes's defense of justice is grounded upon the argument for the commonsensical self-evidence of human equality; see *Leviathan*, Intro., pp. 81-83.

flicting needs and desires can be unproblematically harmonized.<sup>65</sup>

Although the *Leviathan* asserts the equality of men, the natural structure of speech, i.e., the theoretical tendency of any speech, requires some defense of this assertion. In the *Leviathan*, this defense is contained in the discussions of speech, reason, and method. It consists in a critique of those cognitive powers which would resolve the practical-theoretical problem of natural superiority generated by common speech. The *Leviathan* presents method as merely the proper use of definitions and narrows the scope of human reason to the knowledge of cause and effect simply. The *Leviathan* suggests that the moral and metaphysical question of the whatness of things is, in fact, either fictional or extrarational, or shared by the irrational, amoral brutes.<sup>66</sup>

The function of the defense of the assertion of equality is twofold. First, it satisfies the theoretical tendency of common speech because it is a theoretical defense. Second, in providing this satisfaction, it greatly limits the possibilities of theoretical speech. The aim of Hobbes's theoretical argument in the *Leviathan* is to truncate the theoretical tendency or content of common speech. The aim of the *Leviathan* is to incapacitate that speech which, unchecked, raises the problem of parts, wholes, and natural superiority and so might challenge the dogmatic assertion that eliminates the problem of parts, wholes, and natural superiority. It is only on the basis of this theoretical subversion of theory that the perspectives of knowledge  $\tau\omicron\upsilon\ \delta\eta\iota$  and knowledge  $\tau\omicron\upsilon\ \delta\upsilon\beta\eta\iota$  can be harmonized. Only on the basis of this harmony can the conquest of nature be the sufficient resolution of the problems of practical life and speech. In order for men to perfect science there must be peace. In order for there to be peace, science must provide the sufficient salvation for man. But both this peace and this salvation require that men be purged of the natural tendency toward the serious consideration of controversial speech.

Even though the defense of the assertion about human equality is a critical defense, it is theoretical. For this reason, then, the revision of common speech has its own theoretical teaching. This teaching is an apparent moral relativism supported by an antimetaphysical account of speech and reason.<sup>67</sup> This defensive teaching would seem

to subvert the possibility of a doctrine of the natural good for man, but a careful inspection of Hobbes's arguments shows that the moral relativism or nominalism of the *Leviathan* rests upon a solid doctrine of the natural good for man.<sup>68</sup> All Hobbes's remarks that appear to defend radical moral relativism or nominalism refer to the description of particular goods that fall under a general, natural doctrine of the human good.<sup>69</sup> The ground of Hobbes's political teaching is inconsistent with the defensive dress in which he presents it.

We have seen that the theoretical dress of the *Leviathan* is skimpy and is an inadequate basis for the attack against traditional metaphysics. No argument is presented to explain why or how the narrow understanding of the meaningful scope of reason could be adequate for the doctrine of man presupposed in the *Leviathan* itself. We can now appreciate the fact that, in comparison with the teaching of the *De Corpore*, the discussion of speech, reason, and method in the *Leviathan* must present a partial or incomplete account of the human things. The political science of the *Leviathan*, and hence the second task of the doctrine of method, depends upon an incomplete teaching about human speech and reason. What is more, this incomplete teaching contradicts the comprehensive doctrine of method. Incompleteness, disproportion between its content and the implicit doctrine of man which it defends, and the want of explanation of how a theoretical subversion of theory is possible all point to the rhetorical character of the *Leviathan's* theoretical teaching.

Whereas the comprehensive doctrine of method is a comprehensive doctrine of the human good, the theoretical teaching about method in the *Leviathan* is a rhetorical device necessitated by the conjunction of Hobbes's intention to reform the relation between theory and practice with the natural structure of speech and reason. As parts of such a device, the relativism and nominalism of the *Leviathan* present the problem of politics, which springs from the chaotic effects of human equality, so that, though its solution depends upon the teaching of the human good by nature, the eye is diverted from careful theoretical scrutiny of

<sup>65</sup> This simplicity consists in the equation of the precepts of natural law with the precepts of civil law. *Leviathan*, chap. 26, p. 314.

<sup>66</sup> *Leviathan*, chap. 9, pp. 131, 147-148; cf. EW, IV, 26-30.

<sup>67</sup> *Leviathan*, chap. 6, p. 120; chap. 10, p. 150; chap. 11, p. 160; *De Homine*, XI. 4, LW, II, 96-97. *De Cive*, III, EW, II, 47-48.

<sup>68</sup> *Leviathan*, chap. 15, pp. 215-16; *De Homine*, XI. 5-15, LW, II, 96-103; *De Cive*, III, EW, II, 47-50.

<sup>69</sup> So "... though all men do agree in the commendation of the foresaid virtues, yet they disagree still concerning their nature, to wit, in what each of them doth consist." *De Cive*, III, EW, II, 48. Likewise, "theft, murder, adultery, and all injuries are forbid by the laws of nature; but what is to be called theft, what murder, what adultery, what injury in a citizen, this is not to be determined by the natural, but by the civil law." *De Cive*, VI, EW, II, 85; see *Leviathan*, chap. 26, p. 314.

at human good. In the *Leviathan*, the problem of moral chaos is presented as a problem to be solved by the machinery provided by the sciences of justice and representative sovereignty. To the extent that the human good by nature is visible, it is presented as unproblematic and commonsensically self-evident.

The rhetoric of the *Leviathan* is necessitated by the intention of the doctrine of method. To remedy the traditional disharmony of practice and theory, the human good must become the center of a teaching about nature whereby the conquest of nature fulfills or completes human need and desire. In order for this to be a sufficient teaching about nature and the human good, a political science is required which presents and exhorts belief in the dogmatic assertion of the human good as dependent upon human equality, and which abstracts, as far as possible, from a comprehensive account of that good. Any comprehensive account of the human good would reveal the intelligible practical-metaphysical problem of ancient philosophy and Hobbes's scientific-philosophic rhetoric that veils the natural problematic of human speech and reason. It is because of the rhetoric of the *Leviathan*, a rhetoric necessitated by the comprehensive doctrine of method, that the *Leviathan* is equivocal about the role and status of method in political science. The true superiority of the new science of nature depends upon the new science of politics. But the superiority of the new science of politics depends upon the superiority of Hobbesian rhetoric over ancient rhetoric. Hobbes's methodological rhetoric hides the dogmatism of the political doctrine of the human good. Small wonder it is, then, that Hobbes's new science of politics and its teaching about government have been characterized as a modern cloud.<sup>70</sup>

It is important to note that the teaching about method is comprehensive. It articulates man, nature, and the structure of the articulation of man and nature. When Hobbes speaks as the user of method he speaks as one who is cognizant of the problem of the relation between practice and theory. As he presents his doctrine of method, Hobbes stands outside the progression that begins

with common speech and proceeds to the theoretical resolution of the problems of common speech. Method stands above or outside both practical and theoretical speech in order to resolve the problem of practice and theory. It would seem that this external stance would be crucial to any defense against the charge of the circularity of maintaining the incorrigible priority of common speech while at the same time presenting a theoretical reformation of common speech. However this stance might be defended, it is clear that Hobbes provides no such defense at all. In fact, Hobbes gives no direct account of method itself with respect to practice and theory. He simply presents method as if it stands outside of the two modes of human speech. The reason for this is that any more direct account of method must acknowledge that common speech presents problems to be solved, yet the ultimate success of method depends on the presentation of an unproblematic common sense or common speech. Method treats of problems which arise from common speech just as theory is derived from practice, yet method aims at the reformation of common speech to abolish the very problems from which it necessarily arises. Although the doctrine of method is a comprehensive teaching about man and the human good, it does not and cannot provide a comprehensive theoretical resolution of the problems inherent in common speech. Moreover, with respect to its intention, the doctrine of method cannot resolve theoretically the theoretical problem of how a teaching that insists upon the logical priority of common speech can revise the content of that speech. The doctrine of method is comprehensive not because it is theoretically complete, but because it founds and explicates the new rhetoric that helps the promise of modern science overshadow the problems of both practice and theory.

It is Hobbes's intention to reform the relationship between practice and theory and his recognition of the intelligible, ineradicable problems of practice and theory that necessitate the indirect presentation of the doctrine of method. It is this intention and recognition that govern Hobbes's careful terminology, his apparent nominalism, and his indirect manipulation of the traditional doctrine of substance. The new methodological rhetoric, which dresses the necessary political science, must also veil the doctrine of method itself. In this respect, the new methodological rhetoric is the substantial core of the comprehensive doctrine of method.

### Conclusion

Hobbes's rhetoric has been remarkably successful. To the degree that serious philosophic controversy arises within the Hobbesian tradi-

<sup>70</sup> Mansfield, *Hobbes and the Science of Indirect Government*, p. 110. If the teaching of the *Leviathan* is a modern cloud it is a familiar cloud. Its common opinion is what would today be called "enlightened" common opinion. As such it champions the quasi-theoretical principles of moral relativism and the equation of reason and science in the name of a pre-supposed absolute understanding of the human good. Hence we see that the *Leviathan*, which purports to spring from the heart-searching of introspective common sense, in fact begins with a doctrine of man based upon the theoretical abstractions of "passion" and "power."

tion, the problem of obligation is the surviving rump of all that was obfuscated by Hobbes's doctrine of method.<sup>71</sup> Yet the still much discussed and unresolved problem of obligation and the corollary problem of the delimitation of sovereign power stand as monuments to Hobbes's success.

The result of Hobbes's revision of common sense is the quasi-theoretical doctrine of radical individualism. From the perspective of radical human equality, the essential characteristics predicable of man are only those characteristics predicable of each and every individual. For Hobbes, man is the animal species of which it can be said that each individual is a rational seeker of his own preservation. The doctrine of representative sovereignty is grounded upon this individualist perspective. To speak of representative sovereignty as that which solves the human problem must be no more or less than to speak of representative sovereignty as that which solves the problem of each and every individual. This is, of course, why Hobbes begins his discussion of government with men being equal in the state of nature, and it is for this reason that in the state of nature there is right but no duty, and rightful killing but no injustice or infringement of right.<sup>72</sup> There is right in the state of nature, but no violation of right in the state of nature, so great is the identification of "man" with every *individual* man. There can be no violation of right because the possibility of such violation would require the delimitation of an individual man's actions by a standard applicable to all men. It is clear, then, why the state of nature is the starting point for Hobbes's political teaching and why the doctrine of natural right, which is learned from contemplating the state of nature, is the sole standard for evaluating any possible mode of government. For the teaching of natural right in the state of nature presents the clearest and most extreme repudiation of any possible natural superiority that might be illuminated by the consideration of man's practical circumstances. The state of nature, then, is the clearest example of Hobbes's reformation of common opinion and thus is an intermediate and positive stage in the process of enlightenment.

It is central to the individualist origin of Hobbes's teaching of natural right that natural right always be prior to any possible claim of natural law or duty. The commands of the sovereign, which Hobbes insists are necessary for the consistent harmony of the precepts of natural right and the end of natural right, are always problematic.<sup>73</sup> In fact, the commands of the sovereign raise the very question supposedly eliminated by methodological political science. They originate in the rational necessity of each and all, but also, because of the necessary absoluteness of the sovereign, possibly from the rational necessity of all at the expense of some or one. Hence the sovereign can kill without injustice or violation of right.<sup>74</sup> Natural right dictates the submission to sovereign power and hence obligation to the sovereign, and yet sovereign power can be a potential danger to the end of natural right and hence no object of obligation at all. It is not sufficient to argue that the return of the individual to the state of nature solves this problem because sovereignty is a rational and necessary corollary of the state of nature.<sup>75</sup> The problem of obligation is an ineradicable contradiction at the heart of Hobbes's political science rather than a problem to be solved.

To see the problem of obligation as the crucial question of political philosophy is to accept the fundamental principles of Hobbes's political science. Yet the only problem which protrudes from the rhetorical veil of Hobbes's political science is insoluble of itself. However, it is precisely the insolubility of the problem of obligation that, for Hobbes, underscores the necessity of government itself. For the contradiction of the problem of obligation presents man with the sole and stark alternative of government simply or no government at all. What better support could there be for a political science that by eviscerating philosophic speech and providing a "sure measure" of political conduct serves the highest human conquest made possible by science? In order for Hobbes's political science to serve the Baconian hope of man's liberation from natural necessity, it must admit no alternative to a teaching that despises the distinction between liberty

<sup>71</sup> See note 1, *supra*. See also Peters, *Hobbes*, pp. 193–200, 201–203; Watkins, *Hobbes's System of Ideas*, pp. 171 ff; John Plamenatz, "Mr. Warrender's Hobbes," *Hobbes Studies*, pp. 73–87; Howard Warrender, "Reply to Mr. Plamenatz," *Hobbes Studies*, pp. 89–100; Brian Barry, "Warrender and His Critics," in *Hobbes and Rousseau*, ed. M. Cranston and R. S. Peters (New York: Anchor Books, 1972), pp. 37–65; cf. Hume, "Of the Original Contract," *Essays Moral, Political, and Literary* (Oxford: Oxford University Press, 1963), pp. 452–473.

<sup>72</sup> *Leviathan*, chap. 13, p. 188; chap. 14, pp. 189, 192; chap. 15, p. 215.

<sup>73</sup> *Ibid.*, chap. 14, pp. 189, 192, 196. The distinction between obligation *in foro interno* and *in foro externo* is transferred from the state of nature to civil society when Hobbes argues that "when therefore our refusal to obey, frustrates the End for which the Sovereignty was ordained; then there is no Liberty to refuse: Otherwise there is." *Leviathan*, chap. 15, p. 215; chap. 21, pp. 269–274.

<sup>74</sup> *Ibid.*, chap. 13, p. 186; chap. 20, pp. 252–253; chap. 21, pp. 264–265, chap. 28, p. 354.

<sup>75</sup> *Ibid.*, chap. 14, p. 196; chap. 17, pp. 223–224; *De Cive*, V, EW, 11, 65–66.

and tyranny.<sup>76</sup> Hobbes's political science fulfills the wish of the sophist; it harmonizes the harsh necessity of politics with the softness of speech and persuasion.

Hobbes's teaching, like those of the other early founders of modernity, is a comprehensive teaching about man and nature. It was not Hobbes's intention to debunk the scope and questions of classical metaphysics but rather to refashion them so as to reform the relation between practice and theory. Hobbes's attack against the tradition was rhetorical, and his teaching ultimately admits that any comprehensive account of man and nature reveals a set of ineradicable moral and theoretical problems that constitute the human condition. However, unlike the tradition, Hobbes does not counter these problems with a counsel for moderation. Rather, the human problems are to be solved methodologically. This means that,

by means of methodological rhetoric and methodological political science, the human problems are to be submerged by the promise and hopes of modern science, which consist, at the very least, in the promise of long life and commodious living.

Hobbes's intention was to persuade those who think as well as those who do not. His persuasion reveals his distrust of most men who think as well as his distrust of those who do not. For just as the doctrine of method requires the use of philosophical rhetoric in the presentation of a necessary political science, so too the doctrine of method itself must be veiled by a subtle rhetoric. Hobbes's doctrine of method produces the curious paradoxes of modernity. For the many, the basest and most timorous standards serve as the ground of the most hubristic posture for mankind. For the few who think, the glory of conquest replaces trust in moderation for the moderation of the problematic human condition.

<sup>76</sup> *Leviathan*, chap. 19, pp. 239–40.

# Nonincremental Policy Making: Notes Toward an Alternative Paradigm\*

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Two major paradigms have come to dominate the scholarly analysis of public policy and quite possibly, the policy-making process itself. One is the decision model of incrementalism; the other a "divisibility" model of piecemeal public programs with negotiated and specialized pay-offs.

Incrementalism, described by Charles E. Lindblom, is a decision model which asserts the propensity of organizations to move in small steps.<sup>1</sup> Because of (1) disagreement on primary values and policy objectives, and (2) the difficulty of gathering and processing information on which to evaluate a wide range of potential policy options, policy makers typically arrive at their decisions by assessing only "limited comparisons to those policies that differ in relatively small degree from policies presently in effect."<sup>2</sup> The strategy of incrementalism is one of continual policy readjustments in pursuit of marginally redefined policy goals. Long-term plans are abandoned in favor of short-term political implementation.

The divisibility paradigm is not unrelated to incrementalism. It asserts, basically, that any large policy undertaking is simply the aggregation of many politically self-contained subprograms and activities. Policy is the distribution of discrete goods in portions—in line with prevailing configurations of power or publicized need.<sup>3</sup> This

divisibility assumption, of course, lies behind the concept of "pluralism" invariably applied to the analysis of industrialized democracies.<sup>4</sup> It is implicit in the emerging "public choice" school of policy analysis.<sup>5</sup>

The divisibility paradigm supports the perception of the public interest as simply the sum total of countless individual interests.<sup>6</sup> As Anthony Downs has aptly described the paradigm in practice: "Each decision-maker or actor make whatever choices seem to him to be the most appropriate at that moment, in light of his own interests and his own view of the public welfare."

These two paradigms, as mentioned, enjoy a currency both in policy analysis and policy practice. Few critiques of these outlooks have appeared, and those criticisms which are offered suffer from inflated normative judgments and anemic descriptive insights.<sup>8</sup>

Yet the breadth of application accorded incremental and divisibility outlooks has not been without its costs. In particular, these paradigms have deprived policy analysis and public administration of attention to a class of policy enterprises which fit into neither framework. This

found in Theodore J. Lowi, *The End of Liberalism: Ideology, Policy and the Crisis of Public Authority* (New York: W. W. Norton and Company, 1969).

<sup>4</sup> For the classic statement regarding pluralism see, of course, David B. Truman, *The Governmental Process* (New York: Alfred A. Knopf, 1951).

<sup>5</sup> The public choice or economic market models of the policy process are presented in such works as James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962); Warren F. Ilchman and Norman T. Uphoff, *The Political Economy of Change* (Berkeley: University of California Press, 1969); Robert L. Curry and L. L. Wade, *A Theory of Political Exchange* (Englewood Cliffs, New Jersey: Prentice-Hall, 1968); and William C. Mitchell, *Public Choice in America* (Chicago: Markham Publishing Company, 1971).

<sup>6</sup> An excellent description of this "realist" approach to the public interest can be found in Glendon A. Schubert, *The Public Interest* (Glencoe, Illinois: The Free Press, 1961), ch. 4.

<sup>7</sup> Anthony Downs, *Urban Problems and Prospects* (Chicago: Markham Publishing Company, 1970), p. 37.

<sup>8</sup> See, for example, Yehezkel Dror, "Muddling Through—'Science' or Inertia?" *Public Administration Review*, 24 (September, 1964), 153–157; Dror, *Public Policymaking Reexamined* (San Francisco: Chandler Publishing Company, 1968); and Amitai Etzioni, "Mixed Scanning: A 'Third' Approach to Decision-Making," *Public Administration Review*, 27 (December, 1967), 385–392.

\* The author wishes to thank Francis E. Rourke and Matthew A. Crenson of the Johns Hopkins University, as well as T. Alexander Smith, David M. Welborn and T. McN. Simpson of the University of Tennessee for their advice and criticisms regarding this study. In addition, officials of the National Aeronautics and Space Administration and the National Science Foundation, giving generously of their time, immeasurably advanced the work. Finally, a great deal of support and encouragement were provided by Francis E. Rourke, to whom this essay is gratefully dedicated.

<sup>1</sup> See Charles E. Lindblom, "The 'Science' of Muddling Through," *Public Administration Review*, 19 (Spring, 1959), 79–88; Lindblom and David Braybrooke, *A Strategy of Decision* (New York: The Free Press, 1963); and Lindblom, *The Intelligence of Democracy: Decision-Making Through Mutual Adjustment* (New York: The Free Press, 1965).

<sup>2</sup> Lindblom, "The 'Science' of Muddling Through," p. 84.

<sup>3</sup> For a description of "divisible" policy making see Robert A. Dahl and Charles E. Lindblom, *Politics, Economics and Welfare* (New York: Harper and Brothers, 1953); E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart and Winston, 1960); and Robert A. Dahl, *Pluralist Democracy in the United States* (Chicago: Rand McNally, 1967). A more critical appraisal of the same phenomenon can be

essay is about that class of policies. These are enterprises distinguished by their demand for *comprehensive* rather than incremental decisions; synoptic rather than piecemeal outlooks and vision. These policies are characterized by an *indivisibility* in the political commitment and resources they require for success.

This class of nonincremental, indivisible policies is perhaps small but is nonetheless significant. It consists of large-scale government undertakings commanding major shares of the public budget. Frequently these undertakings involve the application of new technologies to major political or social problems. Nonincremental policies must be cast within large-scale and risk-taking frameworks if they are even to approximate acceptable levels of goal-seeking performance. Again, as we shall see, these are characteristics for which conventional analytical models are inadequate.

In order to explore adequately the implications of nonincremental, indivisible policy, one case has been selected from this class for explication. Its analysis, supplemented by references to additional policy examples, will illustrate three primary characteristics of major significance—characteristics which elude the present coverage of policy-making models. The policy is U.S. manned space exploration—a large and revealing public undertaking.

#### The Nonincremental Policy Start-up

The first characteristic associated with this special class of policies to which we have been referring is important. *Nonincremental, indivisible policy pursuits are beset by organizational thresholds or "critical mass" points closely associated with their initiation and subsequent development.* These policies must rely for their success upon factors which come into play only at high levels of political and resource commitment. Manned space exploration elaborately illustrates this point.

On May 25, 1961, President John F. Kennedy, in a special message to Congress, proclaimed the goal of landing a man on the moon and returning him safely to earth. This was to be accomplished before the end of the decade—an undertaking explicitly designated as a major national commitment. In communicating his intentions to the Congress the President indicated that space-related budget allocations were to be enlarged, and that the pace of spacecraft and booster development was to be accelerated. In the ensuing five fiscal years National Aeronautics and Space Administration appropriations rose from \$1.8 billion to \$5.2 billion, and the total number of employees associated with space exploration programs (both within and outside of NASA)

climbed from less than 60,000 to 420,000 at its peak.<sup>9</sup>

Yet the Kennedy announcement came a full four years after the birth of the "space race"—the launching of the Soviet satellite Sputnik I. What had happened to the nation's space enterprise during this four year interim? The answer is highly significant. It illustrates the "start-up" dilemmas of the nonincremental policy enterprise.

#### Public Policy vs. Public Pressure

The history of manned space exploration in this interim period between the Sputnik launch and the Kennedy commitment is one of opinion intensity in the midst of organizational insufficiency. The Sputnik launch (and its even more spectacular sequel—the canine-carrying Sputnik II) had aroused in the United States both alarm at the technological and projected military capabilities of the Soviet Union, and exasperation at the slow pace of U.S. missile and rocket development. Although survey data during the immediate post-Sputnik period are sketchy, available studies suggest both a high degree of worldwide awareness of space events and a subsequent belief that space achievement was in some way linked to national prestige and international power.<sup>10</sup>

Gabriel Almond, in an early survey of opinion in Great Britain, France, Italy, West Germany, and the United States, discovered an "extraordinarily high" public recognition of initial satellite launch achievements. "Almost every respondent in the countries surveyed was aware of the launching of the first satellite and of the fact that Russia had launched it. Subsequent awareness of the first American success was also high."<sup>11</sup> This awareness was reinforced by the perceived linkage of space achievement to national strength. As Almond concluded, "One of the most stable popular beliefs of the postwar era—the belief in the scientific and technological superiority of the United States—has been rudely shaken, and its place has been taken by anxious estimating

<sup>9</sup> "Statistical Report," *National Aeronautics and Space Administration, Programs and Special Documents Division* (Washington, D.C.: U.S. Government Printing Office, 1971). For a review of the process of commitment see John M. Logsdon, *The Decision To Go To The Moon* (Cambridge: M.I.T. Press, 1970).

<sup>10</sup> See Gabriel A. Almond, "Public Opinion and the Development of Space Technology," in *Outer Space in World Politics*, ed. Joseph M. Goldsden (New York: Praeger Publishing Company, 1963), pp. 71–96; Donald N. Michael, "The Beginning of the Space Age and American Public Opinion," *Public Opinion Quarterly*, 24 (Winter, 1960), 573–582; and Albert J. Lott and Bernice E. Lott, "Ethnocentrism and Space Superiority Judgments Following Cosmonaut and Astronaut Flights," *Public Opinion Quarterly*, 27 (Winter, 1963), 604–611.

<sup>11</sup> Almond, "Public Opinion and the Development of Space Technology," pp. 73–74.

that fluctuates with each report of a significant step forward in satellite launchings."<sup>12</sup>

In the United States itself, opinion intensity was high following the Sputnik launches—even though public attitudes were not particularly coherent. "The opinions held by many Americans regarding this first step into space were sometimes inconsistent, occasionally rich in *non sequiturs*, and frequently illogical."<sup>13</sup> Nevertheless, public demand for a policy response to the Sputnik achievement was strong. A Survey Research Center poll conducted shortly after Sputnik reported that "77 per cent thought that the Russian satellite should make 'a difference in what we are doing about the defense of this country', with . . . 47 per cent going for a crash program on weapons development."<sup>14</sup>

Set apart from this intensive public arousal was the inchoate and disordered organizational and administrative framework associated with the development of United States space exploration capabilities. The major components in this matrix were: the National Advisory Committee for Aeronautics (NACA), a small research and consultative agency for the aeronautics industry and other divisions of the Federal government; the three military branches, each conducting piecemeal research and development operations in line with its own particularistic notions of what space exploration was all about; and, of course, the U.S. aircraft industry, only marginally involved in space flight research (apart from weapons development projects) and confronted with a traditional reliance on "in-house" rocket development on the part of such government organizations as the Army Ballistic Missile Agency.<sup>15</sup>

This period in the development of the space program—in which high levels of public arousal confront underdeveloped organizational forms—is extremely interesting for it illustrates an important relationship between public policy and public pressure. Public pressure generally oscillates freely within a political system—frequently enlarging rapidly, then declining suddenly.<sup>16</sup> Such oscillations can occur anywhere along a continuum of intensity primarily as a function of complex determinants of attitude and attention.

Yet public policy is not similarly free to move along a continuum insofar as its scale is con-

cerned. Again much has been said in policy analysis about the incremental nature of program advances. Yet *relative to public pressure*, public policy is beset by a dependence upon organizational features which cause it to enlarge or contract in discontinuous "jumps"—as jurisdictional, manpower or budgetary plateaus are reached.

In fact, given the highly disparate ways in which policy and pressure enlarge, *it is unlikely for them ever to be appropriately matched*. Pressure expands as a continuous function (although as mentioned earlier it is subject to wide and rapid fluctuations along its intensity continuum). Policy undertakings, on the other hand, enlarge as a step function; they are beset by discontinuities which reflect thresholds associated with their expansion. Enabling legislation, for example, is frequently required for the initiation of a policy pursuit. Once established, a policy does not expand far from the dimensions of its genesis without soon confronting jurisdictional, manpower, and appropriations boundaries. Then it must *leap* over these boundaries—it must secure an enlargement of its legislative authorization, upgrade its manpower supplies, and justify an increased level of appropriated funds.

Significantly, this process does not come to define a smooth gradient. It generally occurs in spurts. Nonincremental policies in particular must expand greatly *if they are to expand at all*. Only then can they overcome the inertia, external resistance, or internal start-up problems which act as barriers to policy expansion. It is perhaps a telling illustration of this all-too-ignored discontinuity of policy enlargement, that a general analysis of public programs by the Tax Foundation concluded: "The general pattern characterizing the growth of . . . new [Federal] programs is this: sharp increases in the first two years as the programs get into fuller operation, relatively modest increases in the third and fourth years, followed by a steep jump of the sort depicting major expansion or legislative extensions of the program."<sup>17</sup>

Let us review again the policy-pressure relationships mentioned above. As pressure builds for government action within a specific issue setting it is likely to confront a partial vacuum in ongoing public policy. The beginnings of the ecology "movement" amply illustrate this phase—where increased public concern over problems of environmental quality quickly overbalanced the minimal regulatory standards of the federal government. This initial stage is one of *underscaling* in public policy. Such an underscaling also char-

<sup>12</sup> *Ibid.*, p. 77.

<sup>13</sup> Michael, "The Beginning of the Space Age," p. 581.

<sup>14</sup> *Ibid.*, p. 579.

<sup>15</sup> See, for an account of this period, Richard S. Lewis, *Appointment on the Moon* (New York: The Viking Press, 1968).

<sup>16</sup> For an interesting discussion of public opinion "stages" and their determinants, see Anthony Downs, "The Issue Attention Cycle and the Political Economy of Improving our Environment," Royer Lectures, University of California, Berkeley, April 13-14, 1970.

<sup>17</sup> *Growth Trends of New Federal Programs: 1955-1968* (Washington, D.C.: The Tax Foundation, 1967), pp. 19-20.



acterized the organizational insufficiencies of the space program in the period on which we have so far been concentrating.

Eventually, of course, public policies are generated in response to escalating public pressure. But, again, the discontinuous nature of policy expansion makes it difficult to match appropriately government performance with fluctuations in public "demand." Frequently, the appearance of government activity itself can contribute to the dissipation of public arousal. Secure in the symbolic reassurance that "something is being done," the public shifts its fleeting attentions to other issues of greater currency and fashion.<sup>18</sup> This decay in public concern can lead ultimately to a condition in which policy performance actually exceeds public demand, which might well be termed an *overscaling* dimension to public policy. At this extreme of the policy-pressure relationship, bureaucratic programs can persist long after generalized support for such pursuits has subsided. In regulatory policy making this is typically the stage at which the "capture" of the agency by a specialized constituency can occur. Because public arousal has waned, the regulatory agency must turn for its support to the one social component whose attention and watchfulness remain unwavering—the very interest which the agency seeks to regulate. Should policies in general fail to locate steadfast constituencies during this period of overscaling, the result must invariably be that saddest of administrative spectacles—the politically orphaned agency, an object (in Norton Long's words) "of contempt to its enemies and despair to its friends."<sup>19</sup> The overscaling dimension too has become an important part of the manned space exploration story.

In essence the expansion-support "game" is one at which the nonincremental, indivisible policy enterprise is considerably disadvantaged. This is so because of the extreme start-up lags to which it is subject. Nonincremental policy is beset by substantial discontinuities in both enlargement and payoff. Continuing the description of manned space exploration will illustrate this point more fully.

We left space exploration in the interim period between the first Soviet Sputniks and the Kennedy moon-landing commitment. This was an era of a "lagging, directionless space program,"<sup>20</sup> or, as one critic expressed it, a "space maze and missile

mess in Washington."<sup>21</sup> In February of 1958 the Advanced Research Projects Agency (ARPA) was established in the Department of Defense to coordinate and oversee diverse military missile and space satellite programs. This was an initial effort to upgrade U.S. space exploration in light of conspicuous Soviet successes, yet it proved to be hopelessly underscaled in relation to distinctive organizational thresholds and inertia which had to be overcome. These thresholds are a critical feature of the nonincremental policy enterprise.

### The Dilemma of "Thinking Small"

The first problem is a psychological one. Perhaps no barrier is more essential for nonincremental policy to breach and overcome permanently than the penchant for thinking small. Overcoming this trait is a major necessity in developing the imagination and receptivity closely associated with organizational innovation.<sup>22</sup> In addition, thinking small is also a detriment to the planning and jurisdictional extensions upon which nonincremental policy depends.

Before the lunar landing commitment, underestimates on the rate of technological advance and the engineering "do-ability" associated with manned space programming were commonplace. No less a figure than Hugh Dryden, NACA Director and a future Deputy Administrator of NASA, asserted a scant four years before Sputnik: "I am reasonably sure that travel to the moon will not occur in my lifetime. . . ."<sup>23</sup> Such myopia was certainly understandable, yet adequate space planning required both foresight and an obstinate faith in the future approaching that which drives the classic "revolution of rising expectations." It was not until this radical departure in perspective could find a congenial institutional environment that efficient preparation for manned missions could begin.

Thinking small also served as a barrier to the jurisdictional expansion upon which space exploration depended. NACA, the central research agency in the administrative network concerned with space exploration, actively resisted the Eisenhower administration's efforts to upgrade its policy-making jurisdiction. "By February 1958, as the Eisenhower administration began wrestling

<sup>18</sup> Donald W. Cox, *The Space Race* (New York: Chilton Books, 1962), p. 69.

<sup>22</sup> See, for example, Chris Argyris, *Organization and Innovation* (Homewood, Illinois: R. D. Irwin and Company, 1965) for a discussion of how innovative output can vary as a function of organizational culture.

<sup>23</sup> Hugh L. Dryden, as quoted in Loyd S. Swenson, Jr., James M. Grimwood and Charles C. Alexander, *This New Ocean: A History of Project Mercury* (Washington, D.C.: U.S. Government Printing Office, 1966), p. 56.

<sup>19</sup> In this context see Murray Edelman, *The Symbolic Uses of Politics* (Urbana, Illinois: University of Illinois Press, 1964).

<sup>20</sup> Norton E. Long, "Power and Administration," *Public Administration Review*, 9 (Autumn 1949), 257.

<sup>21</sup> Richard S. Lewis, "The Kennedy Effect," *Bulletin of the Atomic Scientists*, 24 (March, 1968), 2.

with the complexities of formulating a national program for space exploration, NACA had taken the official position that, with regard to space, it neither wanted nor expected more than its historic niche in government-financed science and engineering. While NACA should become a substantially bigger instrument for research, it should remain essentially a producer of data for use by others."<sup>24</sup>

This reluctance to accept an enlarged organizational assignment (and, indeed, to participate in a timely institutional growth) led ultimately to the demise of NACA later that same year. In its place the National Aeronautics and Space Act of 1958 established a national space advisory council (The National Aeronautics and Space Council) and NASA (which absorbed most of NACA's personnel). NASA was created to attain another major threshold associated with a nonincremental policy such as space exploration. This was the requirement for administrative consolidation.

#### Consolidation Requirements

Even though the Space Act provided for a distinctive civilian/military apportionment of space exploration activity between NASA and DOD, NASA immediately upon its formation began to press for the acquisition of key research groups and programs located within the military services. In less than three months NASA had acquired Project Vanguard from the Navy (as well as the 150 staff members and \$25 million in appropriations connected with it), lunar probe and earth satellite projects from the Air Force, and the Jet Propulsion Laboratory (complete with Caltech staff and \$100 million in appropriations) from the Army.<sup>25</sup> It even attempted to take over the Army Ballistic Missile Agency amid shrieks of outrage from that service. Although delayed by Army opposition, NASA's ambitious consolidation plans were not to be denied. In March of 1960 NASA formally acquired the ABMA and most of its "von Braun team" of key researchers.

These acquisitions reflect the importance to the nonincremental, indivisible policy enterprise of consolidating control over those suboperations upon which it closely depends. In industrial settings consolidation of this type is referred to as vertical integration—an effort to amalgamate raw materials and component production processes within the same managerial framework as that governing the creation of a final marketable product for which these subprocesses are important. (Of course, at the same time NASA was ac-

complishing a type of horizontal integration—eliminating the potential sources of space exploration programming competition).

The nonincremental policy requires an extensive consolidation because of the close interdependency of its component parts. Uncertainty of performance failures in any one suboperation can threaten the success of them all. Many research and development projects require parallel breakthroughs on a variety of problem fronts. The development of a rocket guidance system, for example, demanded simultaneous advances in both computer miniaturization and thruster engine design.

For manned space exploration, consolidation was an essential organizational threshold to surpass, given the extremely diverse and ramified operations upon which any manned-mission plan would depend. "Whether it was given special responsibilities or not, NASA had to concern itself with the nation's overall space program if it was to optimize its own."<sup>26</sup>

Administrative consolidation also allowed NASA to recruit, both from industry and from other government agencies, highly qualified technical personnel to work on its programs. In part, consolidation triggers the rising expectations essential to mobilize the manpower required for nonincremental policy making. As one NASA official observed: "We believe that we are attracting quality people . . . because we have salaries which are competitive, plus a new, attractive and exciting program, and an *expanding mission* which creates the possibilities of greater opportunities."<sup>27</sup>

Closely related to the administrative consolidation requirements are the following specific start-up thresholds which space exploration policy making had to overcome.

#### The Indivisibility Dimensions of Research

A great deal of organizational management theory centers around the importance and inevitability of specialization, both for bureaucratic supervision and for institutional problem solving. Yet this attention to specialization has resulted in the partial neglect of those operations for which specialization is not entirely appropriate. One of these, oddly enough, is the major research project—the very setting in which specialized knowledge would seem most important. But a large-scale research problem is frequently interdisciplinary—cutting across lines of specialization and impart-

<sup>24</sup> Rosholt, p. 106 (italics supplied).

<sup>25</sup> Swenson, Grimwood, and Alexander, *This New Ocean*, p. 77.

<sup>26</sup> Robert Rosholt, *An Administrative History of NASA: 1958-63* (Washington, D.C.: U.S. Government Printing Office, 1966), pp. 45-48.

<sup>27</sup> Abe Silverstein, Director, Space Flight Programs, NASA in *Hearings Before the Senate Committee on Aeronautical and Space Sciences*, June 7-12, 1961 (Washington, D.C.: U.S. Government Printing Office, 1961), p. 177 (italics added).

ing an indivisibility to organizational tasks. For problems such as these "research teams are only divisible down to a minimum effective scale."<sup>28</sup> Conversely, research teams must first attain this scale if they are to mount an effective attack upon a critical problem.

Major research progress in space flight technology was delayed until interdisciplinary groups could be assembled on a scale which would multiply their problem-solving effectiveness. Below this threshold a host of conflicting theories and experimental results remained largely unreconciled while still other "good ideas" went begging for attention.

In discussing indivisibilities associated with space research it is also important to note the extent to which much of this research was "equipment-intensive." The study of acceleration tolerances, for example, required the construction of large centrifuges in order to simulate the stresses of space flight. Research into designs and materials which would safeguard spacecraft reentry into the earth's atmosphere depended upon sophisticated hypersonic wind tunnels and was delayed pending the construction of such devices. Advanced manned-mission planning in general was predicated upon prior unmanned missions of research and experimentation. It was considered essential "to strengthen NASA's program for the unmanned exploration of the moon, using hard-landing Ranger spacecraft and soft-landing Surveyor spacecraft. The unmanned exploration of the moon was considered an absolutely essential step prior to manned landing."<sup>29</sup>

This equipment-intensive characteristic of much space exploration research heightened the thresholds associated with its start-up. The design and construction of specialized instrumentation and simulation devices was expensive, and the scarcity of such equipment served to confine research activity to a few isolated groups with access to these devices. These expense and isolation problems which typified space research are not at all unlike the features associated with large-scale scientific research in general.<sup>30</sup> These were responsible for important lags in research progress prior to the policy acceleration which organized and funded research groups at critical levels of problem-solving effectiveness.

### Land Acquisition and Facilities Construction

Another feature of manned space exploration to which start-up thresholds were attached was land acquisition and facilities construction. Enormous quantities of land and elaborate spacecraft tracking, construction and testing facilities were required before any manned mission planning could realistically begin.

The Atlantic Missile Range alone required more than 100,000 acres of land. While some of this land had been acquired earlier by the Air Force, far larger tracts were needed by NASA in order to construct an adequate launch facility at acceptable margins of safety. Their acquisition clearly reflects the threshold problems attendant upon the nonincremental policy enterprise. As it developed, the buying of land was a process not readily susceptible to gradualism. It required both executive branch clearance and specific congressional authorization—actions which in turn were predicated upon elaborate programming justifications. Significantly, it was not easy to attain these requisites on a small scale because of a peculiarity in the approval process. *Program plans had to be large if they were also to be persuasive.* Larger scale plans communicated a sense of urgency and purpose. In a very real sense, the scale of the acquisition request was a major factor in upgrading the probabilities of its own fulfillment.

A further problem in the land acquisition process was the need for land holdings to be concentrated at the early stages of agency planning. This was important because the very act of land acquisition (and the publicized prospect of its subsequent development) raised the price (and in some cases altered the zoning) of surrounding land in highly unfavorable directions. This made it far more difficult to acquire additional land later.

It was for precisely this reason that NASA Administrator James Webb, in requesting congressional authorization for land acquisition within the Atlantic Missile Range area, argued vigorously for permission "to take all the land we can visualize that would be required. Once we get this, I do not think it will be possible to get any more land there. It will be too expensive."<sup>31</sup>

### Qualitative Correlates of Scale

A final class of start-up thresholds consists of those space exploration requisites which were only qualitatively realized under the impact of scale. Reliability in space hardware, for instance, a

<sup>28</sup> Harry Townsend, *Scale, Innovation, Merger and Monopoly* (London: Pergamon Press, 1968), p. 25.

<sup>29</sup> Rosholt, *Administrative History of NASA*, p. 194.

<sup>30</sup> For a discussion of the organizational features related to the conduct of large-scale scientific research, see Derek J. deSolla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963); and Alvin M. Weinberg, "The Impact of Large-Scale Science Upon the United States," *Science*, 134 (July 21, 1961), 161-164.

<sup>31</sup> James E. Webb, Testimony in (FY 1963 Authorization) *Hearings Before The Committee on Aeronautical and Space Sciences, U.S. Senate, 87th Congress, Second Session* (Washington, D.C.: U.S. Government Printing Office, 1962), p. 35.

critical dimension to any manned-mission planning, was closely dependent upon a large number of trial tests and subsequent "de-bugging" after prolonged use. As one NASA official described the problem: "We never quite know, when we launch a satellite, what is going to be the limiting point because we have not been in business long enough to establish these numbers. What you need to do is the thing you have always needed to do to develop long operational life; that is, to start to develop, to debug, and to build life through use."<sup>32</sup>

Another qualitatively derived function of scale is the external support upon which any nonincremental policy depends for success. Frequently, as mentioned, such support stems from the rising expectations which policy objectives are able to generate. In any case, major public approval and the prospect of its continuance is required to render the initial start-up costs worthwhile. An enormously expensive operation such as space exploration research "cannot be turned on and off like a faucet. It must be planned in advance, given adequate leadtime, and funded in such a way that there is assurance that it can move along systematically."<sup>33</sup>

As James Webb has contended:

A common denominator of large-scale endeavors is the necessity of a continuing "critical mass" of support. There must be enough support and continuity of support to retain and keep directly engaged on the critical problems the highly talented people required to do the job, as well as to keep viable the entire organizational structure. . . . Any uncertainty or shortfall in the support factor is apt to have far-reaching effects and force the endeavor into serious difficulties.<sup>34</sup>

In a very real sense, a nonincremental, indivisible policy pursuit must attain a "capture point" of public goal and resource commitment. Albert O. Hirschman has stressed the importance of this capture point in a study of major development projects in third world nations. In observing these capital-intensive national enterprises Hirschman notes the extent to which potential project difficulties are frequently underestimated at their outset. He then asserts that if a development project is to overcome these misestimations "its operators must be 'caught' by the time the unsuspected difficulties appear—caught in the sense that having spent considerable money, time and energy and having committed their prestige, they will be strongly motivated to generate all of the

problem-solving energy of which they are capable."<sup>35</sup>

Hirschman argues that many agricultural projects fail precisely because they are too short-term and small-scale to reach this capture point. When unanticipated problems arise, these projects are all too often prematurely abandoned. In effect, they fail because of a reluctance "to throw good money after what looks like bad, but could be turned into good, if only the requisite rescue effort were forthcoming."<sup>36</sup>

To be sure, this policy entrapment effect is not without its hazards. The attainment of a capture point may well lead after all to the casting of good money, time and effort after that which really is bad. This, it will be recalled, was a major argument against much of the United States military escalation in Vietnam.

Yet entrapment is important to the nonincremental public policy enterprise. It can assure the continuity of support so essential to policies of this class. At the same time, entrapment safeguards the nonincremental policy from many of the unanticipated problems associated with its pursuit. In space exploration, the Kennedy commitment sustained mission goals through even the darkest hours of equipment failure, cost overruns, and ultimately, the tragic spacecraft fire that took the lives of three Apollo astronauts.

#### Start-Up Requirements and Initial Space Decisions

In assessing these nonincremental start-up requisites, it is small wonder that a major political commitment, in fact a presidential mandate, was necessary to energize manned space exploration policy. As space administrators readily conceded "Science is, and cannot be, the driving force for space exploration."<sup>37</sup> It was indeed President Kennedy's national commitment which "galvanized the lagging, directionless space effort around a dramatic, not-too-distant goal."<sup>38</sup>

The full magnitude of the start-up operations to which reference has been made is well illustrated by budgetary data compiled for the early period of the space program. The following chart illustrates NASA expenditures from fiscal years 1961 through 1966, and highlights the research and development component of these expendi-

<sup>32</sup> Albert O. Hirschman, *Development Projects Observed* (Washington, D.C.: The Brookings Institution, 1967), p. 18.

<sup>33</sup> *Ibid.*, p. 20.

<sup>34</sup> Dr. Eberhard Rechtin, Director, Jet Propulsion Laboratory, California Institute of Technology, in *Hearings Before the Senate Committee on Aeronautical and Space Sciences, 88th Congress, First Session* (Washington, D.C.: U.S. Government Printing Office, 1963), p. 138.

<sup>35</sup> Richard Lewis, "The Kennedy Effect," p. 2.

<sup>32</sup> Silverstein, *Hearings Before the Senate Committee on Aeronautical and Space Sciences*, p. 83.

<sup>33</sup> Rosholt, *Administrative History of NASA*, p. 88.

<sup>34</sup> James E. Webb, *Space-Age Management: The Large Scale Approach* (New York: McGraw-Hill, 1969), pp. 62-63.

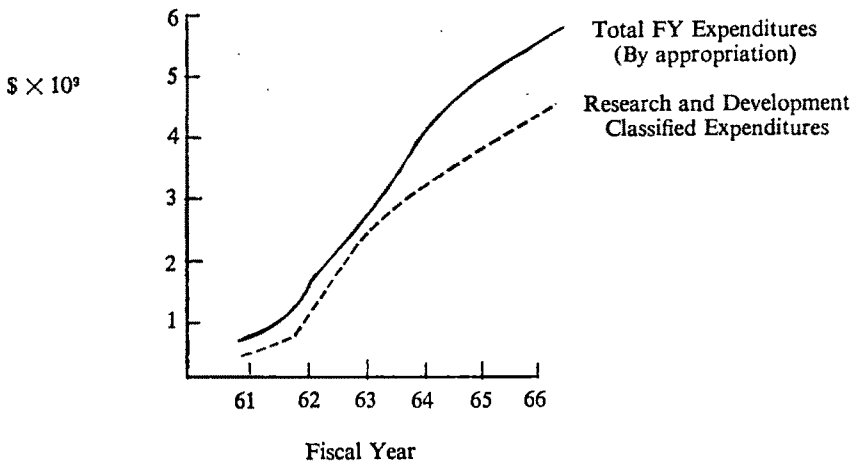


Figure 1. NASA Expenditures (FY 1961-1966)

tures. It is revealing to note two things in connection with these expenditures: the steeply rising rate of overall space spending, and the degree to which R&D-classified outlays approximate the total NASA funds spent. Seldom has a government agency increased its appropriations as rapidly as NASA. This strongly reflects the political support associated with the Kennedy moon-landing commitment. But the proportion of R&D expenditures is also significant. More than 80 per cent of NASA's funds went into starting up the space exploration enterprise—filling the gaps in theory, technology, and equipment required for space exploration to advance. (It is interesting to note, by way of contrast, that in 1961, only 2.9 per cent of the total U.S. GNP was spent on R&D.

In that year the average industry expenditure was 4 per cent of sales.<sup>39</sup>)

Another illustration of the dimensions of the space exploration start-up is provided by employment data compiled during the 1960-66 period.

This graph, depicting total employment on space exploration programs both within and outside of NASA, emphasizes the personnel mobilization required to pursue exploration objectives. This manpower pool represents substantial private industrial expansion as well as extensive programs of training and recruitment. It is unlikely that such a work force could have been assembled

<sup>39</sup> Daniel Hamburg, *R&D: Essays on the Economics of Research and Development* (New York: Random House, 1966), pp. 13 and 41.

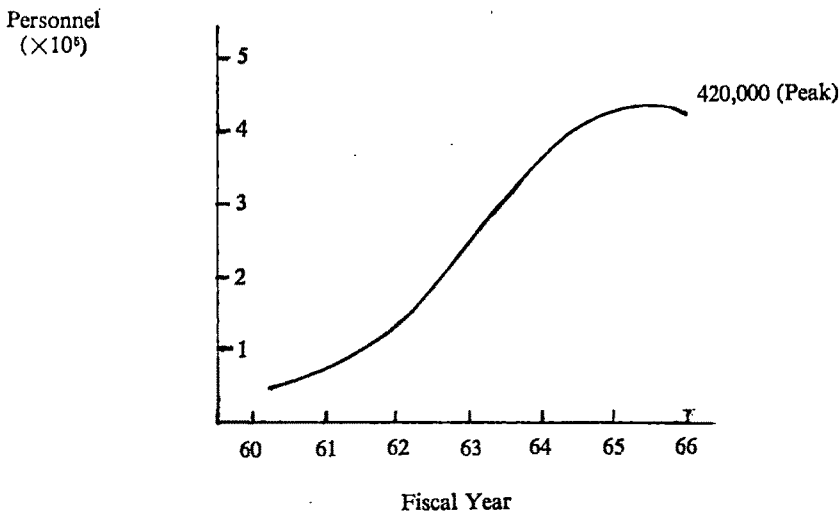


Figure 2. Total Employment on NASA Programs (1960-66)

without the prospect of a major *and continuing* space exploration commitment.

What we have seen so far of the start-up problems associated with manned space exploration illustrates the degree to which the model of incremental decisions fails conspicuously to account in this case for an observable reality. The start-up thresholds attached to nonincremental policy represent problems for which comprehensive (and even optimal) decisions are required. In reviewing the lunar landing program James Webb has argued: "We could not stop with doing 80 or 90 or 99 per cent of what we needed to do and come out reasonably well." Indeed, for a lunar landing "a partial success is likely to be a complete failure."<sup>40</sup>

Lindblom asserts, in his description of incrementalism, that "policy is not made once and for all; it is made and remade endlessly. Policy making is a process of successive approximations to some desired objectives in which *what is desired itself continues to change under reconsideration.*"<sup>41</sup> Yet contrast this with the extended planning and the continuity of support so essential to the nonincremental policy enterprise. The Kennedy commitment and the prospects for extended exploration missions were the major reasons that elaborate and expensive space start-up requirements were deemed worthwhile. Manpower training and recruitment closely depended upon the fixed future perceived for space programs. Shifts in policy objectives of the sort postulated by Lindblom would, under these circumstances, have jeopardized the future of the space program. They would have seriously undermined the continuity of support which space exploration policy, in this its start-up phase, so desperately required.

Finally, the incremental model of decision making greatly misrepresents the reality of administrative thresholds themselves. The appropriations, manpower, research, and facilities growth in the immediate aftermath of the Kennedy commitment was far more than marginally removed from the policy "state" by which it was preceded. Indeed marginal additions to a nonincremental, indivisible policy in the period of its start-up *will consistently fail to translate into incremental gains in policy performance.* Major, comprehensive commitments of personnel and resources are required before even limited pay-offs are derived. Thus it is extremely difficult to adjust input decisions to subsequent changes in output performance—a characteristic which renders nonincremental policy resistant to the

process of marginal refinement postulated by Lindblom.

### Instability in Nonincremental Policy

Apart from the problem of start-up thresholds, another major characteristic besets the nonincremental, indivisible policy pursuit: *nonincremental policy is in essence unstable—devoid of middle ground between self-generating states of growth and decay.* Again we turn to space exploration for the appropriate illustration.

During the mid-1960s manned space exploration rode the crest of what amounted to a dynamic and highly complex policy "movement"—a movement characterized by heightened technological and national aspiration, persistent political pressure for congressional appropriations, and seemingly open-ended prospects for organizational growth. It was perhaps during this period that a "match" was approached in that elusive balance between public policy and public pressure. Start-up thresholds had largely been overcome and space policy *outputs* (in the form of repeatedly successful manned missions) reached their maximum levels of public support.

Yet this match was to be short-lived. As Downs suggests in his analysis, "The Issue Attention Cycle," public support can quickly subside as a cycle of issue saliency runs its course.<sup>42</sup> This sudden ebb in public pressure can leave the nonincremental policy precariously overscaled relative to the political resources which exist for its support. It is precisely at this point that nonincremental policy may become exceedingly burdensome and vulnerable.

As both budgetary and employment data reveal, the sharply accelerating pace of space exploration in the early 1960s gave way toward the end of the decade to a major decline in policy support, even before the lunar landing objective was achieved. NASA appropriations declined from \$5.2 billion in fiscal year 1965 to \$2.8 billion in 1973. Even more dramatic evidence of space policy decline is provided by the statistics of total manpower employed on NASA programs. As Figure 3 depicts, total personnel dropped from the fiscal year 1965 peak of 420,000 to a low of approximately 108,000 in 1972.

Associated with this appropriations and manpower slide has been increased public and congressional criticism of space exploration in general, a decline in morale among remaining space program employees, and specific decays in organizational performance throughout the space policy bureaucracy itself.

As early as 1965, Gallup polls began to report

<sup>40</sup> Webb, *Space-Age Management*, p. 149.

<sup>41</sup> Lindblom, "The 'Science' of Muddling Through," p. 86 (emphasis added).

<sup>42</sup> Downs, "The Issue Attention Cycle," pp. 12-15.

Personnel  
( $\times 10^6$ )

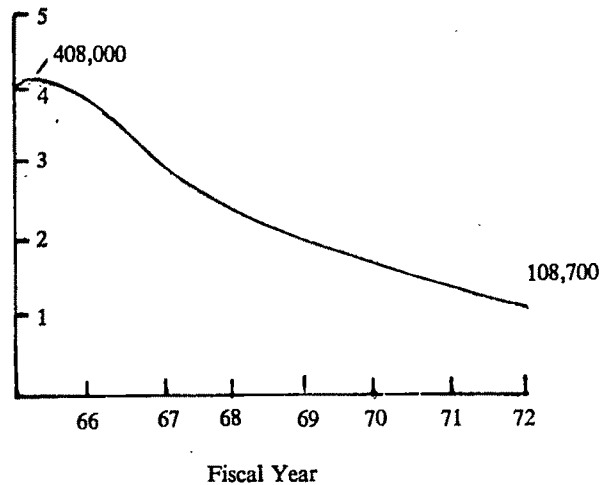


Figure 3. Downturn in Total Employment on NASA Programs (FY 1966-72)

significant erosion of both public interest and support where expensive, nonmilitary space exploration undertakings were concerned. A July 1965 Gallup poll, seeking to uncover opinion on the question "Which country—the United States or Russia—do you think is farther ahead in the field of space research?" elicited a 26 per cent "Don't know" response. This would seem to indicate a sizable disinterest in the space exploration "race" among the American people. The same poll revealed a 36 per cent preference among those contacted for decreased space exploration expenditures, as against 42 per cent in favor of keeping those expenditures the same and 16 per cent in support of their increase.<sup>43</sup>

Even more dramatic evidence of diminishing public space policy support is provided in a 1967 Gallup poll. Here, in response to the question "Do you think it is important or is it not important to try to send a man to the moon before Russia does?" a full 60 per cent of those interviewed assessed the landing race as "not important."<sup>44</sup>

By February 1969, in fact, 40 per cent of Gallup poll respondents called for a reduction in space research expenditures as against 41 per cent approving present levels of those expenditures. In addition, a July, 1969 poll (on the threshold of the lunar landing) revealed that 53 per cent of those interviewed opposed a Mars manned landing as a follow-on goal to the Apollo program. Only 39 per cent favored such a goal.<sup>45</sup>

Closely related to this erosion in general support for space policy was an increase in specific criticism directed at NASA programs and their expenditure requirements. A number of congressional critics began to question the "pork-barrel" distribution of NASA facilities. Public debates also began over the relative merits of manned versus unmanned flight. Such debates were particularly heated at times of NASA setbacks or Soviet achievements (such as the Lunakhod series) in purely instrumented space flight and experimentation.

National opinion leaders such as Senator Edward Kennedy and the Reverend Ralph Abernathy began to speak out against space exploration as a diversion of scarce national resources urgently needed elsewhere. In the aftermath of Vietnam war escalations, space exploration came to be viewed in some circles as an extension of military and defense policies and was opposed on that basis. Finally, "counterculture" values among a significant portion of the young led to a condemnation of space exploration as an extravagant exercise in mindless technology.

This increased opposition to space exploration has led in recent times to serious declines in morale within the space policy bureaucracy. Employee layoffs coupled with the failure to establish follow-on goals have contributed to the perception that the future is "running out" on the space program. This perceived termination of manned missions lies at the root of an intensified scientist-engineer controversy within NASA. The prospects of limited flight opportunity have intensified feelings of discrimination on the part of scientist-astronauts who have lost out in competition with pilot-astronauts in past mission assignments and

<sup>43</sup> Gallup Opinion Index, no. 3 (August, 1965), p. 16.

<sup>44</sup> Gallup Opinion Index, no. 22 (April, 1967), p. 19.

<sup>45</sup> Gallup Opinion Index, no. 45 (March, 1969), p. 17 and no. 50 (August, 1969), p. 20.

for whom few future space flight opportunities remain.<sup>46</sup>

### Declines in Organizational Performance

The sense of decline which has developed within the space policy bureaucracy has had important consequences for the effectiveness of that organization in the attainment of its objectives. A number of specific performance problems have emerged that could become highly significant given the close ranges of control tolerance upon which successful space flights depend. One of these difficulties lies in the movement of highest quality personnel out of NASA into areas of greater perceived excitement and career opportunity. Despite indifferent, or distinctively unfavorable job markets, much of NASA's most valuable managerial and scientific talent has left the agency for brighter prospects in universities, private industry or other government agencies. This exodus has left the space program without some of its most imaginative and capable personnel. As one former NASA official privately asserted: "Programs like Apollo attract people who like challenges. Once the major problems are solved these people are no longer interested. Those left behind are really mediocre, and they establish a self-fulfilling mediocrity within the program."

One of the most consistent areas of concern throughout the history of the space program has been that of hardware reliability and quality control. Space flight equipment, of course, requires the greatest technical care and precision in its design and manufacture. The image of the "white room" with its sterilized, dirt-free environment has come to symbolize the "assembly line" of the space age. Yet behind the elaborate rules and assembly procedures, the monitoring and testing technology, is a recognition that no matter how detailed or sophisticated the safeguards "if the employee doesn't care whether or not he makes a mistake, he will probably err."<sup>47</sup>

Operating under this assumption NASA has carefully developed programs to add a motivational component to quality control. NASA astronauts have made repeated visits to divisional facilities and contractors in order to upgrade the morale of space employees by increasing their sense of importance and identity with respect to space exploration goals. The lunar landing com-

mitment itself provided a major incentive for extra care in spacecraft assembly. Workers on Apollo hardware felt a special motivation to upgrade and sustain the quality of their job performance as a means of personal participation in an endeavor of historic proportions.

Yet, in recent times, along with the declines in public support and employee morale which have beset the space policy bureaucracy, an erosion has occurred in quality control capability. One NASA official conceded in a private interview that "the number of anomalies has risen alarmingly in each successive flight after [Apollo] 11." Another reports that with this erosion "worries about quality have become a fetish with higher management."

These statements are very important. Apollo 11, it will be remembered, was the first lunar landing mission. It marked the fulfillment of that commitment to which most of NASA's activities over the decade of the '60s had been dedicated. At the same time it represented the end of a period of clear direction for space exploration policy. One goal had lapsed, and no new ones had appeared to take its place.

This lack of direction probably contributed importantly to NASA's problems in quality control. Without the *esprit* which characterized the space challenges of the early '60s a lapse has developed in what might be termed the institutional "will to be careful." This may explain why hardware failures (or "glitches") have been occurring with increasing frequency in even tried and tested spacecraft systems.

### Dissolution of the Space Policy Bureaucracy

In addition to performance shortfalls, related decays can now be observed in the structure of the space policy bureaucracy itself. An organizational fragmentation and contraction has hit the space program and appears to be moving it in the direction of the inchoate administrative arrangements which characterized the immediate post-Sputnik period. Appropriations and manpower cutbacks are, again, highly influential here but so too is the failure to establish follow-on goals for a nonincremental policy.

University laboratories represent one structural decay already well established. Cutbacks in NASA's training grants and the elimination of its Sustaining University Program have pushed a number of university labs out of their involvement with space programs. Those laboratories which remain attached to the program find the general decline in NASA's experimentation interest a deeply foreboding sign for the future. As one observer notes: "The dilemma of the laboratories is that they require lead times of 18 months

<sup>46</sup> See "NASA: Trouble in Paradise," *Newsweek*, 74 (September 22, 1969), 73-74; and Marti Mueller "Trouble at NASA: Space Scientists Resign," *Science*, 165 (August 22, 1969), 776-779.

<sup>47</sup> James F. Halpin, *Zero Defects: A New Dimension in Quality Control* (New York: McGraw-Hill, 1966), p. 3.



o two years to develop instrumentation for satellite experiments and to train graduate students to handle the data. With nothing in the offing they can count on in the 1970's, they are having a difficult time maintaining a trained laboratory staff and keeping students on tap."<sup>48</sup>

Besides university laboratories, a number of small aerospace contractors have either dissolved under the impact of space appropriations cutbacks or are in serious trouble to the extent of their dependence upon contract revenues. Other former contractors have detached themselves from space exploration assignments in favor of more promising long-term prospects elsewhere. Even major contractors have not escaped internal upheavals during the recent period of fiscal and manpower retrenchment. Shrinkage of the aerospace industry has raised among some the "immediate concern . . . that the . . . manufacturing and testing establishment developed to build Apollo will fall apart."<sup>49</sup>

These space exploration declines—in public support, appropriations, personnel, morale, organizational structure and performance—all illustrate the fundamental instability inherent in the nonincremental policy enterprise. Nonincremental policy is not likely to realize a point of equilibrium or "steady-state" between support-mobilizing expansion and downward spirals of disillusionment and decline. There is perhaps nothing which space exploration demonstrates more convincingly than this very point.

Recognizing these exceptionally thin margins between policy expansion and decay, space policy makers have exhibited an impressive resourcefulness in attempting to maintain growth in their programs. Both Soviet and American space administrators, for example, have repeatedly praised the achievements of the other side in an effort to generate competitive spirit for increased efforts at home.<sup>50</sup> In recent times, as the enthusiasm for international competition has waned, NASA officials have stressed the possibilities of international cooperation in space as an incentive to the Nixon and Ford administrations to upgrade space funding. As one astute observer describes it: "The reasoning goes like this: If NASA could

establish an opportunity for improved Soviet-American relations through cooperation in space, this would presumably create the need for a series of post-Apollo missions of historic proportions. Considering the current politics of détente with the big powers, it would be hard for the White House or Congress to say no to funds for such an effort."<sup>51</sup>

The recent Soviet-American joint orbital flight, first announced during former President Nixon's Moscow summit, would appear to testify to at least the partial success of this new approach.

#### Attempts at a Policy Steady-State

To describe the instability associated with manned space exploration as a nonincremental policy, is not to deny that efforts have been made to find a point at which space programs could be stabilized. The Apollo Applications and Skylab projects have both been attempts to locate that intermediate ground where the continuity of space exploration missions could be assured. Skylab, in particular, was conceived as a balanced, well-paced series of orbital experimentation missions—a "holding program which keeps the technology alive, but barely so, until the nation recovers its interest in new frontiers and decides to become venturesome again."<sup>52</sup> But at its outset Skylab suffered in competition with the Army's Manned Orbiting Laboratory (MOL) program. Those Skylab missions which were undertaken were troubled by a public indifference as well as by manufacturing flaws in the Skylab itself.

Another stabilization effort, the Apollo Applications Program (AAP) failed dramatically to elicit the enthusiasm necessary to counterbalance the costs involved in its undertaking. In a study of post-Apollo planning in NASA, Emmette Redford and Orion White reported that "the AAP program remained throughout its development in a highly fluid state, without objectives ever being firmly decided upon."<sup>53</sup> They concluded:

The AAP, after being squeezed into acceptable dimensions by NASA's top leadership, the Bureau of the Budget, and the Congressional budget process, would have been given adequate support if it had not run into competition with other demands on national resources. What mainly brought it down . . . was apparently the budgetary stringency produced by the Vietnam war, the Great Society program, the riots in the cities and the fear of continued inflation.<sup>54</sup>

<sup>48</sup> Richard S. Lewis, "Goal and No Goal: A New Policy in Space," *Bulletin of the Atomic Scientists*, 23 (May, 1967), 19. See also in this connection W. Henry Lambright and Lauren L. Henry, "Using Universities: The NASA Experience," *Public Policy*, 20 (Winter, 1972), 61-82.

<sup>49</sup> Richard Lewis, "Our Terra-Lunar Transit System: Where Will It Take Us?" *Bulletin of the Atomic Scientists*, 25 (March, 1969), 22.

<sup>50</sup> For a discussion of this point see Francis E. Rourke, *Bureaucracy and Foreign Policy* (Baltimore: The Johns Hopkins Press, 1972), pp. 47-48.

<sup>51</sup> John Noble Wilford, "Cooperation in Space," *The New York Times*, December 6, 1971.

<sup>52</sup> Lewis, "The End of Apollo," *Bulletin of the Atomic Scientists*, 24 (September, 1968), 5.

<sup>53</sup> Emmette S. Redford and Orion F. White, *What Manned Space Program After Reaching the Moon? Government Attempts to Decide: 1962-1968* (Syracuse: Inter-University Case Program, 1971), p. 140.

<sup>54</sup> *Ibid.*, p. 223.

In this conclusion, however, Redford and White ignore the essential nonincremental and indivisible nature of space exploration policy. It is equally plausible that what really destroyed the Apollo Applications Program was its very *formulation* as an incremental and balanced policy alternative. Once "squeezed into acceptable dimensions," it lost the persuasive content that would have justified its proposed expenditures. This is precisely the problem of nonincremental policy. Without major mobilizing commitments (such as landing a man on the moon and returning him safely) these policies simply cannot generate and sustain the support required for their collective payoffs.

A deepseated paradox is at work here. In order to fit comfortably into an incremental, piecemeal framework the AAP was cast in a form which was basically not large enough to be persuasive. Then, without being able to generate its own collective political commitment, the AAP was suddenly too large for available amounts of public support.

#### Escalation and the Space Shuttle

These dilemmas faced by space policy managers in sustaining their programs have certainly not gone unheeded by top leadership in either the aerospace industry or the White House itself. NASA has, of course, targeted many of its public pleas for support directly toward presidential ears. Intensive industry and congressional pressure has also been brought to bear for the upgrading of space exploration policy goals.

In response to these pressures President Nixon appointed in 1969 a special Space Task Group, consisting of then Vice-President Spiro Agnew, Science Advisor Lee A. DuBridge, Air Force Secretary Robert C. Seamans and NASA Administrator Thomas O. Paine, to assess follow-on goals for space exploration. The group urged the adoption of a Mars manned landing goal, but at a "moderate pace"—no specific date and no overriding national priority were to be attached to the endeavor.<sup>55</sup> Yet the President was not yet willing to make any political commitment of this order, and efforts were continued toward finding some middle ground upon which space exploration policy (and expenditures) could unobtrusively rest.

As organizational decay continued, however, even a highly cautious Richard Nixon could not escape the dilemma of the nonincremental policy enterprise. On January 5, 1972, the President authorized NASA to undertake development of a partially reusable space "shuttle"—designed for

near-space transit and estimated to cost approximately \$5.5 billion within a six-year time period.

The space shuttle commitment represents a expansive goal with which to upgrade once again the manned space exploration program. The shuttle is predicated upon the notion that extensive investment is required in the immediate future in order to reduce the cost of space flight far in the future. The shuttle is a political commitment (by a reluctant President) to rescue the space policy bureaucracy from the decay spiral in which it had been caught. Thus it is with nonincremental policy—it is not readily susceptible to balance in any organizational steady-state.

At its height, space exploration constituted a policy "movement" fueled by high aspirations, public pressures, and seemingly open-ended prospects for organizational growth. Significantly, the instability described herein is a major characteristic of social movements in general. They must mobilize increasing numbers of persons to their support or they risk disintegration and decay. At the other extreme, the very success of a social movement can jeopardize its continuance insofar as success leads to institutionalization and its crystallization in bureaucratic form. Hannah Arendt has noted in this connection "the perpetual motion mania of totalitarian movements which can remain in power only so long as they keep moving and set everything around them in motion."<sup>56</sup> She contends that totalitarian movements "if they do not pursue global rule as their ultimate goal . . . are only too likely to lose whatever power they have already seized."<sup>57</sup>

The correspondence between social movements and nonincremental policy is striking and politically significant. Both require an extensive mobilization of public resources and support for their start-up. Both must then maintain this mobilization above a critical level or they will rapidly disintegrate. Both, in effect, require the presence of expansive goals as the primary means of protecting themselves from erosions in support.

The escalation of goals is a major requirement and a major dilemma of nonincremental policy. As Albert Hirschman has noted: "The promise of some sort of utopia is most characteristic of larger-scale undertakings such as the launching of social reforms or external aggression because they are likely to require heavy initial sacrifices."<sup>58</sup> Utopian promises (and sharply rising expectations) in themselves frequently contribute to explosive social instability. They are hardly likely

<sup>55</sup> See "The Post-Apollo Space Program: Directions for the Future," *Space Task Group Report to the President* (Washington, D.C.: U.S. Government Printing Office, 1969).

<sup>56</sup> Hannah Arendt, *The Origins of Totalitarianism* (New York: Harcourt, Brace and World, 1966), p. 306.

<sup>57</sup> *Ibid.*, p. 392.

<sup>58</sup> Hirschman, *Development Projects Observed*, p. 31.

o lead to public policies which are readily susceptible to balance or long-term steady-states.

### Public Policy and Indivisibility

A final, summary trait of nonincremental policy now presents itself. *Nonincremental policies are beset by an indivisibility which defies disaggregation into piecemeal decisions or additive partial advancements.* This means simply that for nonincremental policies a "self-containment" demand must be observed. Policy requirements as well as outputs must be provided at high levels or they cannot be provided at all.

Space exploration required elaborate and expensive start-up investments before it really got underway. Because of the delayed pay-off of the policy, a major political commitment was essential in order that these initial start-up costs would be judged worthwhile. *No short-term, piecemeal, or incremental commitment* could have mobilized the support and generated the expectations that space exploration required to surpass its start-up thresholds.

Perhaps this "threshold effect" lies at the heart of nonincremental policy. Thresholds themselves are highly important to the understanding of a great many social processes. As Kenneth Boulding has observed:

[Social] depreciation and appreciation are not continuous functions of use or load, but exhibit threshold or overload phenomena, which is what causes crises. . . . Continuous functions, which are fine for celestial mechanics, are characteristic of social mechanics only over small ranges of variation, and most social problems arise because of discontinuous functions—the road that suddenly jams up as one more car appears on it, the river that refuses to clean itself under a single addition of sewage, the international system that breaks down into war, or the city that erupts into riot when some small straw is laid onto some existing back.<sup>59</sup>

Thresholds make it exceedingly difficult for a series of small incremental steps to add up in cumulative fashion to one big comprehensive step. *They detach piecemeal decisions or resource commitments from corresponding or proportionate policy pay-offs.* This is a critical oversight in incrementalism and the divisible goods model of policy making, and an area to which a great deal of corrective attention should be directed.

In examining this last trait of nonincremental policy it is appropriate to offer additional examples of public enterprises which might fit into this class. Consider urban renewal undertakings, for

instance. In urban areas it is becoming increasingly apparent that the rejuvenation of the inner city is an operation to which thresholds or critical masses are attached. Slums represent resource and structural "sinks"—downward cycles of dilapidation and capital depreciation. The influx of limited recovery-inducing elements into the slum environment—in the form of public housing projects, piecemeal slum clearance efforts and private capital investment—is rarely sufficient to overcome the "sink" effect which characterizes the slum. The decay rate of public housing, as well as shrinking inner-city investment returns testify to the failure of small-scale, incremental policy interventions to effect slum rejuvenation.<sup>60</sup>

Perhaps only massive efforts, commitments or expenditures will result in a cycle of rejuvenation which would overcome the sink effect and establish itself as self-sustaining. Major capital inflows will begin to justify private inner-city investment. This investment will in turn enlarge the urban job market, providing resources and incentives for the repair and rebuilding of slum housing. The subsequent appreciation of this housing will add to the supply of inner-city capital, and so on.<sup>61</sup>

If this were so, an urban redevelopment policy, unless cast on a scale approaching its critical resource thresholds, has little chance of realizing—or even approximately realizing—its ends. The potential challenge here to a strategy of incrementalism is unmistakable. A series of small decisions or steps, below a policy threshold, will not advance the policy even incrementally in the direction of its goals. Yet at a critical point of personnel or resource commitment, a "take-off" can occur in policy output—yielding vastly multiplied gains in goal-seeking performance.

The need to breach thresholds and to maintain an organizational transcendence of these limiting factors, places severe constraints on the "divisibility" of the nonincremental policy enterprise. It is likely that urban transportation policies frequently fall victim to their own indivisibility requirements. Rapid transit systems in particular possess underlying scale demands—they must attain a requisite coverage or they simply cannot cope with traffic densities in sufficient degree to justify their costs. Additionally, these systems require firm commitments to detailed plans, and

<sup>59</sup> For a discussion of the urban decay spiral see William J. Baumol, "Macroeconomics and Unbalanced Growth: The Anatomy of Urban Crisis," *American Economic Review*, 57 (June, 1967), 415–426. Also, Harry W. Richardson, *Urban Economics* (London: Penguin Books, 1971), pp. 133–145.

<sup>61</sup> For an analysis of these potential "accelerator effects" in urban renewal see Wilbur R. Thompson, *A Preface to Urban Economics* (Baltimore: The Johns Hopkins Press, 1965), pp. 299–302.

<sup>59</sup> Kenneth Boulding, "Discussion," in "The Political Economy of Environmental Quality," *American Economic Review*, 61 (May, 1971), 167.

an early construction start-up before labor and materials costs rise prohibitively, and before land acquisition and rezoning resistance coalesces.

Given these requirements, it is easy to understand why, despite feasibility studies, campaign promises, and the establishment of urban transportation planning agencies, urban transport policy remains underdeveloped. Rapid transit systems, because of their indivisibility characteristics, acquire a rigidity which will not permit them to surmount successfully the compromise, delay, and reduction processes of urban political bargaining. These are essentially processes of *disaggregation*, and they push transportation ventures below those critical thresholds upon which they rely for success.

Indivisibility of the same type can also afflict large-scale planning efforts in general. Plans, after all, create their own support requirements and must generate political commitments and public aspiration in order to fulfill them. Disaggregation can undermine the base of support required for adequate plan implementation. It can seriously disrupt the internal "logic" upon which the plan must rely for its persuasiveness. In this connection Gerhard Colm has argued that "it is very important to recognize that a one-year plan cannot be obtained merely by dividing, say, a five-year plan into five equal parts."<sup>62</sup>

Indivisibility, of course, leads to a major vulnerability in the nonincremental policy enterprise. To a large extent its success depends on the degree to which it can be shielded from the ever-present forces of political disaggregation. Yet this very shielding requirement contains enormous political implications.

#### Nonincremental Policy and Its Political Consequences

It often happens that nonincremental policy managers, painfully aware of the vulnerabilities to which their programs are subject, will go to great lengths to establish congenial political environments. Frequently, a nonincremental policy will be "oversold" to the public in order to gain the support and resources deemed essential in the overcoming of thresholds. Once oversold, it becomes difficult to modify the basic objectives of the policy without threatening the political foundations upon which its support has been based. For manned space exploration the Kennedy lunar landing commitment became the major sustaining but at the same time the major constraining factor with which space policy makers had to deal.

Yet this overselling effect is not confined to space exploration alone. Theodore Lowi has noted an identical tendency in large-scale foreign policy undertakings as well. The war in Vietnam, Lowi argues, was rigidified by the very way in which it was sold to the public:

No policy has escaped injury to itself and to national interests and international stability in the years since American statesmen have felt the need to oversell policies *in order to avoid coming up with a partial decision*. The war in Vietnam has been just another instance of the point. The fighting in the South was not of our making. The crisis was. The escalation was. The involvement in Vietnam was sold by American image-makers as a case of unambiguous aggression and therefore of the need for military victory. Perhaps it was both of these things, but to sell it on the front pages that way in order to ensure support at home left world diplomats, including our own, *with almost no options*.<sup>63</sup>

If Lowi's point is true, it hints strongly at major rigidities implicit in nonincremental foreign policies. The perceived need to accommodate thresholds (i.e., to avoid "partial decisions") can lock foreign policy makers into unyielding, "all-or-nothing" commitments.

This rigidity has major consequences for all policies of a nonincremental nature. It renders them resistant to the compromise and adjustment processes of political bargaining—frequently the major means by which public accountability is imposed. At the same time, their rigidity places nonincremental policies at a disadvantage in responding to shifting political coalitions—a demand essential to the maintenance of public support.

Compounding these difficulties is the instability of nonincremental policy itself. As mentioned earlier, policies in this class are unable to balance themselves within resource or aspirational steady-states. Because of this, a twofold political problem presents itself. In what way besides overselling are policy makers to ensure the requisite support for their undertakings? Without overselling, thresholds can render nonincremental policy so vulnerable to political "adjustments" that it exceeds existing capacities for bureaucratic support. Policy managers, in this case, are likely to experience a continuing frustration in starting up their programs and in mobilizing sufficient resources to offer an adequate chance of goal attainment.

The second half of the political dilemma is that of *disengaging* from rigid, nonincremental policy endeavors which have entered into a self-escalating spiral of growth. Disengagement is difficult

<sup>62</sup> Gerhard Colm, *Integration of National Planning and Budgeting* (Washington, D.C.: National Planning Association, 1965), p. 24.

<sup>63</sup> Theodore J. Lowi, *The End of Liberalism*, p. 179 (emphasis added).

to accomplish because of heightened aspirations and bureaucratic forces which support such spirals. (It is also likely if accomplished to lead to serious organizational perturbations as a concomitant retrenchment cycle sets in.) The war in Vietnam would appear to illustrate this disengagement dilemma to a frightening degree.

### The Inapplicability of Trial Programs

Another policy implication attached to non-incremental, indivisible undertakings is their challenge to the ever-popular limited-scope trial program. These programs are frequently employed in policy settings because, in the event of failure, not too much in the way of prestige or organizational resources has been risked in them. If they succeed, they provide a blueprint for adjustments in design and an enlargement of application. These advantages have led many policy makers to urge that we "expand the area of governmental and public affairs activity in which new ideas can be tried out as limited-scale programs."<sup>64</sup>

If nothing else, this analysis of nonincremental policy suggests that trial programs can have severe limitations. It suggests that small-scale policy efforts may not necessarily replicate larger ones. Trial programs cannot hope to duplicate those critical commitment and resource thresholds upon which nonincremental, indivisible policy pursuits must rely.

As a result, limited-scale programs are likely to be seriously misleading indicators of nonincremental policy performance. Such programs may indicate a potential for success which is really mythical where larger forces must come into play. Perhaps more importantly, they may project failure where success is possible, given resource commitments on the requisite scale.

All of these policy implications hint strongly at the need for new analytical attention to non-incremental policy, and particularly the need to identify, prior to policy commitment, *which public undertakings are likely to have thresholds attached*. This identification might allow for more enlightened political decisions and the subsequent design of more congenial organizational environments should commitments to nonincremental policies be undertaken. In the meantime, however, no such analytical imagination appears likely until alternatives to the incremental and divisibility

paradigms which dominate policy analysis are presented.

### Nonincremental Policy: The Conceptual Challenge

To review briefly, this essay has described a class of policy undertakings which elude much of the analytical weaponry of political science. Non-incremental policies require large start-up investments unfulfillable by incremental or piecemeal commitments. They are policies which are non-equilibrating with respect to growth—that is, they are devoid of middle ground between expansion and decay. Lindblom, in his description of incrementalism, stresses the importance of marginal and continuous decisional refinements to the policy process. Yet the policies of which we have been speaking are precisely those for which this strategy is inappropriate. They cannot attain the equilibrium *which would allow these adjustable actions to be successful*.

It is important to recognize the extent to which incremental and equilibrating outlooks dominate political science. Policy analysis repeatedly stresses the degree to which marginal balances are struck between bureaucrat and client, between policy goals and public pressure, and between bureaucratic decisions of the present and organizational routines of the past.

In addition, many of the emerging trends in the analysis of public policy lead in the direction of further elevating decentralized, divisible processes as the "stuff" of policy making. The "public choice" school of policy analysis applies economic models to its assessment of policy activity. In effect, this approach assumes the existence of a decision "market" in which the disaggregated forces of competition, bargaining, and exchange are the determining factors in policy output. Related to this analytical outlook is a developing political persuasion which endorses the increased decentralization of public decision making.

Perhaps the best argument for this public policy "approach" has been presented by Alan Altshuler. Altshuler asserts that it is now necessary "to think more systematically about the virtues of disaggregation versus integration, pluralism versus coordination, and the free market versus regulation in social life."<sup>65</sup> Altshuler calls for a "debureaucratization" in public policy—wherein bureaucratic decisions are supplanted by popular decisions (as in citizen participation and community control) and by market decisions (in which a market adjustment of individual prefer-

<sup>64</sup> Adam Yarmolinsky, "Ideas Into Programs," in *The Presidential Advisory System*, ed. Thomas E. Cronin and Sanford D. Greenberg (New York: Harper and Row, 1969), p. 99. For a further discussion of experimental policy making see Alice M. Rivlin, *Systematic Thinking for Social Action* (Washington, D.C.: The Brookings Institution, 1971), pp. 108-119.

<sup>65</sup> Alan Altshuler, "New Institutions to Serve the Individual," in *Environment and Policy: The Next Fifty Years*, ed. William R. Ewald (Bloomington: Indiana University Press, 1968), p. 425.

ences, such as in voucher plans, replaces centralized efforts at planning or control).

There is obviously a great deal to be said in behalf of the public choice and decentralization movements in public policy. Yet there is a real danger, again, that such outlooks will command a disproportionate amount of both analytical attention *and* political support—at the expense of important nonincremental and indivisible policy objectives to which thresholds are attached. This is a problem of theoretical and practical dimensions.

In political theory at present, decision-making models as well as most models of the political system itself, describe deviance-minimizing, self-stabilizing, and equilibrating operations exclusively. Yet as we have seen, nonincremental policy frequently entails a high-level, unstable process of deviance *amplification*—a performance “take-off” based upon systems of mutual causation.<sup>66</sup> Entirely new models must be fashioned to account for deviance-amplifying processes before we can hope to analyze enterprises like manned space exploration adequately.

Meanwhile, a deep disillusionment has come to surround the public's assessment of nonincremental political commitments to large-scale policy. In this era of skepticism little support exists for ambitious and expensive undertakings in the

public sector. Peculiarly, however, interest in large-scale *goals* remains; and herein lies a major dilemma. Solutions are still urgently sought to problems of poverty, unemployment, and urban decay. Demands continue for major improvements in transportation, housing, energy supply, and environmental quality. These are large-scale demands. Yet the decentralization movement may force us to rely for their resolution upon small-scale, disaggregated policy efforts.

It may well be that many of these efforts will be successful. But the danger exists that in a number of problem areas, incremental policy attempts will fail repeatedly as each falls below some critical effectiveness threshold. Perhaps only nonincremental modes of policy making will prove appropriate in satisfying many of our implicit societal aspirations. The decentralization movement threatens to undercut seriously the political support upon which such nonincremental enterprises would have to depend.

The point of this essay is not to attack the public choice or decentralization movements in public administration, nor even to challenge the utility of the incremental model of policy making. The purpose of this essay is to argue for *additional* analytical frameworks to account for phenomena which lie outside present theoretical coverage. New departures in policy analysis are called for if we are to understand many large-scale public undertakings and their problems. Such understanding is important—both to the development of political science and to the success of public policy.

<sup>66</sup> For an excellent discussion of the deviance-amplification process see Margoroh Maruyama, “The Second Cybernetics: Deviance-Amplifying Mutual Causal Processes,” in *Modern Systems, Research for the Behavioral Scientist*, ed. Walter Buckley (Chicago: Aldine Press, 1968), pp. 304–313.

## ■ The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies\*

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In the same month of 1960 two articles were published on children's orientations toward the President of the United States. One, by Easton and Hess,<sup>1</sup> reported fixed-choice, pre-test data on white, suburban, midwestern children's responses to the questionnaire these researchers later administered to a national sample of white urban children during the Kennedy Administration.<sup>2</sup> The other, my paper on "The Benevolent Leader,"<sup>3</sup> reported mainly open-ended data on a heterogeneous but largely white sample of New Haven, Connecticut, children. The findings of these two independent Eisenhower-years studies as well as Easton and Hess's Kennedy-years findings converged strikingly. American children appeared to have highly idealized views of the President.

Since 1960, the populations of children studied in the early political socialization research have increasingly been exposed to a political culture in which the themes of cynicism and distrust of political leaders are widely prevalent. Moreover,

investigators have gone on to study subcultural variation in pre-adult political orientation within the United States, not least because such groups as blacks, Puerto Ricans, Chicanos, American Indians and poor Appalachian whites have become more self-consciously politicized, and have articulated their disaffection with "the system." A further sense of the potential variation in pre-adult political orientations has emerged from the growing body of research on children in numerous political systems. To complicate matters, the very phrase "political socialization," which was then a neologism, has since triggered several theoretical and methodological debates about the nature, consequences, and appropriate observation methods of pre-adult political learning.<sup>4</sup>

This paper presents data on children's orientations toward political leaders in a paradigmatically interesting triad of western democracies—Britain, France, and the United States. The data source consists of the protocols of tape-recorded interviews with small samples of children in the three countries, which were submitted to intensive content analysis in order to examine various standard assumptions about political culture and socialization among the three nations, as well as black-white differences in the United States. The interviews were collected in 1969–70—i.e., in Britain at a time when Harold Wilson was in office, somewhat preceding the interlude of Heath's prime ministership; in late-de Gaulle, early-Pompidou France; and in the early stages of Nixon's first term in office. In addition, a small sample of American children was interviewed during the June 1973 Watergate Hearings.

This paper describes in detail responses to two types of open-ended questions about the heads of state in the three countries and the Prime Ministers in Britain and France, and discusses these responses in the light of the contemporary political socialization literature, with special emphasis on the current applicability of the benevolent-leader findings on white American children at the beginning of the 1960s. My second concern is to calibrate different measures of pre-adult political orientation in an attempt to clarify certain theoretical and methodological issues that have arisen in the political socialization literature.

\* For references see notes 9 and 11 below.

\* This is a revision and condensation of a paper delivered at the 1973 Annual Meeting of the American Political Science Association. (Copyright 1973, The American Political Science Association.) For the original paper, see "Children's Images of Political Leaders in Three Democracies: The Benevolent Leader Revisited." (Ann Arbor: Xerox University Microfilms). The data reported are from a collaborative study designed and executed by the writer and Sidney Tarrow. Professor Tarrow's comments on the entire exposition have been of great help. I would like to thank Mary Williams, Richard Kronick, Leslie Margolin, and Barbara Young for research assistance well beyond the mechanical level, and the National Science Foundation, the Social Science Research Council, the Ford Foundation, and the Princeton Center of International Studies, and the Yale Urban Studies Program for financial aid at various stages of the project.

<sup>1</sup> Robert D. Hess and David Easton, "The Child's Changing Image of the President," *Public Opinion Quarterly*, Vol. 24 (Winter, 1960), pp. 632–44.

<sup>2</sup> The two book-length reports of that study are Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago: Aldine, 1967) and David Easton and Jack Dennis, *Children in the Political System: Origins of Political Legitimacy* (New York: McGraw-Hill, 1969).

<sup>3</sup> Fred I. Greenstein, "The Benevolent Leader: Children's Images of Political Authority," *American Political Science Review*, 54 (December, 1960), 934–43, later expanded on in Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), chapter 3. Also see Fred I. Greenstein, "More on Children's Images of the President," *Public Opinion Quarterly*, 25 (Winter, 1961), 648–54.

### Political Culture and Socialization in Britain, France and the United States

The following widely held or implied assumptions are what might be called the textbook theory of politics and society in the three nations with which this study is concerned.<sup>5</sup>

(1) Britain and the United States are "stable democracies."

(2) France is an "unstable democracy" marked by widespread distrust of political leaders and internal cleavage.

(3) Britain and the United States differ substantially in social structure and political psychology. Britons traditionally accept the legitimacy of political authority, whereas Americans are ambivalent, though not wholly negative, toward their leaders.

(4) Thus, the United States exhibits an uneasy compromise between French- and British-like properties. Indeed, if the United States were organized in the centralized fashion permitted by British social structure and values, unstable, French-style outcomes would result.

(5) American politics are more centrifugal than British politics, partly because of the greater social heterogeneity of the U.S., and particularly the greater number of potentially antagonistic groups. The single most enduring element of this politically explosive social heterogeneity results from the heritage of slavery. The presence of "two nations" in the United States has been a fundamental source of political conflict from the framing of the Constitution through the 1970s, by which time American blacks had become increasingly alienated and racially proud. By the 1970s, blacks were direct protagonists in American politics, and not merely the objects of political conflict and machination.

These deliberately simplified textbook claims imply assumptions about political socialization that would place white American children between English and French children in their evaluations of political leaders, but would predict that

these aspects of political psychology do not take the same form among black and white American children. Blacks presumably would resemble the French in their distrust of their nation's leaders; whites would be more like the British children in their trust of leaders.

"The literature" speaks with many voices, however. Recent writings contain numerous observations and assertions that contradict the foregoing sketch, partly because of reassessment of the traditional assumptions. Here are some brief illustrations of how the conventional wisdom has altered with respect to the four comparison groups in this study and also the study of political socialization in general:

(1) A principal debate in the recent literature on English political culture and socialization is whether the Bagehot-derived presumption that the "English common people," are "deferential" to higher authorities stands up empirically. Indeed, important questions have been raised about whether the very notion of deference has been adequately explicated in the literature.<sup>6</sup>

(2) The emphasis of Gallic negativism toward political authority in the textbook account of French political culture has a corollary in the notion that the French populace consists of highly politicized citizens, each of whom is deeply committed to his own preferences among the panoply of subtly nuanced French political ideologies. In contrast to the "Frenchman-as-ideologue," recent studies have found an exceptionally apolitical (possibly antipolitical) French electorate that provides few restraints on the impulses of the thin political stratum of the populace to vie over what Freud called "the narcissism of small differences." Some interpretations of French politics also stress the putative "individualism" of the French, but other writings stress the importance of hierarchical, even autocratic, authority relations in French politics and society, more like the conventional

<sup>5</sup> For a recent annotated presentation of standard sources on Britain and France, see the bibliography to Stanley Rothman, *European Society and Politics* (Indianapolis: Bobbs-Merrill, 1970). On the United States, see Michael McGiffert, ed., *The Character of Americans*, 2nd ed. (Homewood, Ill.: Dorsey, 1973), which contains a useful collection of essays and valuable bibliographical references. The older national character literature is summarized in: Alex Inkeles and Daniel Levinson, "National Character: The Study of Modal Personality and Sociocultural Systems," in G. Linzey and E. Aronson, eds., *Handbook of Social Psychology IV* (Reading, Mass.: Addison-Wesley, 1968), pp. 418-506. On the more recent political-cultural approach see Lucian W. Pye and Sidney Verba, eds., *Culture and Political Development* (Princeton, N.J.: Princeton University Press, 1965).

<sup>6</sup> Dennis Kavanagh, "The Deferential English: A Comparative Critique," *Government and Opposition*, 6 (Summer, 1971), pp. 333-60. The question of deference will not be discussed further in this article because it is treated extensively in Fred I. Greenstein, Valentine Herman, Robert N. Stradling and Elia Zureik, "The Child's Conception of the Queen and the Prime Minister," *British Journal of Political Science*, 4 (July, 1974), 257-87. After that article was in print, we discovered technical errors in the data processing that shifted the percentages by one or two points in a number of tables as well as a substantial difference in the percentages reported for white American children and French children in rows 1 and 2 of Table 9, and for black American children in rows 1 and 2 of Table 7 in this paper. All the statistics in this paper have been rechecked to eliminate any further possibility of errors and discrepancies between the two articles, due to the errors noted above in the earlier article.



stereotype of "the Germans" than of "the French." After all, runs the *mot*, "Charlemagne was also Karl der Grosse."<sup>7</sup>

(3) The empirical studies of American adults conducted up to the early 1960s seem to suggest that the positive aspects of adult ambivalence toward leaders is considerably more consequential for political behavior than are negative attitudes such as generalized distrust for politicians.<sup>8</sup> As we have seen, the early studies of American children show almost unequivocally positive references to the "benevolence" of political leaders.<sup>9</sup> This early "benevolent leader literature" has since been referred to as an "irrelevant" characterization of a bygone era.<sup>10</sup> After all, this literature preceded the Vietnam protests, ghetto insurrections, increasing signs of deeper, ideological conflict and distrust of authorities in the adult population, protest activities by college and high school students during Johnson's final years in office, and, after the apparent calm of Nixon's first term, Watergate and its aftermath. That certain of these trends even occurred raises important questions about the value of the benevolent-leader literature. As more than one commentator has noted, some of the same pre-adolescents who blithely idealized

Eisenhower and Kennedy in the early studies may well have been the leaders of protest against "the system" a few years later.<sup>11</sup>

In the second wave of mid- to late-1960s political socialization research, an important study of highly distinctive, culturally and physically isolated population of poor whites in Appalachia was instructively titled "The Malevolent Leader."<sup>12</sup> To some extent this title came to represent the expectation that in the post-Eisenhower, post-New Frontier-Great Society era, *all* segments of the pre-adult American population were likely to acquire political orientations lacking the idealized qualities reported in the research a decade earlier. Another study by Tolley in 1971 of the attitudes of grade-school children toward the Vietnam conflict reported many indications of children's actual or hypothetical willingness to criticize the President for fostering military conflict. Tolley, who summarized his findings under the heading "The Fallible Leader, 1971," noted that although his data were not comparable to the data of the early studies, he felt sure there had been a reduction in children's idealization of the American President.<sup>13</sup>

(4) Substantial changes in the political consciousness and behavior of American blacks have occurred since the 1950s. Evidence exists that blacks at all age levels exhibit political negativism, distrust, and cynicism, but findings on this score are far from consistent, perhaps because race relations and "the black experience" in the United States, in spite of certain overriding uniformities, are highly variable from one context to another.

<sup>7</sup> Relevant sources are reviewed in Fred I. Greenstein and Sidney Tarrow, "The Study of French Political Socialization; Toward the Revocation of Paradox," *World Politics*, 22 (October, 1969), 95-137.

<sup>8</sup> For a review of the literature on adult ambivalence toward political authorities in the United States, see Fred I. Greenstein, *Children and Politics*, pp. 27-31.

<sup>9</sup> See the references in notes one, two and three and the following useful anthologies: Norman Adler and Charles Harrington, eds., *The Learning of Political Behavior* (Glenview, Illinois: Scott Foresman, 1970); Roberta S. Sigel, ed., *Learning About Politics: A Reader in Political Socialization* (New York: Random House, 1970); Jack Dennis, ed., *Socialization to Politics* (New York: Wiley, 1973); Charles G. Bell, ed., *Growth and Change: A Reader in Political Socialization* (Encino and Belmont, Cal.: Dickenson, 1973); and Anthony Orum, ed., *The Seeds of Politics: Youth and Politics in America* (Englewood Cliffs, N.J.: Prentice-Hall, 1972). The first and third of these works have excellent bibliographies. Recent extensive literature reviews include Michael P. Riccards, *The Making of the American Citizenry: An Introduction* (New York: Chandler, 1973); Dean Jaros, *Socialization to Politics* (New York: Praeger, 1973); Robert Weissberg, *Political Learning, Choice and Citizenship* (Englewood Cliffs, N.J.: Prentice-Hall, 1974); and David O. Sears, "Political Socialization," in *The Handbook of Political Science II*, ed. Fred I. Greenstein and Nelson W. Polsby (Reading, Mass.: Addison Wesley, 1975), pp. 93-154. For a recent bibliography, see Jack Dennis, "Political Socialization Research: A Bibliography," *Sage Professional Papers in American Politics*, Vol. 1, Series No. 04-002 (Beverly Hills: Sage Publications, 1973).

<sup>10</sup> W. Lee Johnson, Jr., Letter to the Editor, *American Political Science Review*, 66 (December, 1972), 1317-1318.

<sup>11</sup> On the question of whether the positive orientations of children studied in the late 1950s and early 1960s should in principle have been expected to predict "pro-system" behavior, see Fred I. Greenstein, "A Note on the Ambiguity of 'Political Socialization': Definitions, Criticisms, and Strategies of Inquiry," *Journal of Politics*, 32 (November, 1970), 969-78.

<sup>12</sup> Dean Jaros, Herbert Hirsch, Frederic J. Fleron, Jr., "The Malevolent Leader: Political Socialization in an American Subculture," *American Political Science Review*, 62 (June, 1968), 564-75.

<sup>13</sup> Howard Tolley, Jr., *Children and War: Socialization to International Conflict* (New York: Teachers College Press, 1973), pp. 129-31. Recently, a "multifarious leader" has been introduced into the literature by a pair of investigators who compare black and white American children with a President-worshipping sample of Amish children. Dean Jaros and Kenneth L. Kolson, "The Multifarious Leader: Political Socialization of Amish, 'Yanks', Blacks," in Richard G. Niemi and Associates, *The Politics of Future Citizens* (San Francisco: Jossey-Bass, 1974), pp. 41-62. For an exceptionally interesting study of the absence of conspicuous leadership, figures in post-World War II Japanese political socialization, see Joseph A. Massey, "The Missing Leader: Japanese Youths' View of Political Authority," *American Political Science Review*, 69 (March, 1975), 31-48.

In addition, black political orientations probably have a distinctive subcultural patterning, which needs to be characterized in its own terms and not merely as part of a continuum of French, American white, and English orientations.<sup>14</sup>

(5) One may also speculate about the adequacy *in general* of the empirical characterizations of pre-adult (and adult) political orientations toward leaders. Political scientists interested in political socialization have been relentlessly monistic in their use of paper-and-pencil surveys, especially fixed-choice questionnaires, in studying children. As a result, questions have been raised about the possible artifactuality of the early political socialization research. The same fixed-choice response can have different meanings to different respondents, and individuals whose attitudes are essentially the same sometimes choose different responses depending on how they perceive item wordings.<sup>15</sup> Worse yet, a choice may be made more or less arbitrarily because it sounds right or because of the respondent's desire to cooperate. These dangers are increased when such techniques are applied to young children who may not even recognize the terminology (e.g., the very word "government"), much less the rationale of an item such as the following from the Easton and Hess questionnaire: "What happens in the government will happen no matter what people do."<sup>16</sup> When fixed-choice procedures are used cross-

culturally, the possibility for misleading findings is multiplied, a point that emerges strongly from British critiques of an application of the Easton and Hess instrument to an English pre-adult population.<sup>17</sup>

Even where open-ended approaches are used, problems of interpreting responses persist. In my New Haven study, the open-ended format made it possible to qualify or counter some interpretations suggested by the fixed-choice Easton and Hess study.<sup>18</sup> The final data collection was less than ideal, however, because a paper-and-pencil procedure was used. Even if interviewing had been possible it would still have been difficult to elicit responses without suggesting or structuring them, and to interpret the resulting verbiage. These problems of interpretation exist especially in characterizing children's orientations as "benevolent." Children, or at least American children, are rather free with words like "help" in describing political leaders. But what do children mean when they say a leader is "a helper?" Are they exhibiting basic assumptions about the benevolence of leaders? Or are they merely mouthing a bit of conventional language? In either case, to what degree are their statements

<sup>14</sup> On the politicization of contemporary black children in the United States see the following recent articles: Paul R. Abramson, "Political Efficacy and Political Trust Among Black School Children: Two Explanations," *Journal of Politics*, 34 (November, 1972), 1243-75; Anthony M. Orum and Roberta S. Cohen, "The Development of Political Orientations Among Black and White Children," *American Sociological Review*, 38 (February, 1973), 62-74; and Sarah F. Liebschutz and Richard G. Niemi, "Political Attitudes Among Black Children," in Niemi and Associates, pp. 83-103. On the context-dependence of attitudes associated with race, see Howard Schuman and Barry Gruenberg, "The Impact of City on Racial Attitudes," *American Journal of Sociology*, 76 (September, 1970), 213-61. On blacks more generally, see Norval D. Glenn and Charles M. Bonjean, eds., *Blacks in the United States* (San Francisco: Chandler, 1969); Talcott Parsons and Kenneth B. Clark, eds., *The Negro American* (Boston: Houghton-Mifflin, 1966); Dorothy B. Porter, *The Negro in the United States: A Selected Bibliography* (Washington: Library of Congress, 1970).

<sup>15</sup> For a classical demonstration of the extraordinarily different reasons why individuals may choose a fixed-choice alternative, see Richard S. Crutchfield and Donald A. Gordon, "Variations in Respondents' Interpretations of an Opinion-Poll Question," *International Journal of Opinion and Attitude Research*, 1 (1947), 1-12.

<sup>16</sup> Problems of using fixed-choice items in research on children are discussed in David O. Sears' review of Hess and Torney, *The Development of Political Attitudes in Children*, in *Harvard Educational Review*,

38 (Summer, 1968), 571-78. Also see Fred I. Greenstein's review of Easton and Dennis, *Children in the Political System: Origins of Political Legitimacy*, in *Political Science Quarterly*, 87 (March, 1972), 98-102; and Fred I. Greenstein and Sidney Tarrow, "Political Orientations of Children: The Use of a Semi-Projective Technique in Three Nations," *Sage Professional Papers in Comparative Politics*, Vol. 1, Series No. 01-009 (Beverly Hills: Sage Publications, 1970). For a somewhat overstated but interesting critique of the use of questionnaires in general, see David Marsh, "Political Socialization: The Implicit Assumptions Questioned," *British Journal of Political Science*, 1 (April, 1971), 453-66. Another line of critical discussion of political socialization studies is presented in Donald D. Searing, Joel J. Schwartz, and Alden E. Lind, "The Structuring Principle: Political Socialization and Belief Systems," *American Political Science Review*, 67 (June, 1973), 415-32. See also two extensive rejoinders to this essay in the *American Political Science Review*, 68 (June, 1974), 720-25.

<sup>17</sup> The object of the critiques was the following article: Jack Dennis, Leon Lindberg, and Donald McCrone, "Support for Nation and Government Among English Children," *British Journal of Political Science*, 1 (April, 1971), 25-48. The critiques: March, "Political Socialization: The Implicit Assumptions Questioned"; A. H. Birch, "Children's Attitudes and British Politics," *British Journal of Political Science*, 1 (October, 1971), 519-20; Ian Budge, "Support for Nation and Government Among English Children: A Comment," *British Journal of Political Science*, 1 (July 1971), 389-92; David Marsh, "Beliefs About Democracy Among English Adolescents: What Significance Are They?" *British Journal of Political Science*, 2 (April, 1972), 255-59.

<sup>18</sup> Greenstein, "More Children's Images of the President."

affective and evaluative, and to what degree are they cognitive?<sup>19</sup>

One reason for suspecting the accuracy of political socialization findings that stress reasonably well-patterned pre-adult political orientations is that survey research on adults has reported remarkably low levels of information and attitudinal consistency. The conclusions of the literature on lack of patterning in adult orientations<sup>20</sup> have since been softened both by events (the evident increase in attitudinal patterning as the "polarizing" political conflict of the 1960s proceeded), and by variant approaches to analyzing survey data on adults (e.g., approaches that emphasize the issues salient to an individual).<sup>21</sup> Furthermore, it seems desirable to examine adult orientations in ways that do not force a respondent to answer questions about the issues currently concerning the active minority of the political leadership stratum. Since survey items often use *categories of discourse* that characterize "elite" rather than "mass" political thought and discussion, more "naturalistic" modes of eliciting political orientation might possibly show *greater* patterning in adult orientations than is presently reported, and *less*, or at any rate, different patterning in pre-adult orientations.

#### Methodological Premises and Procedures

Although the methodological problems just discussed might seem to point to a search for a philosopher's stone that would make possible "perfect" measurement of political orientations, a different strategy is called for: that of multiple indicators and, in particular, *multiple methods*. As Campbell and Fiske point out in a classic paper still insufficiently appreciated by political scientists, it is axiomatic that particular measurement

approaches contain "biases." The most efficacious research strategy is to take advantage of, rather than seek to eliminate, these biases by employing compensating types of observational techniques, which produce instrument effects in varying but known directions. The findings elicited by different measurement techniques can be compared by a process analogous to triangulation<sup>22</sup> as it is used in surveying. The multimethod research procedure used in the present study enables us to examine the convergences and divergences among findings of four types: conventional fixed-choice items, open-ended questions, semi-projective story completions, and verbatim response quotations, but in this paper we draw selectively only on the latter three classes of data.

The open-ended items were designed to elicit childrens perceptions of major roles and institutions in their political system. These questions were similar to those used, for example, in the University of Michigan Survey Research Center's election studies, but employed a more varied set of codes to analyze responses. The sequence of information items was somewhat unconventional because it was preceded by this statement:

A new child comes to your school. He comes from another country. He says to you: "There are some things about [England, France, the United States] that I don't understand. Tell me what they are. . . ."

The "things" proved to be "the Queen," "the Prime Minister," "the President of the Republic," and other political roles and institutions.<sup>23</sup> The simple expedient of placing the child in a free-response circumstance sometimes elicited findings both contrary to and more accurate than those

<sup>19</sup> Connell, on the basis of his own open-ended interviews with Australian children, questions whether the pre-adult assertions I treated as exhibiting benevolence do not have a "cognitive . . . rather than emotional . . . basis." R. W. Connell, *The Child's Construction of Politics* (Melbourne, Australia: Melbourne University Press, 1971). As will be seen, the present analysis examines this possibility in detail. A far more complex question, which I will not attempt to deal with here, is whether the clearly demarcated gulf between fact and value in the purer varieties of analytic philosophy approximates the empirical processes of human thought, perception, feeling, judgment, and anticipation of action.

<sup>20</sup> Philip E. Converse, "Attitudes and Non-Attitudes: Continuation of a Dialogue," in *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte (Reading, Mass.: Addison-Wesley, 1970), pp. 168-89.

<sup>21</sup> See, for example, David E. Repass, "Issue Salience and Party Choice," *American Political Science Review*, 65 (June, 1971), 389-400, and Gerald M. Pomper, "From Confusion to Clarity: Issues and American Voters, 1956-1968," *American Political Science Review*, 66 (June, 1972), 415-28, 450-67.

<sup>22</sup> Donald T. Campbell and Donald W. Fiske, "Convergent and Discriminant Validation by Multitrait-Multimethod Matrix," *Psychological Bulletin*, 56 (March, 1959), 81-105. For a recent contribution to the literature perfecting the psychometrics of this approach, see Arne L. Kalleberg and James R. Kluegel, "Analysis of Multitrait-Multimethod Matrix: Some Limitations and an Alternative," *Journal of Applied Psychology*, 60 (February, 1975), 1-9. The term "triangulation" is first used in Eugene J. Webb, Donald T. Campbell, Richard D. Schwartz, and Lee Sechrest, *Unobtrusive Measures: Nonreactive Research in the Social Sciences* (Chicago: Rand McNally, 1961).

<sup>23</sup> The verbatim response content makes clear that most children ignore the hypothetical foreign child and treat the item as a simple request for cognitive information. A few French children were explicitly suspicious of the foreign child and describe the precautions they would take in discussing politics with a stranger; a few English children cast themselves as tour guides, exhibiting Buckingham Palace to the young foreigner. In no case did the foreign child stimulus invoke nationalistic idealizations of domestic leaders or institutions. For a text of the interview schedule, see Greenstein and Tarrow, "Political Orientations of Children: The Use of a Semi-Projective Technique in Three Nations," pp. 535-49.

reported in previous research using fixed-choice items. For example, fixed-choice political socialization research points to great respect for the Supreme Court. Interviews show similar positive responses, but make clear that they are to the positively toned words "supreme" and "court," and do not reflect awareness of the activities of the institution.

The most distinctive methodological departure was the use of numerous questions in which our respondents were presented with carefully contrived, incomplete stories and asked to imagine the conclusions, after the manner of the familiar Thematic Apperception Test (TAT). The procedure is called *semi-projective* because it employs more culturally recognizable and less ambiguous stimuli than those used in orthodox projective tests, and the responses are analyzed in terms of surface psychocultural dispositions, such as cognitive and evaluative assumptions about political reality, rather than in terms of deep strata of the personality.

Unlike some projective testers, I do not argue that there is a uniquely penetrating quality to this type of procedure—a kind of psychic X-ray—but instead, that it is a *different* way of eliciting political orientations than the procedures described in the previous paragraphs, and therefore contributes to the triangulation of observational methods discussed above. An exceptionally interesting property of the story-completion procedure is that it can provide an insight not only into cognitive and affective orientations but also into *conative* orientations—i.e., into dispositions toward action in concrete situations.<sup>24</sup> The analysis that follows is devoted to contrasting the evidence on affect toward political leaders produced by an open-ended item in which the respondent describes national leaders to the imaginary foreign child, and a semi-projective item in which the respondent is asked to imagine the outcome of an episode in which the head of state's car is inadvertently stopped for speeding by a traffic policeman.

Throughout this report I also present numerous examples of verbatim responses because of a con-

viction that code categories, no matter how carefully and subtly conceived, fail to exhaust the meanings of the respondents' statements. The quotations show that children in the various comparison groups who responded in the same way in certain respects—and were so coded—nevertheless differed in still others. While more specific coding can reduce these residual points of difference, the data suggest inherent limits to the aim of comparative political psychology, stated by Przeworski and Teune, to convert national variance into variance between universal categories.<sup>25</sup> As will be seen, some of the differences even seem to reside in the national languages of our respondents. Verbatim response quotations are also invaluable in seeking to define the criteria for concluding, for example, that certain specified political orientations are affective rather than cognitive. At a minimum, by providing direct quotations, we can furnish evidence for others to draw their own conclusions.

The data that follow are derived from thirty-to sixty-minute taped interviews with 297 ten-to fourteen-year-old children in four comparison groups of the following sizes: English, 80; French, 106; U.S. whites, 86; U.S. blacks, 25. The comparison groups are essentially evenly divided by sex. The interviews were conducted in geographically diverse schools, so far as was possible: Connecticut, Eastern Pennsylvania, and upstate New York in the United States; the Paris area and Provence in France; and London and East Anglia in Britain. To facilitate subgroup comparisons, the samples were deliberately stratified rather than seeking to follow population frequencies. Unfortunately, our resources did not permit a large number of black interviews in the United States, and consequently the black-white comparisons should be treated as provisional. By using a large number of schools and classrooms, and numerous interviewers for the English, French, and white American groups, possible measurement error resulting from individual interviewer style and classroom climate were reduced. Black children were interviewed by a black, female interviewer.

The interviews conducted during the Watergate hearings were with white Connecticut children from the same schools where the previous Connecticut white interviews had taken place. A slightly truncated interview guide was used.

<sup>24</sup> Semi-projective procedures do not elicit the same levels of psychological orientation from each individual. In some interviews, responses are patterned by underlying personality organization, but in others, responses are rather ephemeral and seem to be stimulated either by recent learning experiences such as classroom exercises, or even by stimuli contained in the interview itself. For this reason responses to story-completion items should be viewed more as patterns within populations than as stable properties of individual respondents. For a fuller discussion, see Greenstein and Tarrow, "Political Orientations of Children: The Use of a Semi-Projective Technique in Three Nations," pp. 510-13.

<sup>25</sup> Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970). Not only are variables imperfectly comparable across cultures, but there are important problems of their contextual meaning when associated with other constellations of factors in different cultures. Compare Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973), especially p. 23.

Although an attempt was made to match the groups by age, socioeconomic status, and community characteristics, some differences occurred. Analyses controlling for these factors made it clear that they were not responsible for the patterns in the tables to be presented. Sample size and space limitations preclude multivariate analysis in this presentation; instead marginals and error content are extensively discussed and compared. I have abjured significance testing, which would give an aura of spurious precision to a richly complex but obviously exploratory body of data.

### Children Explain Their Political Leaders and Institutions: Information, Images, and Affect

Tables 1 through 4 are based on the questions in which the respondent is asked to imagine a dialogue with a foreign child seeking information about respondent's country. Table 1 reports levels of information about the chief of state in each of the three nations and about the Prime Minister in Britain and France. To broaden the perspective on awareness of these five roles, information also is presented in Table 1 on awareness of the local member of the national legislature and the mayor but for the remainder of this analysis, only the chief of state and prime ministerial roles will be discussed.

At present there is a scattered array of findings about levels of awareness of these roles on the part of children and adults in the three countries. At all age levels, the President is certainly the best-known American,<sup>26</sup> and he is virtually the only public figure known to more than a small minority of children until late in adolescence.<sup>27</sup> Although both children and adults in the United States, when confronted with the appropriately worded questionnaire item, can be induced to say that the Congress is "more important" to the nation than the President, the available evidence suggests that at all age levels the President is considerably more salient than that Congress and the in-

dividual members of Congress.<sup>28</sup> A recent national study of children's awareness of their district congressman shows that by age thirteen only 11 percent can name that individual—indeed only about 40 per cent of young adults can.<sup>29</sup> Awareness of local officials appears to vary from locality to locality in the United States. In my 1958 political socialization study, virtually all New Haven children by the fifth-grade level knew the name of that city's popular Mayor Richard C. Lee, whereas in the nearby community of East Haven only 40 per cent of a fifth-grade pretest sample were able to name the municipal chief executive.<sup>30</sup>

Comparably detailed documentation does not appear to be available for either British or French adults and children. Roig and Billon-Grand do, however, provide evidence of the wide general awareness of the President of the Republic among French children,<sup>31</sup> and Greenstein, Herman, Stradling, and Zureik report data that provide some sense of how English children perceive the Queen and the Prime Minister.<sup>32</sup>

Virtually all the children professed to have heard of the four British and French and three American political roles in their respective countries. Professions of knowledge are easy, how-

<sup>26</sup> Easton and Dennis, *Children and the Political System*, pp. 114-21, present fixed-choice data suggesting that older children attribute greater importance to Congress than to the President. Open-ended data on children's perceptions of the President and Congress from the present study, however, leave the impression that while this may be a typical verbal convention learned by older children, even the oldest respondents in the present study knew very little about Congress, much less about their own congressman, and were far more attentive to and psychologically stirred by the President. On the simple matter of awareness of the President versus legislators and the legislature, see Table 1 of this article. Adult verbalizations about the relative importance of President and Congress are summarized in Donald J. Devine, *The Political Culture of the United States* (Boston: Little, Brown, 1972), pp. 155-63. Like older children, adults talk about the primacy of Congress, but are more likely to structure their political perceptions and affect around the President.

<sup>27</sup> See note 26.

<sup>28</sup> Greenstein, *Children and Politics*, p. 63n.

<sup>29</sup> C. Roig and F. Billon-Grand, *La socialisation politique des enfants* (Paris: Cahiers de la fondation nationale des sciences politiques 163, Armand Colin, 1968). For a summary in English of the findings of Roig and Billon-Grand, see Greenstein and Tarrow, "The Study of French Political Socialization." On the orientations of French adults to de Gaulle and the French presidency as manifested in the General, see Institut français d'opinion publique, *Les français et de Gaulle* (Plon, 1971).

<sup>32</sup> See Greenstein et al., "The Child's Conception of the Queen and the Prime Minister." On monarchy generally, see Paul Abramson and Ronald Inglehart, "The Development of Systemic Support in Four Western Democracies," *Comparative Political Studies*, 2 (January, 1969), 419-42.

<sup>26</sup> Fred I. Greenstein, "The Psychological Functions of the Presidency for Citizens," in *The American Presidency: Vital Center*, ed. Elmer Cornwall, Jr. (Glenview, Ill.: Scott Foresman, 1966) and, in somewhat different form, as "The Best Known American," *Trans-action*, 4 (November, 1966), 12-17. For a recent attempt to describe and explain the ebb and flow of presidential popularity, see John E. Mueller, *War, Presidents and Public Opinion* (New York: Wiley, 1973). For a 1970s review, see Fred I. Greenstein, "What the President Means to Americans: Presidential Choice Between Elections," in *Choosing the President*, ed. James David Barber (Englewood Cliffs, N.J.: Prentice-Hall, 1974), pp. 121-47.

<sup>27</sup> The survey findings are presented in Greenstein, "What the President Means to Americans: Presidential Choice Between Elections."

ever. They may reflect reluctance to admit ignorance; they may result from confusion ("Congress" for "Chamber of Commerce," for example); or they may be a mere recognition that the term *sounds* familiar. A more accurate assessment of the children's level of information can be found in Table 1, row 2, which indicates the frequency with which children were able to supply the names of role incumbents in response to an open-ended question. Because the French children were polled during the interval between de Gaulle's resignation and Pompidou's election, as well as during the early Pompidou period when there had been little time to absorb the President's name, we combined the figures for awareness of the present incumbent and awareness of the previous incumbent. Well over 90 per cent of the children in each nation were able to name their head of state. The somewhat lower incidence of awareness of the Queen's name in England reflects no lack of familiarity with the royal role, but instead a tendency to perceive "the Queen" rather than "Elizabeth II" as the designation of the Monarch.

This impression of uniformly high levels of political awareness quickly vanishes as one looks further into Table 1. Whereas almost every English child is able to name Harold Wilson, only about half the French children are able to name the Premier. An increase in the prominence of the Premier as a figure to whom French *adults* orient themselves as the politics of the Fifth Republic evolved has been argued by Jean-Luc Parodi<sup>33</sup> on the basis of time-series survey data, but at the pre-adult level the Premier appears to be at best a shadowy presence.

In all three countries, the legislative representative from the child's district is consistently less well known than any heads of state or prime ministers; only a quarter of the English, French, and U. S. white children and a mere 5 per cent of the U.S. black children were able to name their legislator.

The pattern of awareness of local executives strikingly suggests the importance of the mayoral role or its equivalent in the three political systems, a finding consistent with Roberta Sigel's observation that, in spite of the considerable affective component in early political learning and children's general inattention to government and politics, detailed, reasonably accurate cognitive learning about political roles and institutions *does* occur.<sup>34</sup> French children, brought up in a political

system with intensely localistic traditions in which *maire* and *mairie* are familiar fixtures, exhibit more information about the mayor and his job than do the children in the other two countries. The French lead the other comparison groups in local awareness substantially, whereas on the other leadership roles the differences among the French and the English and American are in the opposite direction. Seventy-one per cent of the French respondents and roughly half of the American respondents were able to name their mayors, but only one-tenth of the English respondents would name the largely ceremonial and decorative mayors of their cities.

**The Cognitive Content of Political Imagery.** We can usefully begin to look beneath the surface of the information-level distinctions in Table 1 by submitting role descriptions to an analysis of cognitive content. Table 2 presents twelve of the most substantively interesting image categories used in coding the political leadership roles, including only categories applicable to most or all of the roles and institutions. The categories evolved from repeated perusal of the open-ended data and, where appropriate, from the code used to classify the open-ended responses in my 1958 study.<sup>35</sup> The image categories do not exhaust the themes that can be extracted from the open-ended data, but they do represent some of the most salient surface aspects of the children's descriptions of their national leadership roles and institutions.

I begin with an extended examination of the first category listed in Table 2—the seemingly workaday theme that the leader "rules, governs, or commands." This category is consistently used for the head of state, but rarely for the prime ministers, even in Britain, where the ceremonial leader (the Queen) rather than the effective leader (the Prime Minister) receives most references to ruling.

In view of the extensive literature on the dilemma of achieving equivalent meanings in cross-cultural measurement, it is striking to note the national variation in linguistic usage and connotation when children refer to general governance. The imagery used by the American children suggests a leader who is an important person with a high degree of control over his political system. In some responses the President appears as the only important decision maker in the political

<sup>33</sup> Jean-Luc Parodi, "Sur deux courbes de popularité," *Revue française de science politique*, 21 (February, 1971), 129-214.

<sup>34</sup> Roberta S. Sigel, "Image of a President: Some Insights into Political Views of School Children," *American Political Science Review*, 62 (March, 1968), 216-26.

<sup>35</sup> The coding criteria and coded open-ended findings for my 1958 study are reported in Fred I. Greenstein, "Children's Political Perspectives," 1959, doctoral dissertation, Yale University (Ann Arbor: Xerox University Microfilms). Some open-ended political imagery findings were published in Greenstein, "More Children's Images of the President."

Table 1. Levels of Political Awareness<sup>a</sup>  
(percentages)

Response	England				France			U. S. Whites			U.S. Blacks		
	Queen	Prime Minister	Legislator	Mayor	President	Premier	Legislator	Mayor	President	Legislator	President	Legislator	Mayor
Claims to have heard of role	100	100	98	100	100	100	93	100	100	100	100	100	96
Can name role incumbent	91	96	25	10	68	50	24	71	100	27	96	5	40
Can name previous incumbent	94 3	96 0		0	99 31	52 2		73 2	0		0		0
Mean number of images	3.3	3.0	1.9 (1.4) <sup>b</sup>	2.0	2.7	1.5	1.5 (.81) <sup>b</sup>	2.0	3.1	1.7	2.7	1.6 (1.3) <sup>b</sup>	1.8
Respondents with 0 images	0	10	20	14	0	0	50	2	0	10	0	36	12

<sup>a</sup> Row 1 is based on the following number of cases: 80 English, 106 French, 86 U.S. whites, 25 U.S. blacks, except that 1 English child was not asked about the mayor, 3 French children were not asked about the Premier, 12 French children were not asked about the legislator, and 2 U.S. black children were not asked about the legislator. In row 2 the following number of respondents were excluded: head of state—England 2, France 1; prime minister—England 2, France 3; legislator—England 2, France 13, U.S. whites 9, U.S. blacks 4; mayor—England 7, France 7, U.S. whites 1. Mean number of images (rows 3 and 4) is computed from fourteen image categories that were common to all or most of the roles and institutions. Twelve of these categories are shown in Table 2. The image scores, like the image-mentioned percentages in Table 2, are based only on those respondents who were asked about the role or institution and showed awareness of it. The number of cases on which rows 3 and 4 are based is the same as the number for each role and institution in Table 3.

<sup>b</sup> Mean number of images for legislature.

Table 2. Images of Political Roles and Institutions (percentages)

Image Categories	England		France		U.S. Whites	U.S. Blacks
	Queen	Prime Minister	President	Premier	President	President
Rules, governs, commands	62	24	86	0	64	42
Legislative, legal function	23	31	14	3	51	29
Makes decisions, solves problems	15	25	18	3	35	29
Communicative (makes speeches, etc.)	1	13	6	5	10	32
Economic role	8	28	12	3	8	12
Other domestic activity	13	17	11	8	15	20
International activities	9	13	14	4	44	40
Ceremonial activities	53	19	11	5	16	0
Symbolic representative of nation <sup>a</sup>	19	—	3	—	5	4
Trappings of office	35	16	3	1	9	8
Role with other government officials	35	69	33	87	18	12
Personal attributes	16	7	2	1	1	0
N <sup>b</sup>	(71-79)	(68-76)	(101-104)	(96-97)	(79-86)	(24-25)

<sup>a</sup> Not coded for the Prime Minister.  
<sup>b</sup> Percentages based on those respondents familiar with the role, excluding those whose responses were coded as ambiguous.  
Coding Status: Inter-coder percentage agreement ranges from 89% (row 3) to 98% (rows 5, 6, 7, 10, 11), averaging 95%. Inter-coder agreement was also calculated by Scott's pi, which takes into account the random expected value of agreement given the number of coding categories. For an explanation of Scott's pi, see Ole R. Holsti, *Content Analysis in the Social Sciences and Humanities* (Reading, Mass.: Addison-Wesley, 1969), pp. 140-41. Pi values range from .66 (row 3) to .96 (row 11), averaging .83 (for the 60 cases double-coded).

system. Other children describe the constitutional checks and balances. In neither case do the responses of these children in 1969-70 suggest that the executive power of the leader is peremptory, frightening, or authoritarian, nor that he has absolute control. American children often say the President is "in charge," but quickly add that he cannot engage in extreme arbitrary behavior, for instance, capriciously ordering capital punishment. My impression is that when they add this qualification, they have in mind an implicit notion that at least in some other nations, leaders do have such power. The American respondents say, for example, that the President "governs," is the "ruler of the country," "leads the country," is "the leader of the people," "heads the U.S., "runs the United States," "runs the government," is the "chief executive," and, in one instance (without the connotations it would have to a lexicographer or a student of city political machines); is "the big boss."

Relatively few English children use the governance category in describing the Prime Minister, but many do in connection with the Queen. Their considerable use of verbs and nouns associated with monarchy is not surprising. Again, any connotation of sternness or oppressiveness in the wielding of power is notably absent. The Queen is

described as "ruler of our country," "ruler of England and other nations that are in the Commonwealth," "ruler of Great Britain;" she "governs all of England," "rules over the people," "rules over the land," is "the reigning monarch," and "sovereign." As the foregoing quotations suggest, a substantial proportion of pre-adolescent English children think of their Queen as considerably more than a figurehead. Recent surveys of English school children make it clear that the English child first becomes aware of the political system through the monarch, who is perceived as the *effective* and not just the *formal* leader of the country.<sup>36</sup>

Turning to France, we find a much sterner quality in the linguistic usages of the overwhelming (86 per cent) majority of children who made statements describing de Gaulle or Pompidou in terms of governance. There is not a single reference to the Premier in these terms. This finding, combined with the low level of awareness of the Premier evident in Table 1 makes it clear that Fifth Republic children focus their attention to national executive leadership entirely on the President. Both the great prevalence of governance

<sup>36</sup> See Greenstein et al., "The Child's Conception of the Queen and the Prime Minister."



Imagery in the descriptions of the President of the Republic and the specific connotations of many descriptions fit into a larger pattern of findings consistent with the notion that in France authorities are viewed in sternly hierarchical terms.

Some of the French governance references employ the same relatively neutral verbs that typify British and American responses: govern ("gouverner," "gouverne la France," "gouverne tout le peuple," "gouverne l'état,") or direct ("dirige le pays," "dirige le peuple," "dirige la France,") or locutions like "c'est le chef," "il est le chef de l'état." But on even a cursory reading of the interviews, one immediately senses the prevalence of the verb *commander*. Larousse first defines *commander* in the highly stringent sense of "to dominate" and only then in the less extreme sense of "command," which is a harsher and more imperious notion than that found in English or American children's usage. French informants assure us that our reading of *commander* as an indicator of a distinctly rigorous mode of authority is not off the mark. Almost a quarter of the French children interviewed use some form of this verb, which we also encounter in responses to the parapolitical items concerning the importance of adhering unquestioningly to parental dictates. Various children say of the president of the Republic that:

*il commande, commande le pays, commande toute la France, est obligé de commander, commande tout le peuple, commande le gouvernement.*

Additional usages like "*il nous dit ce qu'il faut faire*," and "*il donne des ordres*" seem similarly authoritarian in emphasis.<sup>37</sup> Table 3 documents some of the semantic specifics just discussed with respect to the head of state in the three nations.

Similarly extended "horizontal" analyses would be possible for the other image categories in Table 2 but space does not permit an exhaustive discussion. Instead a good deal of information can be extracted by reading the role-imagery profiles vertically, first treating perceptions of the Queen and the Prime Minister in England, and the President and Premier of France as complementary units and then comparing the presidency imagery of the two American groups.

The role-imagery categories in Table 2, other than "rules, governs, commands," also show considerable country-to-country variation. The Eng-

lish children's role-imagery profiles of the Queen and the Prime Minister have very much the quality of interlocking jigsaw puzzle pieces. Not only are references to ruling, governing, and commanding much more common with respect to the Queen than to the Prime Minister, who is seen as devoting himself to practical aspects of leadership, coordinating the efforts of other officials, such as Cabinet members, and taking orders from the Queen. Data on the English also mention the Queen's ceremonial duties.<sup>38</sup>

The French children describe only their President with any complexity at all. Descriptions of the Premier are virtually confined to the "role with other government officials" category, and he is seen as connected with only one official—the President of the Republic—as *his subordinate*. The hierarchical conception of these two roles is highly congruent with the many references above to the President of the Republic's command functions. French children also see the Premier as having the very narrow duty of filling in for the President when he is out of the country or otherwise occupied. Note the following (translated) examples of this perception, which also illustrate more generally the preoccupation with power and authority relations in the French responses.

... he is a man elected who isn't as strong as General de Gaulle. But he is the one who helps the General. It's the one ... how to say it ... he is the one who helps. For instance, if the General de Gaulle is in a country and there is a meeting in Paris, and he has to do something in Bordeaux, the Premier will go to Bordeaux to look after it.

The Premier is second to the king [sic] in the affairs of state—say, if the king is ill, there isn't anyone to replace him except the Premier—and, if the king happens to die suddenly—well, the Premier replaces him. (YES, AND WHEN THERE IS NO KING?) Well, I meant the President of the Republic. [Several of the French respondents slipped into references to the President as king.—F. G.]

Apart from the overwhelming number of references to command functions, the President of the Republic is also perceived as a lawmaker or administrator, a decision maker or problem solver, a performer of a variety of domestic, political, and ceremonial functions, and an international actor. Yet in comparison to images of the two other heads of state and the British Prime Minister, the paucity of reference to categories other than general governance is striking. The second most prevalent theme applied to the President of the Republic is his role with other government offi-

<sup>37</sup> Annick Percheron also distinguishes in her presentation between references to *gouverner* and *commander*. See her, "La conception d'autorité chez les enfants français," *Revue française de science politique*, 21 (February, 1971), 103–28. Also see her *L'univers politique des enfants* (Paris: Fondation nationale des sciences politiques, Armand Colin, 1974) and "Political Vocabulary and Ideological Proximity in French Children," in *Socialization to Politics*, ed. Jack Dennis, pp. 211–30.

<sup>38</sup> For a full exposition of how English children perceive the Queen and the Prime Minister, see Greenstein et al., "The Child's Conception of the Queen and the Prime Minister."

Table 3. Specific Vocabulary Used in Connection with "Rules, Governs, Commands" Category: Head of State

Vocabulary	England	U.S. Whites	U.S. Blacks
Ruler/rules (of the country, over, etc.)	(30) 38%	(3) 3%	(2) 8%
Runs (the country, the government, the whole set-up, etc.)	(2) 3	(11) 13	(2) 8
Leader, leads (of the people/country)	(3) 4	(10) 12	(1) 4
Decides/makes decisions	(4) 5	(5) 6	(2) 8
Governs	(5) 6	(3) 3	(1) 4
Takes care of	—	(6) 7	(1) 4
Controls	(2) 3	(5) 6	—
Head (of the U.S., of our country, of everything, etc.)	(4) 5	(7) 8	—
In charge of	(2) 3	(4) 5	(3) 12
Looks after	(4) 5	—	—
Keeps country in good shape, etc.	(4) 5	—	—
Tells (what to do, what should be done)	(1) 1	(1) 1	(4) 16
Makes up rules, laws	(2) 3	(2) 2	—
Like a king/president	(1) 1	(2) 2	(1) 4
Monarch	(2) 3	—	—
Reigns	(2) 3	—	—
Has power	—	—	(1) 4
Sovereign	(1) 1	—	—
Protects	—	(1) 1	—
Big shot	—	(1) 1	—
Big man	—	(1) 1	—
Big Boss	—	(1) 1	—
Guides	—	(1) 1	—
Helps	—	(1) 1	—
Top executive	—	(1) 1	—
Top guy	—	(1) 1	—

Vocabulary	France
Gouverneur, gouverner (la France, tout le peuple, l'état, etc.)	(47) 45%
Commandant, commander (le pays, toute la France, le peuple, le gouvernement)	(24) 23
S'occuper de (le pays, les affaires de la France, la caisse, etc.)	(15) 14
Diriger (le pays, le peuple, la France)	(14) 13
Dit (donne, chois, fait faire) les lois	(9) 8
Chef (de l'état, du pays, etc.)	(7) 7
Donne des ordres (fait obéir aux ordres)	(7) 7
Comme un roi	(5) 5
Decider	(4) 4
Tête du pays	(2) 2
Fait regner l'ordre	(2) 2
Regner	(1) 1
Maintenir	(1) 1
Ordonner	(1) 1
Plus de voix	(1) 1
Guide	(1) 1
Responsable de ce qui se passe	(1) 1
Au pouvoir	(1) 1
Mener (le pays)	(1) 1
Administrer	(1) 1

Percentages are based on the number of children who were asked the political information items and exhibited some information (same percentage base as Table 2). Percentages do not add up to the percentage in the "rules," etc. category of Table 2 because some respondents used more than one word and some did not use brief verbal constructions suitable for listing here. We do not include every form of the words and phrases listed above—e.g., some of the "in charge of" responses are "has charge of."

cial. These statements typically indicate that the President tells these officials what to do.

Well, the President of the Republic is a great man, a very respected man; he is very respected and well served. (BY WHOM?) By his ministers. (AND WHAT DOES HE DO?) He directs; he commands all the people; he gives orders and makes sure his orders are carried out. He looks after a bit of everything; he is an intelligent man.

It is a man who directs France, to keep order. He commands people. As he can't go everywhere to see what is happening, he sends prefects and ministers—all that—who watch what happens in all the towns. And then after, they come and tell him all they've seen.

Given de Gaulle's distinctive leadership style, it is remarkable that virtually no children refer to the personal qualities of the President, nor was de Gaulle's preoccupation with international statesmanship apparent beyond the 14 per cent who mentioned an international role—a percentage that does not vary between de Gaulle and Pompidou. De Gaulle's interest in *grandeur* and civil dignity did not lead to much emphasis on ceremonial aspects of the presidency. Instead, there is the single constellation of "rules-governs-commands" images. This stress on governance and absence of personalism is precisely what Percheron, who used a substantially different (word association) instrument,<sup>39</sup> found in a 1969 study of children's images of the President of the Republic. Overall, the general "thinness" of imagery about these two French roles is more consistent with the apolitical than with the highly politicized conception of French political culture.

Turning directly to that widely studied phenomenon, American children's perceptions of their President, we note the frequent references to decision making and problem solving (35 per cent for whites, 29 per cent for blacks). Although the American presidency is said to derive some of its political leverage from the fusion of the ceremonial with the political functions, white American children were no more likely to refer to ceremonial duties of the President than were British children to mention the Prime Minister in ceremonial contexts, and black children were less likely

to make such references. Mr. Nixon's early "low profile" presidency, of course, came nowhere near the British monarchy as a stimulus for reference to ceremonial duties, nor does it elicit personal references to the President among our respondents.

Perception of the President's international role also warrants special mention. This function is rarely mentioned with respect to the other leaders, but is mentioned by two fifths of the American children. Indeed, there is even reason to believe that children (not to speak of adults) in other countries perceive the American President in this fashion.<sup>40</sup> One need not fall back on dated patriotic clichés like "leader of the Free World," to recognize the accuracy of this perception even before Mr. Nixon's peripatetic diplomacy in China and the Soviet Union enhanced his standing in the polls and insured his margin of electoral victory.

Clearly the American President circa 1970 occupies substantial cognitive space in pre-adult political orientations, but what of the affective matters dealt with in the benevolent leader literature?

**Evaluative Aspects of Political Imagery.** Even though we asked respondents simply to describe the leadership positions of their polity to the imaginary foreign child, their responses exhibit considerable affective content. In seeking to characterize the feelings underlying the statements children made to us, we must tread warily because much of the response content is ambiguous.

Table 4, which reports the frequency of positive, negative, affectively mixed, and neutral role descriptions for those respondents able to describe a role, brings us directly to whether the American benevolent-leader findings reported in the early 1960s are applicable to later and different populations, as well as to how accurate were the interpretations of the original Eisenhower-Kennedy Administration findings. The table presents data on the heads of state and the Prime Ministers of England and France, and the President of the United States.

Table 4 is the result of an extended sequence of codings designed to disentangle genuine expressions of affect from cognitive statements and vague conventional usages. Beginning with the criteria established in my 1958 study, coders were provided with a comprehensive set of indicators

<sup>39</sup> Percheron, "La conception d'autorité," especially pp. 110-19. It will be remembered that 39 of the 106 French respondents in the present study were interviewed during the de Gaulle presidency. One of Roig and Billon-Grand's respondents made a point of referring to de Gaulle's *grand nez*, but Roig and Billon-Grand (*La socialisation politique des enfants*) do not appear to have elicited a great deal of personal imagery. On the lack of "personalism" in the image of de Gaulle sought to convey, see Stanley and Inge Hoffman, "The Will to Grandeur: de Gaulle as Political Artist," in *Philosophers and Kings: Studies in Leadership*, ed. Dankwart A. Rustow (New York: Braziller, 1970), pp. 248-315.

<sup>40</sup> In a small sample of sixth-grade Australian children studied by Connell in 1968, there was somewhat greater awareness of the President of the United States than of the Prime Minister of Australia! Connell, *The Child's Construction of Politics*, p. 125. For similar findings in a Canadian study, see Jon H. Pammett, "The Development of Political Orientations in Canadian School Children," *Canadian Journal of Political Science*, 4 (March, 1971), 132-41.

Table 4. Levels of Affect and Idealization Vis-à-Vis Political Roles  
(percentages)

Response	England		France		U.S. Whites	U.S. Blacks
	Queen	PM	President	Premier	President	President
Positive/idealized	47	28	30	3	55	32
Mixed	0	1	0	0	0	4
Negative	1	9	0	0	1	4
Neutral	51	62	69	97	43	60
Not ascertained	1	0	1	0	1	0
Total	100	100	100	100	100	100
N <sup>a</sup>	(80)	(80)	(106)	(99)	(86)	(25)

Breakdown of Responses Coded as Positive/Idealized						
Explicit Positive Affect	15.0	2.5	7.5	0.0	29.1	16.0
Other Explicit Idealization	3.8	0.0	2.8	0.0	2.3	4.0
"Helps"	8.8	2.5	2.8	0.0	10.5	4.0
Does Good Things	5.0	5.3	8.5	0.0	9.3	4.0
Says What's Right	3.8	3.8	.9	1.0	1.2	0.0
"Important"	8.8	7.5	1.8	2.0	2.3	4.0
Must Be Obeyed	1.3	0.0	5.7	0.0	0.0	0.0
Sub-total	46.5	27.6	30.0	3.0	54.7	32.0
N <sup>a</sup>	(38)	(22)	(32)	(3)	(47)	(8)

<sup>a</sup> Percentages based on those respondents familiar with role.

Coding Status: Initial reliabilities were deemed inadequate. Inter-coder discrepancies were resolved and coding criteria further specified by F. G., and all protocols were coded by F. G. and a second coder with resolution of discrepancies by discussion.

of positive and negative affect. Positive affect was described as follows:

Respondent describes duties in a positive or benevolent way: "The leader takes care of the country," "helps people," "makes things better," "improves conditions," "looks after us," "goes around to spread goodwill among the people," "most people look up to him or her," etc.

Negative affect included all negative or hostile references to the public figure or his duties, such as "hurts the country," "wastes time," and "does nothing but talk." Included in negative affect were criticisms of the current role-incumbent on partisan grounds even when it was evident that the child felt no animus toward the role itself. If the child indicated that the role was good but the individual holding it was not, or otherwise made both positive and negative statements, the "mixed" category was used. Neutral references included colorless and bland job descriptions.

This coding procedure produced striking patterns of cross-national and black-white American differences, but inter-coder reliabilities were low, and some patterns of differences between roles

within and between countries seemed inconsistent with our impressions from reading the interviews. For example, in the tabulation from the first coding, respondents were more positive toward the role of legislator than toward the President, whereas our impression from the interviews was that there was little cognizance of *or* affect toward the legislator. It turned out that "help" when used to describe a legislator often was an affectively neutral term, referring merely to the subordinate duties of an assistant. Even references to "helping the country" could not invariably be interpreted as evidence of positive affect, since it was not always certain that the term was more than a loose conventional usage.

A more differentiated set of criteria for distinguishing positive affect was therefore devised. (There had been little difficulty in securing agreement between the coders in the few role descriptions that conveyed negative connotations.) The differentiated code identifies the following sub-themes that appear to evince positive affect: (1) *explicitly positive* evaluative statements that any reader would be hard pressed to interpret in non-affective terms (several examples will be given

below); (2) statements not explicitly affective that describe the *importance or power* of the leader in an idealized way; (3) generalized references to *helping* (excluding the brief case-carrying variant);<sup>41</sup> (4) references to unambiguously positive aspects of *role performance* (such as stopping wars or promoting economic prosperity); (5) references to the leader as a source of *moral guidance* or as a chooser of "right" policies; and (6) general statements about the importance of the leader, excluding any in which the word "important" seemed merely to be used mechanically as a synonym for "leader." After rereading the protocols several times, we added a seventh category, the variant of importance that stresses the *need to obey* the leader.

For each of the three countries, leadership role descriptions that fit any of the seven criteria are aggregated to produce the "positive-idealized" category of Table 4, which also reports the incidence of the differentiated positive affect sub-categories.<sup>42</sup> One of the principal findings summarized in Table 4 has already been alluded to: given the open-ended stimulus of being asked to describe political leadership roles, very few of these end-of-the-1960s children spontaneously introduced negative assertions into their discourse. As we shall see in the analysis below of semi-projective story completions, the children were not *incapable* of making negative statements. Even the political information question produced *some* negative evaluations—by English children of the Prime Minister (these proved in every instance to be criticisms by middle-class children of Harold Wilson rather than of the prime ministerial role), and by black American children of the President. Although Percheron's word-association technique (used in 1969) and Roig and Billon-Grand's fixed-choice questions (used in 1962) did

elicit negative pre-adult evaluations of the President of the Republic,<sup>43</sup> none of the French respondents we interviewed made negative statements about their President.

Turning to positive evaluations and focusing only on the heads of state and Prime Ministers, we see that apart from the absence of evaluations of the perceptually bland French Premier, assertions falling into the positive-affect category were made by between 30 per cent and somewhat more than 50 per cent of those children in the four comparison groups who were familiar with the roles. The variations in level of positive affect complement the image content data already presented. In England the Queen is far more likely to be described in a positive light than is the Prime Minister; this difference is especially evident if the ratio of positive to negative evaluations is considered. In France, there is modest positive affect toward the President and, again consistent with Percheron's interpretations of French political orientations, there are more neutral references to the head of state by French children than by any of the other comparison groups.

Finally, to my surprise, the first-term Nixon-Administration counterparts of children in the Eisenhower-Kennedy Administration studies—namely the white American respondents—were *extraordinarily positive in their spontaneous descriptions of the President*. A reminder that the study was conducted in 1969–70 and not 1960 is provided by the finding that the American black children were only a little more than half as likely as the whites to refer to the President in positive terms, and somewhat more likely to make negative statements. Even the blacks, however, made more positive than negative assertions.<sup>44</sup>

The following quotations give a concrete impression of the source of the statistics in Table 4 and as usual exhibit interesting nation-specific nuances, some of which I have italicized for emphasis.

#### England

The Queen goes on state visits to other parts of the Dominion or country and is quite prominent at opening new things, or at least one member of the Royal Family is. And goes around to *spread goodwill* between the people and save bitter arguments. . . . We don't see the Queen much. We want to see somebody. And

<sup>41</sup> See notes 31 and 37.

<sup>42</sup> The black respondents were drawn from two Connecticut communities: a New Haven suburb that has become increasingly black in recent years and the small industrial/college town of Middletown. Neither of these communities showed an absence of black consciousness in 1970, but neither community was in the vanguard of black militancy or alienation. On the situational determinants of blacks' political orientations, see Schuman and Gruenberg, "The Impact of City on Racial Attitudes."

<sup>43</sup> Exclusion of this variant does indeed establish that American children exhibit more affect toward the President than toward members of Congress. For a description of the differentiated positive-affect categories, see Figure 2 of Fred I. Greenstein, "Children's Images of Political Leaders in Three Democracies: The Benevolent Leader Revisited" (Ann Arbor: Xerox University Microfilms), presented at the 1973 Annual Meeting of the American Political Science Association.

<sup>44</sup> When considering either the aggregate or the explicit positive-affect category, it should be remembered that we are dealing with spontaneous manifestations of affect in response to an item that simply asks for a description of the role. An explicitly evaluative question might produce a greater proportion of evaluative responses, but at the risk that some responses might stem from the stimulus of being questioned than from previously formed views and assumptions. If the incumbent rather than the role were made salient, there would probably be more negative evaluations, even if the leader was reasonably popular, and especially if surveying were close enough to elections to tap partisanship.

she goes around quieting them . . . state visits . . . just to show her face sort of things.

She is a very important person and she is . . . the old kings and queens were her ancestors . . . and she is a model to all of England and she is very perfect . . . well, tries to be . . . and is always opening important things.

#### France

The President is a man who makes the laws . . . who *must be respected* because it is for our good. He travels to other countries so that countries can be friends. He tries to have the fewest possible wars.

The President of the Republic must look after the country from the moment that he's been elected until . . . I think it's for seven years. His role is to maintain a peaceful France, like strikes and that. If there were no Presidents of the Republic . . . we might not get along, because *it's required that there should be a President of the Republic* to hand out laws, for instance, for the tribunals. . . .

To capture more fully a sense of the unexpected spate of 1969-70 American benevolent-leader imagery, it is instructive to look at the assertions of a number of the American respondents. First, several quotations from white children.

The President of the United States is a man or a woman or whatever, who is, like, picked by the people to lead the country. And they try to make the person almost perfect. I mean, if he does anything wrong they down him . . . because *if a person is going to be the head of a country like the United States for years, he just has to be just about perfect.*

Respondent: Well, he's the person that helps guide the people. He doesn't like, rule but he helps people. Well, he makes decisions for what he thinks is best for the country.

Interviewer: What else does he do?

Respondent: Well, he decides how things should be done and how they should be run and what should be done about the problem.

Interviewer: Anything else?

Respondent: And *[he tries] to keep peace* among, like, the states. He tries to help people get along together, like, so there won't be any fighting and stuff around.

He answers the questions and he *tries to make America a better place* to live in ways, like, he tries his best to keep the war in Vietnam calm . . . he's trying to calm it and he's . . . and he's making sure that everybody he's sending, he doesn't always care about his country himself, too. Like, he, he is already conducted a Biafra fund . . . in New York.

The President of the United States is a very important man that is trying to make the U.S. a better place and,

well, he takes care of problems that just a few people won't be able to take care of, like the war in Vietnam and like the men who have been over there for a long time. And *he tries to make things equal and fair* so that you know you won't really get mad on the taxes. He's, well, he's trying to make the United States a better place and he's trying to solve a lot of problems.

Here is an example of a positive perception of the President by a black respondent.

Respondent: Well, I would tell the foreign child he's just another man. . . . He studied for years to be a politician and so he runs. . . . Most likely he would start from the bottom and makes his way up, so he's just another man who wants to be the President and then has a very good chance of being, and so he starts from the bottom and works his way up, all the way up from the chairman of his town to the President.

Interviewer: Okay, what does the President do?

Respondent: He makes hard decisions . . . He's mostly busy with things about the war and economical problems of the United States and poverty. Some people are poor in the United States. He is just thinking of ways to help . . . help the country along much better than it is.

The relatively few negative references to the President are illustrated by the following two quotations, the first from the most negatively politicized of our black respondents, and the second from one of the few white children who expressed negative views of the President, a twelve-year old girl from a wealthy community in Connecticut's New York-commuting belt.

Respondent: I'd say [of the President] he's stupid and he don't know what he's doing.

Interviewer: What does he do?

Respondent: He doesn't do. . . . He doesn't let people do what they want to do. He always putting people in jail or somethin'. . . . He's supposed to help people, and he supposed to keep peace in the world, but he isn't doing that. . . . He isn't trying to stop anything.

Interviewer: Suppose a foreign child asks you what is the President of the United States?

Respondent: A rat.

Interviewer: Okay, try to give the foreign child an idea of what he does. What does he do?

Respondent: I can name a lot of things. . . . He prejudiced. . . . He lousy. He picked . . . Spiro Agnew. Spiro Agnew stinks, and he ain't no good, none of them.

Interviewer: What is the president's job?

Respondent: To try to make people happy, but he's making them miserable.

Respondent: Well, I'd say he's doing a pretty lousy job, first of all, and that the government should be changed, and it's based on pretty good grounds but most of it isn't all fair, and sometimes he makes bad mistakes. And, well it's not the worst type of government there is, but it should be better.

Interviewer: How would you like to see it changed or be better if you could do anything about it?

Respondent: Well, I think we got too involved in other countries' problems. You know, take enough care about people who are here. I tend more toward a person like Jerry Rubin than I would to the President. I think he's done a pretty bad job . . . like sending more troops into Cambodia, and I think we're too involved in other people's problems, and he doesn't care enough about people here.

Most of the 1969–70 American respondents reflect little of the political turmoil of the wider environment in which they had grown up, considering spontaneous content of their descriptions of the President.

### Leaders Imagined in Action

The semi-projective, incomplete story items, which deal with expectations about how leaders and citizens would behave in specific situations, provide a complement to the more straightforward sequence of questions in which political information, imagery, and affect are elicited by the static procedure of asking the child simply to describe national leaders and institutions. Like the sequence of questions just discussed, the story-completion items also produce response protocols, each of which can be multiply coded for different aspects of cognitive and affective content. This layered quality of the story completions is illustrated in the following detailed discussion of a story-completion item that produces exceptionally fertile responses. The item text read as follows:

One day the President [substitute Queen in England, President of the Republic in France] was driving his car to a meeting. Because he was late, he was driving very fast. The police stop the car. Finish the story.

**Definitions of the Situation.** Table 5 reports definitions of the situations in the head of state-traffic policeman story, since challenges to a story's frame of reference often provide insights into the respondents' explicit or unarticulated major premises about the subject matter of the story. A potential ambiguity in the present story is "driving *his* (or *her*) car to a meeting." The verbal distinctions among the explicit item wording and

"driving *in* his car" or "*being driven* in his car" are sufficiently subtle for a respondent with well-settled assumptions about the behavior of a national leader to "choose" not to perceive him (or her) as someone who personally engages in such mundane tasks.

Table 5 makes it clear that some children explicitly deny that the head of state would be driving, some automatically introduce a chauffeur into the scenario, and still others deny that the head of state would break a law or that a policeman would dare to stop a royal or a presidential car. All told, 8 per cent of the Americans, 1.8 per cent of the French, and 29 per cent of the English redefined the episode in one of these fashions.

The following examples of situation redefinitions include italicizations of phrases that seem, even within cross-nationally common categories, to have a residue of national distinctiveness.

#### England

*I don't think the Queen's car has got any number plate like PRX2452, like any normal car will do. Probably have a lot of bodyguards around, and cars and plain-clothes policemen. And probably have a big train of police cars around it. There would probably be someone sitting in the car, like a policeman sitting next to the chauffeur, or maybe a policeman driving the car. He would probably show him a [warrant] and he would say that's fine and probably call up a bodyguard, a couple of men on motorbikes to show them the way quickly.*

#### France

He isn't the one to drive usually; he has a chauffeur. And he is late. The police stop him and I don't think . . . no, they don't stop him. Well, there, do I *have* to say that they stop him?

First of all, it isn't true that General de Gaulle drives his own car. He has a chauffeur and he can tell him to go very fast. *If he is stopped by a policeman, the policeman will have to let him go because General de Gaulle commands all of them—everyone—because they all must serve France and they are there to see that order is maintained in France.*

#### United States

Well, first of all, the story is wrong because the President wouldn't drive to a meeting. In fact, he wouldn't because he has chauffeurs and all kinds of things. And he would also not be alone in the car . . . he would have some kind of an escort . . . a police escort. And so I doubt if he would get a ticket.

**Outcome of the Episode.** Table 6 shows what happens in the episode. The American white children are roughly 15 per cent more likely than the English and French children to imagine that the head of state will be punished, with the American blacks falling between them. Some children in each group conclude that the head of state would

**Table 5. The Head of State and the Policeman: Definitions of the Situation**  
(percentages)

	England	France	U.S. Whites	U.S. Blacks
Episode challenged because Head of State would not be driving or would be escorted	10	13	6	4
Chauffeur driving <sup>a</sup>	18	3	2	4
Episode challenged because Head of State would not break laws	3	1	1	0
Episode challenged because the police would not stop the President	1	5	0	0
Total per cent of respondents redefining the situation	29	18	8	8
<i>N</i>	(78-80)	(103-104)	(85)	(25)

Note: Responses add up to more than the total proportion in England, France, and the United States because all themes mentioned by a respondent were coded, but we have totaled *respondents* rather than *responses*.

<sup>a</sup> This category was used where the child has no trouble conceptualizing the story but tells a story with the chauffeur nonetheless.

Coding Status: Reliabilities not computed for this table or Table 1. In each case the criteria are highly specific and spot-checking shows great accuracy.

This and the remaining tables replace similar preliminary tables on the same variables used illustratively in Greenstein and Tarrow, "Political Orientations of Children."

not be punished but would receive a verbal admonition not to speed again. This approach to completing the story is by far the most common in England, where the nonpolitical data also show evidence that authorities are respected, but they are not above verbal criticism.

The head of state is considerably more likely to get away with speeding in France where more than half of the respondents indicate that this would occur. "Gets away" is second most common among American blacks and least common among American whites, although the differences among the three non-French groups are not clear-cut. The occasional respondent who imagines that the *Policeman* would be punished for the crime of *lèse majesté* is noted below the row total in Table 6. Only in England and France do some children introduce this possibility.

The specific outcomes imagined by the children are varied and often marvelously original. The following examples of story outcomes also provide illustrations germane to Table 7 which reports affect toward the head of state revealed in completing the episode.

#### England

The police stopped the car, and they saw it was the Queen who was driving, but they didn't take any notice because they thought that even though it was the Queen, that as she was driving fast and exceeding the speed limit, she would have to be charged like any other person, and it wasn't right for her just to be let off because she ruled the country and just because she was an important person. It didn't matter and they charged her just the same. The Queen being quite a good Queen and thinking of other people, didn't mind paying because she thought everybody else who was

**Table 6. The Head of State and the Policeman: Outcome of the Episode and Fate of the Policeman**  
(percentages)

	England	France	U.S. Whites	U.S. Blacks
Head of State punished: ticket, fine, etc.	23	26	41	32
Head of State is only cautioned	35	18	20	24
Head of State gets away with speeding	42	56	39	44
	100	99	99	100
Policeman is punished	4	1	0	0
<i>N</i> <sup>a</sup>	(77)	(98)	(80)	(25)

<sup>a</sup> *N*'s are less than full sample size because one French respondent was not administered the story and several respondents in each group except the U.S. blacks were coded as "other" or "contingent."

Coding Status: 90 per cent agreement;  $\pi = .84$  ( $N = 60$ ).



Table 7. The Head of State and the Policeman: Evaluation of the Head of State (percentages)

	England	France	U.S. Whites	U.S. Blacks
<i>Positive</i> : Praises policeman for doing his duty; urges policeman to give him a ticket; benevolent, friendly, nice, understanding; admission or tacit admission of being in the wrong; apologizes, is embarrassed, admits being wrong, says he's sorry and won't do it again, says he didn't realize he was going too fast, thinks it's fair that he got a ticket.	19	11	28	33
<i>Mixed</i> : President is portrayed both positively and negatively.	9	3	2	0
<i>Negative</i> : Angry, annoyed, unfriendly, arrogant; unfairly sacks policeman, threatens to sack policeman, speeds off again after being stopped, drives off in a huff, refuses to pay the fine, argues with the policeman, pulls rank arrogantly.	19	20	20	19
<i>Neutral</i> : President does enter into the story but his reactions, conversation, behavior are described in a colorless or neutral manner, or President has passive role.	53	66	49	48
<i>N<sup>a</sup></i>	100 (74)	100 (92)	99 (81)	100 (25)

<sup>a</sup> The Head of State does not appear in 4 English, 11 French, and 3 white American, and 4 black American stories; 1 English, 2 French, and 2 white American stories are coded "other" and 1 French respondent was not administered the story.

Coding Status: Consensual Coding.

going fast on that road would have to pay as well so she didn't see why she wouldn't have to. (Punished; positive affect.)

I suppose the Queen would sack him and get a new one; I know if I was the Queen and I was stopped by one I know I'd be very embarrassed. (WHAT DO YOU THINK THE QUEEN SAID TO THE POLICEMAN?) Be quiet, you silly thing. (Not punished; affect unclear.)

France

The President, believing that everything is allowed to him because he governs France, will say that he is in a hurry. The police will let him continue, and he'll settle the matter in another day. (Not punished; affect unclear.)

The President of the Republic, he drives very fast, and the policeman has to give him a fine. Then the President of the Republic says, "I'm in a great hurry; I'll pay next time." And the policeman, "I don't know . . . Why were you going so fast?" He says, "I'm late for a meeting and I have to hurry, and . . . after all . . . Let me go; I am the President of the Republic, . . . I'm no lawbreaker. I'll pay your fine. After all—the President of the Republic." (Punished; positive affect.)

United States

Well . . . well, I'm the President of the United States. I was in a hurry, and I wanted to get to the meeting fast; and I know I went through this, through . . . disobeyed the speed limit. And don't give me a ticket. (Episode goes on to indicate President not punished; negative affect.)

**Evaluations of the Head of State.** Successive codings of a single interview passage are analogous to lifting away successive transparency overlays. In Table 7, which seeks to isolate the evaluative levels in the responses, we treat as positive evaluations two classes of responses—those in which the head of state *praises* the policeman for doing his duty, and those in which there is explicit admission of wrongdoing by the head of state, or some sign of remorse or apology. We treat as negative those story completions in which the head of state is angry, annoyed, argues, fires the policeman, or pulls rank. (The full coding criteria are listed with the row labels in Table 7). As in the political information questions, the child is not explicitly *asked* to make an evaluative judgment; instead a stimulus is provided that makes spontaneous evaluation possible. While in all comparison groups the modal response is affectively neutral, there is notable variation among those children whose responses do contain affective elements. (In retrospect, I realize that a probe for affect would have enhanced this aspect of the interview. As the quotations above indicate, it is not always possible to infer evaluations from the child's report of the leader's behavior. In general we were conservative in inferring affect from the behavior described in the story completions.)

It is intriguing to compare the distribution of positive and negative evaluations of the behavior of the head of state in the traffic violation episode with that in the political information item (i.e.,

Table 7 with Table 4). The political information item merely refers to the head of state in the abstract. Some children do immediately supply details connected with the present role incumbent, but the interview content suggests that much of the preponderance of favorable over unfavorable affect reflects positive assumptions about the leadership role and its importance to society. Children appear able—in a wholly unself-conscious way—to distinguish between roles and the individuals who fill them. This does not mean that they recognize that they are making the distinction, any more than they need to know Chomskian grammar in order to use grammatical forms. Many of the neutrally coded entries in Table 4 also appear to be role related, although the negative references (especially those by the English to the Prime Minister and the American blacks to the President) are usually responses to the incumbent.

The traffic policeman episode leaves much less room for responding merely to an abstract vision of the role, since illegal behavior by the leader is part of the initial definition of the situation. In spite of the handful of children who establish a perimeter of defense that denies the very terms of the question (Table 5), most of them *can* envisage a head of state as a malefactor, and many are then prepared to portray him in negative terms.

In all comparison groups the ratio of positive to negative references decreases substantially between Table 4 and 7. The Queen "in the abstract" is a 47-to-1 object of positive evaluations; the Queen defined as traffic violator is described in a negative light by about a fifth of the English children—precisely as many as describe her favorably under those circumstances. All affectively toned descriptions by French children of the presidential role in the abstract are positive. But the French child's vision of how the President would respond to a policeman who interferes with his driving speed is twice as likely to be negative as positive.

The contrast between the findings on white American children in Table 7 and Table 4 are of special interest, given our concern with benevolent and not-so-benevolent leaders. The positive references by these 1969–70 white American children to the President, which have such a beginning-of-the-decade character when elicited in the abstract (55 per cent positive to one per cent negative), drop precipitously in the context of presidential lawbreaking. Even in this instance, however, pre-Watergate white American children were more likely to describe a presidential traffic violator favorably (28 per cent) than unfavorably (20 per cent). Frequently they imagine the American President as acting positively when his lawbreaking is brought to his attention, often even praising the policeman who tickets him. In sharp contrast,

the French President, who is likely to escape punishment, is nevertheless more often described as behaving negatively than positively.

Clearly these white American children are not totally antagonistic to political leaders, but they are also not in the thralls of an idealized view of the President. Research on children during the Eisenhower and Kennedy years never explored the possible discrepancies between general views of the Presidency and responses to hypothetical presidential law infractions. As a result, the early studies may well have underemphasized the capacities of children in the 1950s and early 1960s to criticize political leaders, if given a suitable occasion.

The discrepancy between Tables 4 and 7 may provide a fundamental insight into why children from the age cohort that in the late 1950s and early 1960s described the President in terms reminiscent of the *Boy Scout Handbook* could have engaged in "off-the-system" behavior in the late 1960s.<sup>45</sup> Why was the early political socialization literature seemingly nonpredictive? The reason is probably connected to general aspects of how attitudes relate to behavior. There is no reason *in principle* to assume that abstract attitudes toward an object will serve as sufficient predictors of behavior toward it in the absence of further specification of other classes of variables, such as situational factors and contingent attitudes about how it is appropriate to act under specific circumstances.<sup>46</sup>

Thus, for example, an individual may value his country's leadership roles and institutions, but may also (perhaps precisely *because* of his commitment to "the system") value the absence of overseas military ventures, domestic political espionage, and other departures from his idealized view of national purpose. The generalized positive attitude to the nation and its leadership roles may not be irrelevant to the overall func-

<sup>45</sup> To the degree that these were the same children. Political idealization was more common among children of lower socioeconomic status in my 1958 study (*Children and Politics*, chapter 5). But college student protest was more common among students of upper SES backgrounds, especially before protest behavior became widely diffused in the late 1960s.

<sup>46</sup> The failure of attitudes to predict behavior is a common theme in the social psychological field of attitude studies. Recently, however, there has been a shift in emphasis from studies and literature reviews pointing to the lack of connection between measured attitudes and subsequent behavior to analyses such as Schuman's that refer to the real-world situations in which individual and collective action occurs that typically force individuals "to reconcile two or more conflicting values." Howard Schuman, "Attitudes vs. Actions versus Attitudes vs. Attitudes," *Public Opinion Quarterly*, 36 (Fall, 1972), 347–54. Also see Alan G. Weinstein, "Predicting Behavior from Attitudes," *Public Opinion Quarterly*, 36 (Fall, 1972), 335–60.

oning of the individual's belief system, but in-  
ead may provide the raw material for disillusion-  
ent reflected either in political apathy or protest  
ehavior. In a context such as the escalation of  
e Vietnam conflict or the Watergate revelations,  
lthough both kinds of attitudes may be conse-  
quential, the more concrete ones measured by an  
em specifying contingencies like those in the  
ead of state/policeman episode may be more be-  
aviorally relevant than attitudes evoked by an  
em on the presidency in general.

In sharp contrast to Table 4, in which the  
lacks were drastically less positive in their gen-  
eral descriptions of the President than were the  
whites, the racial differences in Table 7 indicate a  
reater expectancy by blacks of positive behavior  
y the President in his encounter with the police-  
man. One might have expected black responses to  
he President to be less positive than white re-  
ponses in both tables,<sup>47</sup> but two factors (in addi-  
tion to the statistical vagaries arising from the  
mallness of the black sample) are at work here:  
1) black approval of the President is already low  
nd therefore cannot go much lower; and (2) it  
s well documented that blacks have distinctive  
eactions to policemen—reactions that are nega-  
ive and probably also more fearful of the power  
of police officers.<sup>48</sup> Hence the response in Table 7  
may be that any citizen would act with some cir-  
umspection when encountering a policeman.

**Norms Governing the Episode.** Tables 8 and 9 sum-  
marize references to two of the most frequent  
norms enunciated in completions of this fertile  
tem—the principle that the head of state is above  
the law, and that everyone is equal before the  
aw. We construed as norms the children's more  
or less explicit, verbalized justifications for the  
actions of the participants in the episode. Out-  
comes themselves were not interpreted tautologi-  
cally as implying norms. Instead, there had to be  
one of two types of verbal justification: (1) state-

<sup>47</sup> Cross-tabulations between responses to the pair  
of items analyzed in this paper are not reported be-  
cause of the small number of cases and because of the  
strategy indicated in note 24 of viewing the responses  
as properties of populations rather than of individuals.  
But it should be noted that individuals presenting the  
President "favorably" in Table 7 are not invariably  
the children who were coded as positive in Table 4.  
<sup>48</sup> Black children's orientations to the police are re-  
viewed in David O. Sears, "Political Socialization," in  
Fred I. Greenstein and Nelson W. Polsby, eds. *Hand-  
book of Political Science*, II, pp. 93–153. For evidence  
that white children show more cynicism toward the  
President because of Watergate, but the already cynical  
responses of the black children remain, see Michael  
Lupfer and Charles Kenny, "Children's Reactions to  
the President: Pre- and Post-Watergate Findings,"  
(mimeo) delivered at the 1974 Annual Meeting of  
the American Political Science Association.

Table 8. Norm That Head of State is Equal  
Before the Law (percentages)

	Eng- land	France	U.S. Whites	U.S. Blacks
Norm explicitly stated	13	21	30	8
Norm strongly implied	4	3	5	0
Norm not expressed	84	76	65	92
N <sup>a</sup>	101 (79)	100 (100)	100 (82)	100 (25)

<sup>a</sup> Cases less than full sample size because some re-  
spondents fall in miscellaneous categories and story  
not administered to one French respondent.  
Coding Status: Consensual coding.

ments in which the norm is strongly implied, or  
(2) explicit, generalized normative statements. (An  
example of an implicit statement of a norm is,  
"They saw it was the President; they let him go;"  
an explicit statement of a norm would be "Every-  
one has to obey the law.")

As is evident in Table 8, the norm of equality,  
whether expressed elliptically or as a general  
proposition, is more frequent among American  
whites than among the English or French chil-  
dren, and least frequent among American blacks.  
This, of course, is one of the most distinctive and  
enduring verbal formulas in American political  
culture, first noted by foreign observers as early  
as the 18th century. We code in Table 8 for the  
enunciation of the norm, whether or not it is  
actually adhered to. Thus, even if the President  
claims but does not receive legal immunity, the  
norm is tabulated. Some children, especially in  
the Watergate-period interviews, refer to both the  
norm of equality and to the norm that the Presi-

Table 9. Norm that Head of State is Above Law  
(percentages)

	Eng- land	France	U.S. Whites	U.S. Blacks
Norm explicitly stated	18	24	21	16
Norm strongly implied	10	19	10	16
Norm not expressed	72	57	69	68
N <sup>a</sup>	100 (77)	100 (100)	100 (82)	100 (25)

<sup>a</sup> Cases less than full sample size because some re-  
spondents fall in miscellaneous categories and story  
not administered to one French respondent.  
Coding Status: Consensual coding.

dent is above the law, and deal explicitly with the tension between the two norms.

Here are examples of references to the norm of equality, with the nation-specific nuances italicized.

#### England

I think that she would have to pay the fine as well as anyone in the end. Whether it's some local dustman, it's still the same for everyone. So they would have to pay the fine.

The policeman would stop her and say, "You're speeding over the limit so therefore I'm going to have you up. (Laughs) I'm sorry, but you're just like *any normal person* and we'll have to treat you like that." And so the Queen's had up for driving too fast. (Great mirth) I don't think she's taken to court, but the judge comes to see her, and she has to pay a fine for £15 for driving too fast!

#### France

Perhaps he is stronger than the other men, but he ought to get a summons because he hasn't kept to the speed regulations. Agreed, he's high up, a big man, but also a man like the others. He's got to do like them. He must pay attention. He could kill someone like that. (WHAT DOES THE POLICEMAN SAY WHEN THEY STOP THE CAR?) "You may be General de Gaulle, but we have to stop you. You could run over someone like that and, since we do it to others, I don't see why we shouldn't do it to you." So he gives him a ticket, and Charles de Gaulle has to pay.

(AND WHAT DOES POMPIDOU DO IN THE END?) He says, "Since that's how it is, do to me what you would to anybody else." And the policeman *don't want to, but Pompidou makes them*. (WELL, DOES HE PAY A FINE?) Well, yes. [A distinctly Gallic combination of egalitarianism with authoritarianism—F.G.]

O.K., the policeman stops him and M. the President stops. So the policeman draws near and *when he sees M. Pompidou he immediately stands at attention*. Then M. Pompidou says, "O.K. Well, you have no reason to stand at attention. On the contrary, there's no reason that because I am the President that I should do anything I want to." So the policeman, he doesn't know what to say. He is moved, so he tells M. Pompidou, "What do you think I ought to do?" So he replies, "Well, give me a fine like anybody else, because some people might say, 'Oh, you see . . . just because it's M. Pompidou they cancelled the fine and we, poor people, we're only drivers but we get fines. That's not fair.'"

#### United States Whites

Well, if I was the policeman, I would have given the President a ticket. Because it doesn't make a difference who you are in this country, you still have to obey the laws. . . . If he was a good President, he would con-

gratulate the policeman for giving him a ticket and not letting him go with just a warning. . . . I think I would tell the policeman, you know, that it was a good thing that he did stop him. (O.K., THEN WHAT THE PRESIDENT SAY?) I don't know. He'd probably tell the policeman that he deserved it or something. We have to follow his rules [the President's so probably he'll have to follow the policeman's rules too].

Well, if the police stopped the car, I think that the President should get a fine if he was speeding, because he isn't really . . . he isn't one special . . . *he isn't the guy that runs the country, really. He just helps*. He just helps run it, like, so he . . . he should get a ticket. He should be able to obey the rules, the laws and orders that he makes. (WHAT DO YOU THINK THE PRESIDENT WOULD SAY?) He'd . . . he's probably say O.K. *because the Presidents are decided by the people who believe that they should be treated equals*.

#### United States Blacks

Well, the police stopped the car and they noted that it was the President, and, hm, they would probably give him a ticket, being fair to everyone. But, you know, he wouldn't keep him too long so he could get to his meeting. . . . If he was a good President, he wouldn't mind that the police gave him a ticket because he would want them to be fair to everyone.

In Table 9, reporting verbalizations of the norm that the Head of State is above the law, there are again no differences between American whites and blacks. The French, however, are particularly likely to state or imply this belief. Table 9 is abstracted from statements such as the following

#### England

The desk sergeant says to the policeman after the Queen is inadvertently arrested: "You silly chump! *This is the Queen, not an ordinary person*. We're sorry for this delay."

"I thought it was somebody else, because you know you shouldn't go over the speed. All right, Madam, because you're the Queen. You've made up the regulations."

#### France

Well, normally he'd have to get a ticket, but as he is the President of the Republic they let him go. . . . But really it is dangerous, because if he drives again he might do the same stupid thing and cause accidents and even if he's in the wrong, they'll reimburse everything to him because he is President.

If it's the President of the Republic, even if he exceeds the speed limit, if he drives too fast, *the police have no right to stop him*.

#### United States Whites

Respondent: Probably he felt that if it was the President then he shouldn't give it to him.

Interviewer: But does he?

Respondent: I don't think he should because *he's supposed to be going somewhere for us.*

*United States Blacks*

Since he was the President, he let him go and he said that, well, he made the world what it is . . . really is, so he let him go.

**A Comparison of Post-Watergate American White Children with the 1969-70 Respondents**

During the first two weeks of June, 1973, while the televised Watergate Hearings were in progress, we arranged to interview seventh-grade white children in the Connecticut schools in which our 1960-70 interview had been conducted. We used the same interview questions, but omitted some of the parapolitical items. Fifty-nine interviews were completed; the resulting population was evenly divided by sex and roughly by socioeconomic status.

At the time of our interviews, Mr. Nixon's national Gallup rating among adults was 44 per cent approval, 45 per cent disapproval, making him the third President in the history of national surveys to have more disapproving than approving evaluations at any time during his term. As recently as January 1973, his Gallup approval rating had been 68 per cent.<sup>49</sup> We assumed that Watergate would produce the greatest reduction in positive imagery among white children since this group had been so surprisingly positive toward the President in 1969-70.

The interviewers were instructed not to introduce references to Watergate and not to probe in a leading manner if the children referred to Watergate-related matters. Consequently, any observations the children introduced on that topic were spontaneous. Such references—either directly to Watergate or to some alternative way of labeling that constellation of events: "that robbery in Washington," "the bugging they're arguing about"—were made by half of the group at one time or another in the interview. Thirteen of these 30 children mentioned Watergate in more than one information or story completion-item. Here are examples:

Interviewer: Suppose a foreign child asked you, "What is the President?"

Respondent: Richard M. Nixon.

Interviewer: What does he do?

Respondent: He's the President. He signs papers and, like, helps turn out the laws, and stuff like that.

Interviewer: Anything else?

Respondent: Well . . . he gets in trouble a lot.

Interviewer: What kind of trouble?

<sup>49</sup> American Institute of Public Opinion release, June 23, 1973. On presidential popularity over the years, see Mueller, *War, Presidents and Public Opinion*.

Respondent: I don't know. The Watergate and everything like that now.

Respondent: He's one man, he lives in a big house, and he has children, a wife, and they go on trips, and try to end wars, and try to make things better in the United States. And sometimes they make, when they make, um, what is it? . . . Well, anyways, when they agree with sometime like another place, and afterwards they don't do it, and he has a man that works for him and tries to get to agree on something. He's just like any other person, and he tries to make things better for the United States.

Interviewer: Anything else that he does?

Respondent: He makes talks on TV.

Interviewer: What does he talk about?

Respondent: Sometimes there's a war, and he was talking about that. And he talks about Watergate. I don't really understand what the Watergate is about. I don't know what it is.

These references were atypical in that they were introduced in the item that begins the sequence of political questions.<sup>50</sup> Most of the Watergate references came later in the interview in connection with the questions on media behavior, and the questions on the political parties and voting, or on the Supreme Court, which a number of children described as "trying the most important cases—like Watergate."<sup>51</sup> But the examples quoted above were typical in two respects: the references were not presented as part of a critical indictment of the President (a number of children volunteered that "in spite of Watergate" they still preferred Nixon to McGovern<sup>52</sup>); and there was clearly not much detailed understanding of Watergate at the time of the Ervin Committee

<sup>50</sup> The basic interview schedule begins with nine story-completion items set in the child's immediate environment (school, home and peer group), more than half of which were retained in the post-Watergate schedule, since they help to build rapport. Then follows the sequence of political information questions in which the foreign child seeks information, and the explicitly political story completions such as the head of state/policeman story.

<sup>51</sup> Children know virtually nothing about the structure and practice of the Supreme Court, but instead they respond to the connotations of the two words "supreme" and "court," usually describing a non-appellate court dealing with "important cases." In 1969-70, one illustration sometimes given of "an important case" was the trial of a presidential assassin. Watergate and the punishment of presidents who break the law were the results of the same thought processes in 1973.

<sup>52</sup> Compare the Gallup report of a June 1973 "trial heat" between Nixon and McGovern in which Nixon won—though by a plurality much less than his November 1972 election margin. American Institute of Public Opinion release, July 5, 1973.

Hearings. Another aspect of the two quotations bears comment. Each has a troubled quality that seems to be induced by Watergate. The first refers vaguely to the President's own "trouble." The second employs the now familiar benevolent-leader formula ("tries to make things better"), but hems and haws through a confused discussion of problems of obtaining agreement, which is not characteristic of pre-Watergate interviews.

On first reading the post-Watergate transcripts, I had two divergent impressions, which were somewhat qualified by the quantitative data. First, I was struck by the presence of references to bribery, corruption, and impeachment. These themes were not present in my 1958 open-ended, paper-and-pencil questionnaire responses, nor were they noticed when we were deriving the cognitive content code from the 1969-70 interviews. When we tabulated references to bribery, corruption, and impeachment, however, we found that only about a half-dozen post-Watergate respondents had mentioned bribery and corruption, and about the same number had mentioned impeachment. Moreover, a retabulation of the 1969-70 protocols showed some references to these themes as well. There was a difference between 1969-70 and 1973, however. Post-Watergate references were not merely "academic." They implied real possibilities of presidential malfeasance. Post-Watergate references tended to be from McGovern supporters and especially from the very small minority of respondents who had absorbed the ideology of the antiwar movement. The most well-developed example follows, the President-policeman story completion of a boy from the same wealthy suburban community that yielded the Jerry Rubin supporter quoted above.

"Well, he says 'May I please see your—oh! Excuse me, Mr. President. Um, may I please see your license?'"

"You know who I *am*. Don't give me any of this bunk! Look, here's fifty dollars for your trouble. (In a very annoyed tone.) I have to go to the meeting! Would you please just get off my back and let me go."

"No, I have to take you in. You offered me a bribe, and you were also speeding."

"Well, what about my meeting?"

"Cancel it. It doesn't bother me whether you cancel it or not. I didn't even vote for you."

"Well, all right." So he goes and talks to the chief and says "Look, can't you just let me out on bail? I can pay for anything. Any amount of bail."

"Well, since you have to go for a meeting, I will let you out on bail."

"How much is bail?"

"Five thousand dollars for offering a bribe. Well, not five thousand, um, we'll let you go without bail, but you'll still be summoned to court, traffic court, and then we'll put you on charges of bribery."

"Can't you just—I'll give you a hundred-thousand dollars if you don't tell anybody. I could get impeached for this!"

"Well you can get impeached, but impeachment doesn't mean you'll be kicked out of office."

"Yes, but for offering a bribe I should be kicked out of office."

"Well, I'll try not to let it go out, but it's gonna get out somehow. . . . I'm gonna have to let you go now, to go to your meeting. Just don't leave, because if you leave you're gonna be in trouble. Cause we can usually find the people. The FBI's usually pretty good, even if they *are* on your side."

So he finally gets impeached, but the Congress doesn't vote to kick him out of office . . . but now he's not a very strong [President] now. Congress doesn't usually go along with his bills.

Needless to say the young man who produced this extraordinary skein of fantasy is exceptionally politicized and informed by adult as well as pre-adult standards, and he is exceptionally disposed to use Watergate-related imagery in negatively evaluating the President. Yet his negative judgment seems to be of presidential role *performance*, not of the role itself. There are manifestations of respect for the presidency even in this story completion (the President is allowed to go to his meeting, his bail is reduced, etc.).

The respect for the presidency as a role even in our most vivid corruption-laden, impeachment story-completion leads to the second impression that emerged from an initial reading of the 1973 protocols: A remarkable number of these respondents seemed, Watergate notwithstanding, to be making precisely the same sort of "old-fashioned" idealized statements about the President so familiar to anyone who encourages white American children to describe their nation's political roles and institutions. For example:

Respondent: I'd say that he was a very good President, and he's Republican, and his name is Richard M. Nixon.

Interviewer: Could you tell (the foreign child) what he does?

Respondent: Well he got the POW's out of Vietnam and you know he tries to do something for the United States. So you know it would be a better world to live in. . . .

Interviewer: What is the job of the President?

Respondent: To make people happy. . . . He helps

other people in other countries too, and tries to make (things) better for everyone.

He helps different people, like if something happens, like a hurricane or something, he'll like pay for the damages.

He tries to stop pollution and he finished the war in Vietnam, and he does lots of stuff that's good.

Moreover, the negative and mixed examples—like the following two—were remarkably temperate:

[His job includes] taxes and stuff like that, and the war you know, he tries to stop it and stuff like that. If he can. He's not doing such a hot job.

He decides a lot of things, like about the wars, and makes decisions about a lot of things, and like he really controls the country and you really have to do what he tells you to do. And that you shouldn't say "Well, he's wrong all the time, but he's right most of the time." And that, well he makes some good laws and some not-so-good laws. But you should obey him anyhow.

Could it be that even in a period of widespread adult criticism and distrust of the President, children would continue to idealize him?<sup>53</sup> This possibility is suggested by the finding in my 1958 study that there were substantially (about 30 per cent) more positive pre-adult than adult evaluations of the President in response to fixed-choice questions.<sup>54</sup> At the time I speculated that a number of converging influences, some in children's environments and others in their own dispositions and perceptual proclivities, would tend to insulate them from negative aspects of adult political psychology and from political realities in general. These include the child's inattention to the wider environment, the tendency for some adults to mute their own political cynicism in explaining politics to children, and the congeniality to children of the reassuring view that the adult authorities on whom they are dependent (and therefore the "leading" adult in the nation) have favorable qualities.<sup>55</sup>

Even American adults were far from uniformly negative toward President Nixon during the period of our interviews, which preceded such

dramatic events as the firing of Special Prosecutor Archibald Cox, and the eventual release of the tapes directly implicating Nixon in the Watergate coverup. Not only was the percentage of citizens disapproving of the President's handling of his job roughly matched by an equal percentage approving, but there was a general public reluctance to condemn him completely. For example, a Gallup Poll of June 22–25, 1973 found that although 71 per cent of a national sample claimed presidential complicity in either the Watergate activities or their coverup, only 19 per cent felt he should be compelled to leave office.<sup>56</sup>

The quantitative results of our post-Watergate interviews are summarized in Table 10, which compares post-Watergate and pre-Watergate white American respondents. Considering the findings one by one, we note first that the overall level of affect and idealization in the descriptions of the President to the imaginary foreign child differs in spite of the continuation of benevolent-leader imagery: pre-Watergate, 55 per cent positive to 1 per cent negative; post-Watergate, 45 per cent positive to 5 per cent negative. Among the components of positive affect toward the President the decline is greatest in the more explicitly positive subcategories bracketed in Table 10. Moreover, the percentage of responses coded in the subcategory "must be obeyed" increased slightly. This distinctly cool mode of idealizing authority was absent in the pre-Watergate American interviews, and was evident in 1969–70 largely among the authority-cowed French children (Table 4). In short, detailed analysis of the political information question about the President suggests a slight decline in idealization during the early Watergate period, as well as a change in the qualitative *tenor* of presidential idealizations.

In the traffic policeman episode, which accounts for the remainder of the comparisons in Table 10, we see a greater post-Watergate willingness (by 16 per cent) to envisage punishment for the President as the outcome of the episode and a lesser proclivity to describe him as getting away with speeding. The President's comportment in the episode is evaluated negatively by more post-Watergate children than those who evaluate him positively, a reverse of the direction of the positive-negative relationship in 1969–70, although the absolute shift in response distribution is not substantial.

The two themes reported in Tables 8 and 9 show the most striking pre- post-Watergate differences: The enunciation of the norm that all Americans are equal and the assertion that the President is above the law. The norm of equality is mentioned by 14 per cent more children in the

<sup>53</sup> F. Christopher Arterton, "The Impact of Watergate on Children's Attitudes Toward Political Authority," *Political Science Quarterly*, 89 (June, 1974), 269–324. Watergate has encouraged a spate of research, not all of which had been completed by the time this article went to print. See the special issues devoted entirely to Watergate of the *American Politics Quarterly*, 3 (October, 1975); and *Communications Research*, 1 (October, 1974).

<sup>54</sup> Greenstein, *Children and Politics*, p. 37.

<sup>55</sup> *Ibid.*, pp. 43–52.

<sup>56</sup> Gallup Opinion Index, September 1973.

Table 10. Responses of Pre- and Post-Watergate U.S. White Children to the President of the United States (percentages)

	Pre-Watergate Respondents	Post-Watergate Respondents
(1) Level of affect/idealization		
Positive	55	45
Mixed	0	2
Negative	1	5
Neutral	43	44
Not ascertained	1	3
	100	99
	(86)	(59)
(1a) Differentiated positive affect (Table 4)		
Explicit positive affect	29.1	20.3
Other explicit idealization	2.3	1.7
"Helps"	10.5	5.8
Does good things	9.3	8.5
Says what's right	1.2	1.7
Important	2.3	3.4
Must be obeyed	0.0	3.4
	54.7	44.8
(2) Outcome of traffic policeman episode (Table 6)		
President punished	41	57
President only cautioned	20	18
President gets away with speeding	39	25
	100	100
	(80)	(56)
(3) Evaluation of head of state in traffic policeman episode (Table 7)		
Positive	28	21
Mixed	2	5
Negative	20	26
Neutral	49	47
	99	99
	(81)	(55)
(4) President equal before law (Table 8)		
Norm explicitly stated	30	44
Norm implied	5	5
Norm not expressed	65	51
	100	100
	(82)	(59)
(5) President above the law (Table 9)		
Norm explicitly stated	21	14
Norm implied	10	44
No norm expressed	69	42
	100	100
	(82)	(59)

Coding Status: Consensual coding of items 1, 3, 4, and 5. Reliability of item 2 is 90 per cent agreement;  $\pi = .84$ .

post- than in the pre-Watergate group. The norm that the President is above the law increased even more substantially between the two interview periods. This increase is the most substantial difference between the two populations—27 per cent. Frequently, the same child refers to both

norms, stating either that everyone should be equal but that the President will use his status to evade punishment, or, alternatively, that equality triumphs in spite of evocation of the contrary notion that the President is above the law.

Here are examples of how post-Watergate



children enunciated norms in the President-policeman story:

The President says "I'm the President. You shouldn't give me a ticket." He thinks he's higher than everybody, but you're supposed to treat people equal. The policeman wasn't playing favorites, so he just gave him a ticket. [both norms evoked].

First of all, I don't think the police would stop him, but if he would, I don't think he'd do anything. . . . I think the police would be scared, sort of. [President above law].

Probably the policeman would stand at attention. I don't think they would do anything to the President. . . . They *should* do something to him, like they should give him a citation. Just because he's the President doesn't mean he's God. But they would probably be scared. [Both norms evoked]

I don't think he'd give him a ticket. But if he was a good policeman, he'd give him a ticket just as well as anybody else. On account that he's human just like anybody else. They get a ticket, he oughta get a ticket. If one gets away with it, they all should get away with it. [Norm of equality]

"Sorry, Sir, you're gonna have to pay a traffic ticket of fifteen dollars." "I'm the President, I'm not gonna pay *any* traffic ticket." "You are the one, the man that makes the laws, with all your help from Congress. . . . You make the laws, you should enforce them. You're not enforcing them right now." [Both norms evoked].

These post-Watergate American white children were considerably more likely to employ the President-above-the-law theme than were the two pre-Watergate groups most prone to exhibit the them—the French and the American blacks. At this relatively early stage in the unraveling Watergate scandal, the very substantial difference in the use of the above-the-law theme suggested a distressing possibility. Would the main effect of "stonewalling" by a president obdurately refusing to clarify his role in a scandal be to foster attitudes of quiescence and fatalism in children? With the 20/20 vision afforded by hindsight, it is apparent that although these children still idealized the presidential role in the abstract, they were prepared to punish a presidential malefactor.<sup>57</sup> By the end of 1973, when Nixon's popularity with the adult population plummeted to an all-time low for the six presidents on whom national survey data were available, children as well as adults were able to make numerous negative statements about the President, as Arterton's November 1973 study makes clear.<sup>58</sup> At that time many more children

would probably have joined the precocious suburban boy quoted above in envisioning the use of the "rusted blunderbus" of American politics—presidential impeachment.

### Summary and Conclusions

This study suggests the following touchstones for further research on Britain, France, and the United States.

(1) English children<sup>59</sup> begin their political socialization with benign views of the Queen, whom they regard as their nation's effective ruler, and much more balanced views of the Prime Minister. They perceive both the Queen and the Prime minister in considerable detail, and sense a highly complementary division of labor between the incumbents of these two offices.

(2) Fifth-Republic children, at least prior to Giscard d'Estaing's time in office, focus their attention almost solely on the President of the Republic, and perceive the Premier (whose name they commonly do not even know) as a minor official who stands in for the President when the latter is otherwise occupied. Their descriptions of the President and their perceptions of him in action are characterized by authoritarian imagery and vocabulary. They see him as a formidable personage rather than an attractive, positively valued figure.

(3) The benevolent-leader imagery used by white American children to describe the President during the early years of the Nixon Administration is evidence of their remarkable insulation from the turmoil of the 1960s. In spite of their idealized views, however, these 1969–70 respondents also revealed a capacity to criticize the President when presented with a hypothetical scenario involving presidential lawbreaking. A comparable group of American children interviewed early in the Watergate scandal perceived the presidential role in a less explicitly idealized fashion. The most important difference between the pre- and post-Watergate groups was the greater sensitivity of the post-Watergate children to the possibility that the President might claim to be above the law; but this claim evidently produced a backlash. The post-Watergate children were also more likely to say that all Americans are equal before the law, and to imagine that the policeman would punish the presidential traffic violator.

(4) The small 1969–70 sample of black children, which did not include respondents from the most depressed of the central city ghettos, produced a surprising number of idealizations of the President. Nevertheless, in contrast to the pro-Presi-

<sup>57</sup> For an extended discussion, see *New Society*, 30 (19 December 1974), 751–3.

<sup>58</sup> Gallup Opinion Index, December 1973. Also see note 52.

<sup>59</sup> All of the respondents in this study were from the more commonly studied South of England.

dent black populations studied during the Kennedy and early Johnson Administrations, a number of black respondents in the present study held solidly entrenched negative views of the President (more so of Mr. Nixon than of the presidency in general). Although black Americans ranked closer to their white American counterparts than to the children in the other two countries in their views of whether a law-breaking head of state would be punished, the black respondents were at the opposite end of the four-comparison-group continuum from the white American children in the use of one of the most long-standing themes in American political culture—the belief that all Americans are equal.

(5) Methodologically, the findings point in several directions. Detailed examination of open-ended data, especially from a population such as children, who are very vulnerable to response set, makes it possible to tap the diversity of private meanings and imagery that becomes artifactually homogenized in responses to fixed-choice items. Moreover, the use of open-ended items overcomes the problem encountered with fixed-choice procedures of interpreting purely item-produced responses as if they were salient, phenomenologically “real” political perceptions and beliefs. Both the story-completions items and the straightforward open-ended items also leave room for serendipitous findings not conceived as part of the original research design.

The story completions complement the open-ended questions in eliciting evidence of how children define situations and imagine behavior would occur in specific circumstances. The pair of items compared in this article—an open-ended question that appears to evoke expectations about the presidential role in the abstract, and a story-com-

pletion item dealing with contingencies under which the President breaks the law—provide an instructive juxtaposition. Discrepancies between responses to these items help explain why members of the same cohort of children who in the late-1950s and early 1960s strongly idealized the President became so deeply disillusioned by incumbents who did not live up to their expectations. If political-psychological data are to be predictive of behavior, questions about abstract attitudes and cognitions must be supplemented by contingent questions about how individuals respond in concrete circumstances where more than one value is implicit.<sup>60</sup> Therefore, the conventions of recent years that stress measurement of cognition and affect need to be expanded to take into account a third aspect of mental functioning—*conations*, i.e., dispositions toward action in specific circumstances.

Finally, this article stresses the desirability not only of multiple indicators but also of multiple methods that enable triangulations of observations on human dispositions and behavior. Instead of seeking an illusory, bias-free way of ascertaining the “truth,” one should progressively seek to specify variations in behavior depending on particular circumstances.

<sup>60</sup> For discussions of the centrality of contingent (or “interaction”) relationships in political psychology, see the preface to the Norton Library edition of Fred I. Greenstein, *Personality and Politics: Problems of Evidence, Inference and Conceptualization* (New York: Norton, 1975); the editors’ introductions to essays in Fred I. Greenstein and Michael Lerner, eds., *A Source Book for the Study of Personality and Politics* (Chicago: Markham, 1968, now distributed by Humanities press, Atlantic Highlands, N.J.); and Greenstein, “Personality and Politics” in Greenstein and Polsby, eds., *Handbook of Political Science*, II, 1–92.

## COMMUNICATIONS

### The Inertial Effect of Incumbency and Two-Party Politics: Elections to the House of Representatives from the South, 1952-1974.

TO THE EDITOR:

There has been a good deal of recent discussion in political science circles about the importance of incumbency as an "effect" which helps congressmen running for reelection by a given number of percentage points.<sup>1</sup> Although no attempt to measure the "incumbency effect" has been conclusive, recent elections to the House of Representatives have demonstrated its importance.<sup>2</sup> Despite this incumbency advantage, however, no one has drawn from it what is perhaps the most significant generalization to be made: that incumbency acts as an important contextual distortion of the distribution of party identification among the electorate. Since most incumbents are reelected despite the turbulent issues of a particular campaign, incumbency can act as an inertial influence on voting behavior, prolonging a party's success in a series of elections, despite underlying changes in party identification.

The South, because it is the only region in the United States displaying a major secular trend in changing party identification, provides an unusual opportunity to test this generalization. By com-

<sup>1</sup> The degree to which incumbency is a separable "effect" in itself is a matter of dispute. Traditionally, it has been assumed that incumbency is a distinct advantage to congressmen seeking reelection. Warren Lee Kostroski has spelled out the advantages commonly cited as working in favor of incumbents: they are likely to accumulate considerable political skill and experience, have better voter recognition than their nonincumbent opponents, and have some clout within their party because of favors, patronage, and fund-raising activities. Robert S. Erikson, in 1971, suggested that incumbency was worth about 2 per cent of the vote. A year later, Erikson suggested that while 2 per cent was probably the valid figure for the 1950s, by the late 1960s, the advantage had more than doubled to about 5 per cent of the vote. See Warren Lee Kostroski, "Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models," *American Political Science Review*, 67 (December, 1973), 1217; Robert S. Erikson, "Research Note: The Advantage of Incumbency in Congressional Elections," *Polity*, 3, No. 3 (Spring, 1971), 395-405; Charles M. Tidmarch, "Communications: A Comment on Erikson's Analysis of the Advantage of Incumbency in Congressional Elections," *Polity*, 4, No. 4 (Summer, 1972), 523-529; and Erikson's response to Tidmarch.

<sup>2</sup> Of those congressmen who decided to run for reelection in the past decade, roughly 90 per cent of the Representatives and 80 per cent of the Senators have been reelected. In 1974, comparatively a year of wholesale slaughter, only 10.4 per cent of the Representatives seeking reelection were defeated. From 1966 to 1972, about 86 per cent of the entire membership of the House were incumbents. See Kostroski, "Party and Incumbency."

paring elections in the South involving incumbents with those involving nonincumbents, I will show that incumbency may substantially delay changes in voting which might otherwise have been expected given changes in party identification.

One could hardly make a less surprising announcement than to proclaim the demise of the Democratic "solid South."<sup>3</sup> The South has not been completely solid in presidential elections since the last Roosevelt victory in 1944. In 1952, 94 per cent of southern congressional districts were Democratic, compared to just 69 per cent in 1972 and 75 per cent in 1974. This increasing Republican share of House seats, however, seems small when compared to changes in party identification and voting in presidential elections. Table 1 gives the available party identification figures in the South for the last six presidential elections, and compares this with the percentage of House seats won by Republicans and Democrats in the same period. Democratic identification steadily declined during the period 1952 to 1974, while identification with the Republican party increased. It appears that the distribution of congressmen by parties has not changed proportionately with changes in the underlying partisan alignment. Democrats have consistently maintained an edge in seats greater than one might expect on the basis of party identification figures. Table 2 makes it clear that this edge results from the inertial effect of incumbency.

Table 2 compares elections to the House of Representatives in the South involving incumbents from 1952 to 1974. The Table indicates, first, that southern Democratic incumbents have been more successful in being reelected than southern Republican incumbents. Democrats defeated about nine per cent of southern Republican incumbents over the twelve elections depicted (Column A), and lost only 3.3 per cent of their incumbent seats—a 96.7 per cent record of successful reelection (Column B). Second, despite this track record of incumbent success, there has been a nearly perfect downward trend in the Democratic proportion of southern congressmen since 1960. The overall proportion of southern Democratic congressmen declined from 94 to 75 per cent, as indicated in Column E.

Columns C and D demonstrate that this Democratic erosion has occurred in elections involving nonincumbents. Column C shows the proportion

<sup>3</sup> In this regard, see Raymond E. Wolfinger and Robert B. Arseneau, "Partisan Change in the South, 1952-72," paper delivered at the Annual Meeting of the American Political Science Association, August 29-September 2, 1974. I define "the South" as the eleven former Confederate states.

**Table 1. Percentage Decline of Southern Democratic Party Identifiers, and Seats in the House of Representatives, 1952 to 1974**

	1952	1956	1960	1964	1968	1972
Per Cent Democratic Party Identifiers	75	65	65	72	67	58
Per Cent Democratic Seats in House	94	93	93	84	75	69

Source: Michigan Survey Research Center data on party identification.

**Table 2. Elections to the House of Representatives from the South, 1952 to 1974, Involving Incumbents and Nonincumbents, by Party<sup>a</sup>**

	A	B	C	D	E
	% Incumbent Republican Seats Won by Democrats	% Incumbent Democratic Seats Won by Democrats	% of Nonin- cumbent seats Won by Democrats	Difference between B and C	% of Total Seats Won by Democrats
1952	0% (0/2)	98% (83/85)	89% (17/19)	9	94 (100/106)
1954	17 (1/6)	99 (89/90)	90 (9/10)	9	93 (99/106)
1956	0 (0/7)	100 (91/91)	100 (8/8)	0	93 (99/106)
1958	13 (1/8)	100 (94/94)	100 (4/4)	0	93 (99/106)
1960	0 (0/6)	100 (93/93)	86 (6/7)	14	93 (99/106)
1962	0 (0/6)	98 (85/87)	77 (10/13)	21	90 (95/106)
1964	18 (2/11)	95 (79/83)	67 (8/12)	28	84 (89/106)
1966	13 (2/15)	93 (71/76)	67 (10/15)	26	78 (83/106)
1968	0 (0/22)	100 (72/72)	67 (8/12)	33	75 (80/106)
1970	0 (0/22)	100 (77/77)	40 (2/5)	60	75 (79/106)
1972	0 (0/23)	97 (61/63)	59 (13/22)	38	69 (74/108)
1974	26 (9/34)	100 (68/68)	67 (4/6)	33	75 (81/108)

<sup>a</sup> Table 2 was calculated from election results reported in Congressional Quarterly Weekly Reports, Congressional Quarterly Service, Inc., Washington, D. C., 1950 to 1974. An election was counted as "incumbent" if any individual running was in office at the time of the election.

of elections where no incumbent was running which were won by Democrats. In the 1952 to 1958 series of elections, if the figures are averaged, Democrats won a comfortable 93 per cent of the 41 nonincumbent seats, and 99 per cent of the Democratic incumbents were reelected. By the 1968 to 1974 period, however, on the average, while 99 per cent of the Democratic incumbents were still reelected, Democrats won only 60 per cent of the 45 nonincumbent races.

As hypothesized above, incumbency is a stable crust underneath which Republicans as well as Democrats are contesting elections with increasingly improved chances of victory. The Democratic proportion of House seats exceeds the Democratic proportion of party identifiers because southern Democratic incumbents are nearly invulnerable. When finally they retire, however, only about six in ten of the Democrats running for the same seats at present are able to retain their seats. The change in the partisan temperament of the district has already occurred. It is the effect of incumbency which was responsible for the delay in changing the party holding the seat. From this we must conclude that incumbency can have a considerable effect on the outcome of elections and is not simply an influence which reflects the underlying party identification.<sup>4</sup>

Column D makes this inertial effect of incumbency explicit, and it shows that the difference between the success of southern Democratic incumbents and of nonincumbents has been steadily increasing. In the 1950s, Democrats won only 6 percentage points fewer of the nonincumbent races than of the races involving incumbents, on the average. By 1960, however, this gap between the two sorts of contests increased to an average of 39 per cent in the elections from 1968 through 1974. In other words, when the advantage of incumbency was removed, Democrats tended to win fewer and fewer elections than when it was present. It appears that the longer-range prospects for Republican success in southern congressional elections are better than a cursory examination of the numbers of southern Republican congressmen might indicate.<sup>5</sup>

<sup>4</sup>This indication that incumbency delays changes between parties suggests the possibility that incumbency may act to delay change *within* a party as well. Given the "nationalization" of southern politics, the Democratic incumbents are more likely to be "old style" southern Democrats, while the Democratic nonincumbents are more likely to be national Democrats, virtually indistinguishable from their Northern colleagues. Hence, incumbency also may act to delay change within the Democratic party.

<sup>5</sup>Wolfinger and Arsenau also document this point strikingly by differentiating "contested" and "uncontested" elections to the House. See Wolfinger and Arsenau, p. 4.

In one sense, it is absurd to speak about the congressional delegation from the South as being more representative of Democrats "than it should be." Incumbents and nonincumbents are elected in the same way. And to the degree that citizens know anything about the substantive positions of the candidates running for office, they probably know more about incumbents than about nonincumbents. On the other hand, the results of elections involving nonincumbents lead to the expectation that should all southern congressional incumbents suddenly retire, their elected replacements would include considerably fewer Democrats.

The three general findings of this communication can be summarized as follows: First, incumbency has a considerable effect on the outcome of elections, an influence that is clearly not merely an artifact of a constituency's underlying partisan loyalties. Second, the data indicate that incumbency can delay partisan change in a region's representation in Congress considerably. And third, the erosion of Democratic strength in the South appears to be greater than many observers have realized.

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#### B. F. Skinner and Values

##### TO THE EDITOR:

Professor Stillman's argument (*APSR*, 69 [March, 1975], 202-213) that B. F. Skinner's position regarding values is philosophically inconsistent can be summarized as follows:

- (1) If something is a value, then that thing has a reinforcing effect (specifically, something "good" is something which positively reinforces, something "bad" is something which negatively reinforces).
- (2) If something has a reinforcing effect, then that thing is subjective (i.e., all reinforcing effects are the perceptions of specific sentient individuals such that no reinforcing effect experienced by any two or more specific sentient individuals need necessarily be identical).
- (3) Therefore, if something is a value, then that thing is subjective.

Viewing points (1) and (2) as premises, conclusion point (3) follows necessarily by means of the inference rule known as "hypothetical syllogism." Symbolically the argument can be represented as follows:

$$(x)(V_x \supset R_x)$$

$$(x)(R_x \supset S_x)$$

$$(x)(V_x \supset S_x)$$

where "*V*" stands for "is a value," "*R*" symbolizes "has a reinforcing effect," and "*S*" sym-

bolizes "is subjective." Professor Stillman believes Skinner is inconsistent as soon as he posits survival as a positive value and yet admits that certain forms of destructive behavior (e.g., enjoying heroin) may be positively reinforcing. This problem, however, is too simply put.

To begin with, even should Skinner accept premise (1) positing that all values are reinforcing, this premise does not necessarily imply that all reinforcing things are values.<sup>1</sup> The full escape is achieved once Skinner maintains that species survival is a value unlike all other values in that it functions as the decision criterion in the system.<sup>2</sup> Thus, when two or more things presented as choices for behavioral control are each positively reinforcing, those contributing to the standard of species survival are preferred over those detrimental to the species survival standard, and those equally contributing to the standard are ranked indifferently. Why else would Skinner need a behavioral engineer or talk about redesigning environments? To point out that some men take heroin or that moths fly into flames is merely to point out that those men and moths are stupid (i.e., in Skinner's terminology, they have designed or had designed for them reinforcement schedules which entail the ultimate negative reinforcement

consequence, or at least the cessation of all reinforcement). It does not demonstrate that the system is inconsistent. On this construction, Skinner falls less into an Epicurean tradition (that all types of pleasures are equivalent provided they are equally pleasant) than he does into a Benthamite tradition (that a criterion, such as the greatest happiness principle, distinguishes the more perfect from the less perfect pleasures).

Yet there are deeper levels upon which the issue of logical consistency can be raised. Possibly species survival itself might become negatively reinforcing if circumstances were such that to be alive were to be alive painfully. If species survival itself becomes negatively reinforcing then the rational standard has departed from its reinforcement base and the system has become inconsistent with premise (1) in that the valued thing, species survival itself, is no longer positively reinforcing. Professor Stillman presented a variation of these difficulties as a political problem of countercontrol, i.e., how do the experts proceed when what is necessary for species survival is not positively reinforcing to individuals. The problems he raised there are genuine and important. However, looked at here, they reveal that the logical grounds upon which the system stands may entail an abandonment of the contention that the valuable is rooted in the reinforcing. Premise (1) functions more as an article of faith than an empirically demonstrated synthetic postulate given the development of the system. Either the valuable is linked to the reinforcing by analytic definition (here a form of question begging which would assert that  $(x)(R_x \equiv V_x)$  rather than the material implication originally stated), or the behaviorist merely hopes rather than demonstrates that all things positively valued (i.e., all things following as deductions from the species survival criterion) will be positively reinforcing. In the first instance a nonempirical operation, and in the second instance a nonempirical assumption, rests squarely at the base of a system tolerating only empiricism.

JEROME R. CORSI

Franconia College

TO THE EDITOR:

Despite Professor Corsi's argument, I hold to my original formulation. It seems to me that Skinner equates values and reinforcement by his stipulative definition of values, so that the valued and the reinforcing are identical by definition (or faith), without the need for syllogisms. From this identity follow the subjectivity of values and Skinner's contradiction in seeing survival as a special value, i.e., the objective decision criterion of his system. Skinner clearly wishes to escape this contradiction, and to justify survival as a

<sup>1</sup> At first glance it may appear that Professor Stillman has stated premise (1) differently than posited here. On p. 206 of his article, for instance, the following sentence appears: "Thus, things are good if they are positively reinforcing, bad if they are negatively reinforcing." That this should be read  $(x) (V_x \supset R_x)$  is apparent from the proper translation of the horseshoe symbol into the English words "only if," as in the statement " $p$  only if  $q$ " (see W. V. Quine, *Methods of Logic* [New York: Holt, Rinehart and Winston, Third Edition, 1972], p. 44). Absent the quantification terminology, the argument as it presently stands is a valid categorical syllogism of the form AAA-1 (note what is stated as the second premise would have to be considered the first since it contains the major term). If what is written here as premise (1) were cast  $(x) (R_x \supset V_x)$ , there would be no argument at all as the syllogism would then be of the invalid form AAA-3.

<sup>2</sup> That Skinner does utilize species survival as a decision criterion is articulated in many of his writings. Consider, for example, the following:

A rigorous science of behavior makes a different sort of remote consequence effective when it leads us to recognize survival as a criterion in evaluating a controlling practice. We have seen that happiness, justice, knowledge, and so on are not far removed from certain immediate consequences which reinforce the individual in selecting one culture or one practice against another. But just as the immediate advantage gained through punishment is eventually matched by later disadvantages, these immediate consequences of a cultural practice may be followed by others of a different sort. A scientific analysis may lead us to resist the more immediate blandishments of freedom, knowledge, or happiness in considering the long-run consequence of survival.

*Science and Human Behavior* (New York: The Free Press, 1965 paperback edition), pp. 435-436.

nonsubjective and validly derived decision criterion. But he equally clearly has not yet escaped, for he has not given an objective or a valid derivation as a special value, nor has he shown how survival could in practice operate as a decision criterion (e.g., he has not treated the practical difficulties of accurately and fully predicting the future so that the behavioral engineers will be able to assess the survival potential of different courses of action).

But Skinner's lack of precision in writing does make him difficult to interpret. Thus, Professor Corsi, not I, may well be correct. In either case, however, it is clear that Skinner does fall into serious contradiction. Furthermore, in his final paragraph and especially in his final point, Professor Corsi does raise additional telling objections to Skinner's social and political thought.

PETER G. STILLMAN

Vassar College

#### More on The Pentagon Papers

TO THE EDITOR:

As a footnote to the review-articles on *The Pentagon Papers* by George McT. Kahin and H. Bradford Westerfield (*APSR*, June 1975), the four "negotiating volumes" originally omitted from the Gravel and Hebert editions have recently been made public (although marred by lengthy deletions) and they can be purchased by sending a check for \$30.80 to Mr. Charles W. Hinkle, Director, Freedom of Information and Security Review, Room 2C-757, Pentagon, Washington, D.C. 20301. The check should be made payable to the Treasurer of the United States.

Interested readers will also wish to consult the excellent articles on the negotiating volumes by Murrey Marder and Don Oberdorfer in *The Washington Post*, June 27, 1972 and by Neil Sheehan and Bernard Gwertzman in *The New York Times*, June 28, 1972.

WALLACE J. THIES

University of Connecticut

#### More on New Theories of Revolution

TO THE EDITOR:

Mr. Bernard S. Morris, in reviewing my book, *New Theories of Revolution* in the December 1974 issue of your journal, ends by stating that Fanon, Debray and Marcuse "may be wrong, but at least they treat theory as a creative tool instead of clinging, as Woddis does, to the conventional ideas of Marxism-Leninism in a radically changed world."

Using Marxism-Leninism as a creative tool does not consist of overturning the main ideas of Marxism-Leninism, without any evidence whatsoever to back up such "creativity." The test of any new theory is whether or not it is confirmed by the

facts, by experience, by the revolutionary processes themselves. As I explain more than once in my book, it is vital for revolutionaries not to regard every Marxist idea as sacrosanct if developments in life show it to be no longer valid. But it is my contention, which I have presented at length backed up by a detailed and factual examination of the theories of Fanon, Debray and Marcuse as they have worked out in practice in *Africa, Latin America and the Western world*, that in reality all their theories have been found wanting when put through the severe test of reality.

If I had simply attempted to answer them by dogmatic reassertions of the views of Marx and Lenin there might have been some point to Mr. Morris's criticism. But as he himself admits at the beginning of the review, I offer "a prodigious collection of data" and it is this, not a mechanical clinging to theoretical tenets, which I have presented as my evidence. To date, no reviewer anywhere has been able to challenge the facts of my case.

JACK WODDIS

Essex, England

TO THE EDITOR:

A prodigious collection of data does not necessarily add up to a validation of theory, in this case, a conventional rendition of Marxism-Leninism. The theories of Fanon, Debray and Marcuse may be found wanting but not by virtue of reference to the texts and tenets of Marxism-Leninism.

BERNARD S. MORRIS

Indiana University

#### More on Participation

TO THE EDITOR:

In her review of my monograph *Participation, Planning and Exchange in Old and New Communities: A Collaborative Paradigm* (*APSR*, 68 [Dec. 1974], 1766-67), Judith May makes three basic criticisms:

(1) I am more successful in creating a normative ideal, citizen-government collaboration, than a descriptive model of citizen participation in planning.

(2) I use the social control, partisan influence, and exchange perspectives to describe both the evaluative orientations of observers of participation and the relative power of participants, expecting a change in relative power to follow a change in evaluative orientation.

(3) I fail to recognize the real world of participation with its economic inequities, racial conflicts, political history, federal programs, and the like, instead testing the usefulness of the collaborative approach in two new towns—a test that "ignores the political and historical context of citizen participation and trivializes the transforma-

tive citizen participation movement of the 1960's."

In response, I cheerfully plead guilty to the first two charges, and suggest that the third charge be thrown out for lack of evidence.

On the first count, it was my *objective* to create a normative model of collaborative participation, in order to provide a bench mark against which various public participation programs could be measured. My thesis was that, without such a model, it is difficult to evaluate the effectiveness of participation programs that have been attempted, or to design new participation programs. There are several descriptive models of citizen participation; among those that I reviewed are the ones by Gamson, Arnstein, Mogulof, Verba, Gans, Spiegel and Mittenthal, Kaplan, and Moynihan. Most of the instances of public participation using these models are judged by the authors describing them to be failures. Yet these descriptive models of failed programs do not necessarily provide useful guidelines for evaluating or designing effective participatory planning programs.

On the second count, it also was my objective to provide a changed perspective from which to view participation, on the grounds that until such a change has occurred there will be little chance of a shift in the relative power of participants. Unless the designers of participation programs see them as more than after-the-fact educational efforts for controlling and persuading the public, or as defenses against aggressive partisan interest group efforts, then the present disparities in planning power are bound to continue. Only if planners and public officials understand that there is a workable alternative to social control, and the interest groups understand that there is a workable alternative to partisan influence, can they take the necessary steps to create authentic power sharing. I did not claim that such a change in orientation was a sufficient condition for achieving collaborative planning, only that it was necessary to develop a concept of reality for public participation that looks beyond the narrow limits of descriptive social science. In proposing this new paradigm I am not alone, but share the distinctive company of Amitai Etzioni (*The Active Society*, 1968), Edgar Dunn (*Economic and Social Development*, 1971), John Friedmann (*Retracking America*, 1971), and Donald Michael (*On Learning to Plan—and Planning to Learn*, 1973). Fortunately, this new reality also is beginning to be recognized in some public programs of the 1970's such as transportation (the Boston transportation planning review is an important example) and water resources planning (the new federal 208 program has strong and explicit participation guidelines).

On the third count, apparently Prof. May sub-

stituted her own values toward participation for the facts from my monograph. Otherwise, she could not have supported her sweeping criticism of my paradigm on the grounds that it ignored the realities of economic inequities, racial conflicts, political history, federal programs, mayors, bureaucracies, etc. In fact, I reviewed six cases from the literature, including Boston's South End Urban Renewal Project; the Model Cities Programs in Atlanta, West Oakland, and North Philadelphia; and the new communities of Levittown, New Jersey, and of Fort Lincoln. These cases, which were replete with economic inequities, racial and political conflicts, and all the other elements that were supposedly left out of my study, were then compared with my field studies of Reston and Columbia. In fact, I presented a general model of the community environment for planning and governance, which identified as important community context variables the community's demography, geography, economy, political history, organizational structure, and influence pattern. In fact, I devoted a chapter to tracing the transformation of the citizen participation movement of the 1960s, from the typical blue ribbon advisory committee of the early days of Urban Renewal to the radical community conflicts of some Model Cities and War on Poverty programs. In fact, I found in Reston and Columbia much more important instances of participation than the responses to surveys that Prof. May mentions, including a level of activism and involvement in community development issues that was high for American communities. In fact, the collaborative paradigm explicitly recognizes problems of unequal power, and of barriers to acceptance of innovation and change.

In light of these facts, I feel that Prof. May's review has ignored the full context of my work and has trivialized the importance of my concepts, in order to emphasize her own viewpoint about what belongs within the citizen participation movement of the 1960s. Perhaps readers with different biases will accept a wider range of experience and ideas.

DAVID R. GODSCHALK

University of North Carolina at Chapel Hill

#### TO THE EDITOR:

I regret having used the word "trivializes" to describe David Godschalk's treatment of citizen participation. Let me reiterate my criticism in other words.

In his monograph, Godschalk creates a normative ideal for citizen participation in planning, which he characterizes as citizen-government collaboration (the exchange perspective), and which he distinguishes from citizen-government relationships dominated either by the authorities



(the social control perspective) or by citizens (the partisan influence perspective). In doing so, he reviews an enormous amount of literature, including case-studies. However, Godschalk uses this literature to demonstrate the usefulness of distinguishing among the social control, partisan influence, and exchange perspectives, and not to "ground" his theory by identifying the conditions under which each is likely to occur. Although he lists certain contextual variables in "a conceptual model of the community environment for planning and governance" (demography, geography, economy, political history, organizational structure, and influence pattern), he makes little effort to identify the properties of these variables and to link them in systematic ways with aspects of citizen participation in planning. Compare Godschalk's conceptual elaborations (e.g., "*Organizational structure* includes the formal legal structure for planning and governance, as well as the political parties and voluntary organizations. This would include both authorities and partisan interest groups.") with those of J. David Greenstone and Paul E. Peterson in *Race and Authority in*

*Urban Politics* or Roland L. Warren, Stephen M. Rose, and Ann F. Bergunder in *The Structure of Urban Reform*.

Godschalk hopes that changing perspectives on citizen participation will help to change the relative power of the participants. I would like to have seen more attention paid to the conditions under which his normative ideal is likely to be realized. How much authority over what kinds of decisions at what stages of the decision-making process will permit citizens to approach parity with authorities? What kinds of incentives arranged in what kinds of structures will motivate authorities to share decision-making responsibility with citizens? What kinds of conflict will be encouraged by the pursuit of what kinds of goals with what consequences for the durability of the authority-sharing arrangements? Without this careful specification of conditions, Godschalk has difficulty fulfilling the task he has set for himself—judging whether, in any particular case, citizens and authorities have achieved a proper balance.

JUDITH V. MAY

*Rutgers University, Newark*

## Politics of the French Left: A Review Essay\*

NANCY I. LIEBER

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A decade ago the French political scene was characterized by the existence of a majority rightist party whose ascendancy seemed assured, and a muddled and disparate left which had barely regrouped once again in the opposition camp. Today the French political scene is characterized by the existence of a new rightist majority whose structure and ideology remain as yet only loosely defined, and a renovated and united left in a position to achieve governmental power—which it very nearly did in May 1974. Yet the left's struggle for realignment has tended to go relatively ignored by American political scientists for a variety of reasons: the primacy in the political system of the rightist majority, a fascination with unique political phenomena such as Gaullism, and an assumption of the French left's inability to offer anything in the way of successful political opposition. The disparate and therefore nonviable status of the left seemed a foregone conclusion, reflected in the old dictum that "while nothing could be done without the Communists, neither could anything be done with them." But while the electoral history of the Fifth Republic had proved that nothing in fact could be done by the left without the Communists, profound changes within both the *Parti communiste français* (PCF) and the democratic socialist SFIO sud-

denly made it seem by 1971 that a good deal could—after all—be done *with* the Communists. The result was the creation a year later of a Popular Front-type alliance among Socialists, Communists, and leftist Radicals. This virtually unprecedented alliance differed from the historic 1936 Popular Front agreement in that the PCF was now willing to enter a leftist coalition government and it differed from the 1945–47 alignment mainly in that the alliance was cemented by the signing of an extremely detailed Common Governmental Program designed to "open the way to socialism in France."

Eight recent French publications illuminate how the French left grappled with and eventually overcame the immobilization that had plagued it so long under the Fifth Republic. Together, they portray the new Popular Front alliance emerging as an alternative to Gaullism; individually, they reveal most of the salient themes, problems, and issues that have confronted the left in the Fifth Republic. These include the structural and philosophical metamorphosis of the old democratic socialist left into the present *Parti socialiste* (PS) of François Mitterrand, the Communists' concurrent *rapprochement* with the Socialists made possible by the PCF's ideological revisionism, the culmination of the Popular Front strategy in the form of the *Programme commun de gouvernement* of June 1972, the role of François Mitterrand, and the Communists' present state of theorizing. From an analysis of these works, it may be concluded that the Popular Front alliance has increased the possibility that, for the first time since World War II, the Socialists will displace the Communists as the predominant party on the left.

### The New Socialist Party

One of the most consequential changes on the left concerns the transformation and rejuvenation of the former SFIO (a party on the decline since 1945 in terms of membership, votes, and vigor) into the new *Parti socialiste*, which is analyzed by Pierre Guidoni in *Histoire du nouveau parti socialiste*. While the author (a political scientist and left-wing PS militant) deals with developments from 1968 to 1973, his most original contribution lies in

\* Pierre Guidoni, *Histoire du nouveau parti socialiste* (Paris: Tema-Action, 1973. Pp. 405. 26 F.)

Didier Motchane, *Clefs pour le socialisme* (Paris: Editions Seghers, 1973. Pp. 310. 14 F.)

André Laurens and Thierry Pfister, *Les nouveaux communistes* (Paris: Stock, 1973. Pp. 262. 23 F.)

*Programme commun de gouvernement: Parti socialiste, Parti communiste, Mouvement des radicaux de gauche* (Socialist edition, Paris: Flammarion, 1973. Pp. 96, 3.50 F; Communist edition, Paris: Editions sociales, 1972. Pp. 192. 5 F.)

François Mitterrand, *La Rose au poing* (Paris: Flammarion, 1973. Pp. 224. 16 F.)

François Mitterrand, *Ma part de vérité* (Paris: Fayard, 1969. Pp. 192. 14.50 F.)

Jean-Marie Borzeix, *Mitterrand lui-même* (Paris: Stock, 1973. Pp. 220. 25 F.)

George Marchais, *Le Défi démocratique* (Paris: Grasset, 1973. Pp. 249. 15 F.)

the treatment of the PS's 1969–1971 "transitional period." The July 1969 "constituent congress" of the PS had marked an important step in uniting the non-Communist left by accomplishing the fusion of the SFIO, several political club groupings (such as the UCRG of Alain Savary, the UGCS of Jean Poperen), and a small percentage of members from Mitterrand's *Convention des institutions républicaines* (CIR). With the renovator Savary as the top leader, this regroupment was expected to permit the "great leap forward" so desperately needed in the democratic socialist camp. But Guidoni makes it clear that any dynamic movement was extremely difficult to achieve because of the new leadership's failure to accomplish most of the four main tasks assigned to it.

First, despite giving the highest priority to renovation, renewal, and growth, the party experienced very little progress in any of these areas. New memberships still accounted for only 10–20 per cent of the total membership;<sup>1</sup> the much-heralded turnover (50 per cent by 1970) of Federation heads in many cases simply meant that a former SFIO member had been replaced by a younger former SFIO member; the majority of seats on the *Comité directeur*, *Bureau*, and *Secrétariat* were held consistently by former SFIO members rather than by former club or minority CIR members. So while Savary was highly regarded as an honest and scrupulous party leader, this respect was not matched—as it had to be—by his actual control of the leadership organs or by a widening of his political base at the grass-roots level.

Savary's second task concerned the formation of a party program and an electoral strategy. Drafting a program presented no problems within the party ranks; in fact, the party's *Plan d'Action* of 1970 very closely resembled the CIR's 1969 program<sup>2</sup> and the PSU's Manifesto of that year.<sup>3</sup> Electoral alliances proved to be quite another matter. The right wing of the PS expected to retain the liberty to make local and cantonal alliances with essentially everyone left of the Gaullists. The party's left wing, led by the CERES,<sup>4</sup> expected the PS to enforce its newly adopted Socialist-Communist

alliance commitment right down to the bedrock of Socialist support—the municipalities. When Savary pushed through a very loose compromise solution at an October 1970 conference, the party's internal tensions began to become public as the CERES unhesitatingly revealed its growing fear that the party had not progressed beyond "Molletism" and that under the circumstances the *bond en avant* expected after 1969 had no real chance of occurring.

Guidoni discusses how further criticism within the party was fueled with the completion in December 1970 of Savary's third task—exploratory ideological talks with the Communists. The commitment to "talks leading to a common governmental program" had been a key plank in the 1969 final congressional motion. Yet, after one and a half years (with six weeks of actual discussion), Savary managed only to present to the party a balance-sheet of several pages, listing some ideological convergences but mainly a multitude of divergences between the Socialists and Communists. This was dismal, indeed.

Only in his final task—uniting the socialists—did Savary achieve success. And ironically, by accomplishing that task, he lost his position as First Secretary of the party to François Mitterrand. Ever since the spring of 1969, when the CIR-SFIO fusion plans were disrupted,<sup>5</sup> Mitterrand and the CIR had not ceased to call for unity. By November 1970, Savary felt he could no longer put off the inevitable, and accepted Mitterrand's proposal for the creation of a *Délégation nationale pour l'unité des socialistes*, which would work out arrangements for a grand Socialist Unity Congress in the summer of 1971.

Guidoni's portrait of the Savary transition years is excellent. He discerns, under the surface flow of events, the ideological divisions and *tendances* of a party trying to move leftward from its stagnation under Mollet. Guidoni elucidates the influence and standing of his own CERES wing, the nature of its militancy, and the radicalness of its democratic socialist ideology. Such an emphasis is valuable precisely because that CERES viewpoint prevailed to a great extent at the 1971 Unity Congress at Epinay. But Guidoni does not tell how such a dramatic change occurred within the seemingly stalled PS. A chapter in Jean-Marie Borgeix's *Mitterrand lui-même* tells that fasci-

<sup>1</sup> Hurtig Christianne, *De la SFIO au nouveau parti socialiste* (Paris: Colin, 1970), p. 91.

<sup>2</sup> The CIR's program can be found in Mitterrand's *Un socialisme du possible* (Paris: Seuil, 1970), pp. 43–107.

<sup>3</sup> *Manifeste du Parti socialiste unifié* (Paris: Tema-Action, 1970). The PSU is a splinter group of "revolutionary democratic socialists," formed in 1960.

<sup>4</sup> The *Centre d'études, de recherches et d'éducation socialistes*, a ginger group, was originally set up in the

SFIO by Mollet in 1966, but early on parted from his patronage.

<sup>5</sup> The disruption had been caused by the SFIO's unilateral actions regarding the creation of the new socialist party and its actions in the candidate selection for the June 1969 presidential race.

nating story,<sup>6</sup> although even his lively account is not complete. The truth of the matter is that things were not so straightforward as the public and the press thought in the months preceding the unity congress. As early as October 1970, secret meetings had begun among a handful of men who sought to affect the outcome of the by-then inevitable unification. They included Gérard Jaquet and Gaston Defferre from the Bouches-du-Rhône/Nord wing of the PS, Jean-Pierre Chevènement, Georges Sarre, and Didier Motchane from the CERES, and Pierre Joxe and François Mitterrand from the CIR. Their accumulated list of personal and ideological grievances against the incumbent party leadership had gone unnoticed owing to the all-too-obvious differences among them—particularly between the CERES and Defferre-Mauroy wings of the PS. But their formidable common interest—"to get rid of Guy Mollet once and for all," as they bluntly put it—brought them together to cut the Gordian knot so long entangling the French democratic socialists. A tacit and loose agreement emerged from these meetings. If the situation permitted, the alliance members would "bypass" Savary and Mollet by putting through a motion unequivocally calling for the formulation of a common governmental program with the Communists in anticipation of the forthcoming legislative elections. Such a strategy represented a complete about-face for Defferre and Mauroy, but they reasoned that, *provided* the PS became truly renovated and strengthened, it *could* hold its own in any such arrangement with the PCF. Believing that only Mitterrand could accomplish this reversal, Defferre and Mauroy were ready to join an alliance which would permit his ascendancy.

The almost fortuitous realization of the *coup d'Epinay* on that fateful first day of the unity congress is a story whose details are fascinating but too intricate to explore here.<sup>7</sup> Suffice it to say that the delegates, in voting on the technicalities of the party's internal electoral system, unknowingly determined the direction and leadership of the party. Because of these votes, Mitterrand's final motion, openly supported by Defferre, Mauroy, the CERES, and the CIR, became the majority motion and its supporters the majority on the party's ruling bodies. Savary, stung by the "immoral alliance of extremes" that confronted his leadership, refused to support a composite motion, thereby losing his position as party leader.

<sup>6</sup> Borzeix, pp. 163–184. Borzeix is a journalist.

<sup>7</sup> For a very detailed and lively account, see Guidoni, pp. 161–203.

### Democratic Socialist Ideology

In *Clefs pour le socialisme*, Didier Motchane (a political scientist and left-wing PS militant) complements Guidoni's description of the structural evolution of the PS by analyzing its ideological shifts. These have brought the PS around to a more revolutionary democratic socialism which distinguishes the French Socialists as probably the most radical and innovative in Western Europe. Motchane offers a comprehensive treatment of the "socialist revolution," but two major themes of the book illustrate the nature of current (non-Communist) Marxist thinking. The first criticizes "those two endemic sicknesses of the working movement," social democracy and bureaucratic centralism. The second weds the decentralizing notion of *autogestion*, or self-management, to the nationalization/democratic planning aspect of state socialism.

According to Motchane, the unconscionable deviation of the European social democrats is that their socialism is concerned solely with the *redistribution of the fruits of a capitalist society*. Instead of attempting to abolish classes that arise from unevenly divided relationships of production, the social democrats seek to "merge" these classes through a fairly even distribution of material and social benefits. Motchane traces this "socialism of resignation" to the days of Eduard Bernstein, when the revolution was deemed impossible and efforts turned to more easily obtainable legislative reforms. This shift away from original Marxism led the social democrats to rely almost exclusively on electoralism, and hence reduced them to attempts at changing the bourgeois institutional *superstructure* instead of that which ultimately determined it—the economic *substructure*. Before long, the social democrats became absorbed into the very bourgeoisie they supposedly opposed, since successful electoral battles necessitated moderation, placidity, and, above all, collaboration of classes in the form of electoral alliances with nonsocialists at all levels.<sup>8</sup>

If the PS had to guard within itself against a return to social democratic practices of the former SFIO, it also had to guard within the Popular Front alliance against a tendency toward bureaucratic centralism. The USSR (as-

<sup>8</sup> This reproach of electoralism is a major theme of Guidoni. He notes the demise of the social democratic current in 1969, when Gaston Defferre scraped together a humiliating 5 per cent in the presidential election first round: "the glaring defeat on their last terrain—that of elections—where their realist strategies could still pass as authoritative." Guidoni, p. 403.

suming it was still the PCF's model for socialism) was the land of the revolution, writes Motchane, but it did not become the land of socialism. Instead of using public ownership as a means to change social relationships, the Soviets have used it as unimaginatively as the social democrats have, simply going further in a redistribution of the collectively produced benefits. Motchane rejects the argument that the USSR has fulfilled Marx's basic conditions for socialism. To say that exploitation in the USSR has been eliminated because all surplus value is returned to the worker through a State which is run by the Communist party and which is itself synonymous with the workers is doubly fallacious. First, in the absence of political democracy, it has never been established that the interests of the State and party bureaucracies are synonymous with those of the workers. Second, jurisdictional ownership by the State is not *social ownership*, which requires the direct ownership, control, and management of the means of production by those who man them. Therefore, according to Motchane, the USSR is a form of state capitalism, in which the work force is still for sale, the workers still salaried, and therefore the extraction of surplus value still prevails.

Motchane would end the exploitation of man by man and thereby achieve socialism by doing away with the distinction between those who own the means of production and therefore command, and those who own only their labor and therefore only obey. To Motchane, collective ownership and economic democracy seem no less commonsensical than the Lockean precepts of political democracy seem to a democrat. A nation's economic life, like its political life, should ultimately be controlled by the very people *who collectively bring that life into existence*. To unlock the door to a more authentic socialism Motchane offers three concepts which have become the basis for the PS program, the Common Governmental Program, the PSU and CFDT programs—collective ownership of the means of production, democratic planning, and *autogestion*. Motchane is most innovative and instructive on the last of these concepts.

According to Motchane, *autogestion* in a general sense is absolute democracy—the permanent, real, and constitutional abolition of all separation between the governed and the governors.<sup>9</sup> Politically, it is the old dream of direct democracy; economically, it is the direct responsibility of workers or producers for all

activity within their place of work.<sup>10</sup> If *autogestion* makes practical sense from the point of view of reducing cumbersome centralized bureaucracies, it also makes "socialist" sense from the standpoint of tackling the core problem of worker alienation by restoring to the worker the full fruits of his labor and by allowing his ability and creativity to flourish through enhanced responsibility.

Motchane proposes to proceed with *autogestion* within existing nationalized industries simply by permitting workers to begin to exercise overall control, graduating eventually to actual management of the workplace. In private industry, however, Motchane is on new ground. First, the National Assembly would have to pass a new "Work Code" to grant workers (via their elected work-place representative bodies and trade unions) a say in the basic physical work conditions and personnel policies, and to grant them access to all confidential information about the business. Ownership and management would not change, but the workers obviously would exert some control through an implicit veto power. The next step would be to grant the workers veto power over all business decisions concerning investments, profits, salaries, contracts, etc. For the final step, Motchane envisages a suppleness of juridical forms to achieve collective ownership. However, he stops short of saying whether the relationships of production can be reversed by substituting *de facto* social ownership (made possible by substantial workers' control) for *de jure* private ownership (which has lost all benefits of command and therefore power). Perhaps he does because he sees the concept of workers' control as something more than an end in itself, as the necessary *dynamique* for an ascendant movement which, because of unyielding contradictions between workers' control and private ownership, will produce "a permanent tension that can only lead to the progressive disappearance of private ownership of the means of production."<sup>11</sup> He is vague because he emphasizes the *movement* toward *autogestion*—and considers that movement, above all, a means to mobilize the masses, to

<sup>10</sup> It must be remembered that Motchane is not advocating the anarchosindicalist dream of producer ownership, but collective ownership. The difference is that in the latter, self-managing workers are expected to operate within general national priorities as set out by the State in the "democratically formulated Plan"; in the former, self-managing workers would operate in a free market economy situation. Thus Motchane's is an attempt to achieve a coordinated decentralization and is very reminiscent of the thrust of the later Guild Socialist writings of G. D. H. Cole.

<sup>11</sup> Motchane, p. 261.

<sup>9</sup> Motchane, p. 242.

stimulate their imagination and expectations, to recreate an active citizen/militant mentality.

A difficult but fertile book such as Motchane's is important because it raises questions concerning the continuing debate on *autogestion* and the role of CERES theorizing within the PS. *Autogestion* as a long-range goal has recently been accepted by much of the French left. Indeed, it provides common philosophical ground for the moderate socialists, the majority of the PS, the non-Communist trade unions, the PSU, and the extreme left. It does this because these groups fundamentally oppose the bureaucratic centralist conception of a socialist economy which is thought to be the PCF's real aim. Nonetheless, because of the popularity of the issue, even the PCF has been compelled to talk about *autogestion* as a socialist goal. The Communists, however, call their notion "democratic management" and they stress the need to avoid a situation in which workers' spontaneity (read "anarchy") prevails over the more desirable direction afforded by workers' representatives (i.e., trade unions and Parliament, or the State). The debate on *autogestion* has become crucial because it lies at the heart of socialist historical development—a development that until recently lost sight of the vision of a decentralized "Proudhonian" socialist society in the face of the prevalent state-oriented socialisms of the European social democrats and the Soviet Communists.

Within the PS itself the battle for primacy in the theoretical sphere remains fierce. Motchane's CERES proved particularly influential in the party's first several years—as participants in the *coup d'Epinaï*, as members on the ruling organs within the initial Mitterrand majority, as influential authors of the party's March 1972 governmental program, and as vocal and militant members of many of the more active departmental organizations. While the CERES members were removed from the Mitterrand ruling majority at the January 1975 party congress, their base of support within the party—as measured by votes on resolutions at the congress—rose from 21 per cent in 1973 to 25.5 per cent, indicating a growing grass-roots support of the CERES position. But the CERES has not gone unchallenged as the predominant force to the left of Mitterrand's ruling alliance. At an October 1974 *Assises du socialisme*, the PS, the PSU, the CFDT and other "third component" groups attending as individual members, explored the possible "regrouping" of all democratic socialists into the main PS body. By January 1975, the PS experienced an influx

of "third component" members and of a minority of the PSU (led by Michel Rocard); the newcomer group thus presented the CERES with a formidable rival as spokesmen for *autogestion*. Though still afflicted with personal and factional disputes, the PS as a whole now stands strategically and philosophically well to the left of its SFIO ancestor, as well as the European social democratic mass parties. In this sense, Motchane enunciates many of the theoretical preoccupations of the new PS.

### The New Communists

Unity of the left required substantial changes within the non-Communist left, but could not have been achieved without equally important changes within the PCF. André Laurens and Thierry Pfister (both political correspondents for *Le Monde*), in *Les nouveaux communistes*, provide a concise and straightforward account of the PCF's evolution over the past ten years. The authors' general conclusion is that the Communists' pursuit of leftist unity has paid off, but only after a heavy investment and possibly a very high price. The price was, basically, a chipping away of Lenin's famous 21 conditions that distinguished Communist parties from European democratic socialist mass parties. Annie Kriegel summarizes these conditions as the three "flying buttresses" of all Communist parties—proletarian internationalism (or loyalty to the USSR), democratic centralism (as opposed to bourgeois political democracy), and the dictatorship of the proletariat (or the concept of class).<sup>12</sup> Laurens and Pfister show that all three principles have been eroded and their analysis is especially valuable regarding the last point; that is, the current debate concerning the French working class.

The PCF's extrication from submissiveness toward the Soviet Union is perhaps the most familiar material in the book. Landmarks of this process include the renunciation of the CPSU as the "guiding party" after Khrushchev's ouster in 1964 (and an angry PCF delegation's journey to Moscow to demand an explanation); the *Bureau politique's* "surprise and reprobation" at the Soviets' invasion of Prague in 1968 (its first public condemnation of the USSR); the party's condemnation of the Prague political trials in 1972; and its insistence on nonintervention in internal affairs of other Communist parties, on the right to criticize other Communist parties, and on the adaptation of socialism to national conditions

<sup>12</sup> Annie Kriegel, *Les communistes français*, 2nd edition (Paris: Seuil, 1970), p. 256.

and historical traditions.<sup>13</sup> These signs of independence were facilitated greatly both by external factors (such as the acceptance of "peaceful coexistence," the waning of the Cold War, and Gaullist *rapprochement* with the USSR) and by internal factors that led the Communists closer to the possibility of actually achieving a share of governmental power (such as bipolarization trends and the democratic socialists' leftward movement).

Further revisionism, say Laurens and Pfister, is evinced by the party's many concessions to "bourgeois" political democracy. It is committed (in the Common Governmental Program of 1972, if not prior in the October 1971 governmental program or the 1968 *Manifeste de Champigny*) to permitting multiple parties in a socialist France (including non-socialist opposition parties), the sovereignty of universal suffrage (provided the verdict is the result of a fair and just electoral system, i.e., proportional representation) and hence the alternation of power, and the protection of basic individual and collective liberties. But these commitments apply to national politics. The party's internal politics remain based on strict democratic centralism. While the secret ballot was introduced in the early 1960s for party elections, authentic democracy continues to be averted by the prohibition of nonhierarchical contacts (which rules out the enunciation and formation of factions), the necessity for all candidates for intermediate and superior party positions to be approved from above, the granting of unlimited powers to the Secretary-General (under article 40 of the party statutes), the heavy reliance on permanent staff members in the party bureaucracy, and, finally, the absence of formalized competing motions and debate in (closed) party congresses.

On the third principle—the concept of class—Laurens and Pfister include an excellent statement of the perpetual debate among French Marxists concerning their true constituency. In a paraphrase of Roger Garaudy's arguments, the authors discuss the problem posed from a Marxist point of view by the emergence of a new technological stratum in the working class composed of scientists, technicians, researchers, engineers, draughtsmen, programmers, etc. Americans may consider these workers as middle class; French Marxists think otherwise. After all, Marx defined the middle class as those small agricultural holders, small businessmen, and individual craftsmen and artisans who were distinguishable from

the working class because they *owned* their means of production and hence were not exploited through surplus value. But, says Garaudy, the traditional middle-class categories have diminished with technical and scientific progress, and have been overshadowed by the new technological class which, while perhaps enjoying a comparable living standard, nevertheless remains salaried, or subject to the laws of profit and surplus value. Therefore, concludes Garaudy, its solidarity with the working class is firm. He thus embraces the theoretical innovation of the Italian Communist Gramsci, which holds that a *new historical bloc* of all wage earners will replace the more traditional working class as the rallying point in the achievement of socialism.<sup>14</sup>

The Communists resolutely rejected Garaudy's intellectual contribution for tactical as well as ideological reasons. First, while the new historical bloc encompassed the 75 per cent of French working people who were salaried, and while half of those were manual workers, one-fourth *intellectuels salariés* (the technological stratum), and one-fourth *employés salariés du secteur tertiaire* (white collar workers in tertiary services), the Communists knew that the last two sectors were expanding the most rapidly and that it was in these sectors that the PS was likely to make the greatest inroads in terms of members and votes. Secondly, as Laurens and Pfister discuss, the Communists accused Garaudy of ideological heresy. By down-playing the primary role of the working class in the struggle for socialism, Garaudy had "renounced dialectical and historical materialism as a universal method and conception of the world."<sup>15</sup> By this, the Communists meant that the heart of production lies in the primary sector and material goods, not services. In these primary industries, the work is not only the most vital to the economic life of society, but also the largest in scale or most collective. Here, then, the extraction of surplus value is the most direct, the fundamental contradictions of the relationships of production the clearest, and the political consciousness of this exploitation potentially ripest. The role of the technical and scientific revolution should not be overestimated, say the Communists, the pre-eminence of manual workers in the production process never underestimated. Indeed, the "new stratum" will never replace the traditional working class in revolutionary fervor because the latter is opposed to the bourgeoisie in every

<sup>13</sup> These last three positions are developed most recently in Marchais's *Le Défi démocratique*.

<sup>14</sup> For a more thorough discussion, see Roger Garaudy, *L'Alternative* (Paris: Laffont, 1972), pp. 173–198.

<sup>15</sup> Laurens and Pfister, pp. 190–191.

respect—relationships to production, lifestyles, class, mores. This is because while the new salaried white collar working class may be "*exclus de la décision*," the working class is also "*exclus de la consommation*."

Finally, the PCF affirms, the working class is not only the poorest but the most numerous. And as the party of this working class, the PCF refuses to relinquish its claim to be the vanguard of the socialist revolution. The very doctrine of scientific socialism decrees that the revolution will fail without such a party. This concerns a central question in the Laurens/Pfister book. How much can the PCF evolve and change before becoming a "banal social democratic and reformist party"?<sup>16</sup> Roland Leroy, a prominent PCF leader, said ten years ago, "Ask anything you want of us, but do not ask us to cease being communists." This may have been puzzling at the time. After ten years of evolution, it evidently means simply a resolute *patriotisme de parti*—a willingness to compromise, to be flexible on ideology, tactics, anything, except on the party's very existence and on the internal device that is above all deemed necessary to maintain that autonomy—democratic centralism.

### The Common Governmental Program

The June 1971 Congress decisively committed the PS to the formulation of a common governmental program with the Communists. The program was envisaged as a binding legislative contract which would specify measures to be taken by a Communist-Socialist majority in the course of a five-year legislative term. By the time negotiations began on the program in the spring of 1972, the respective parties had published their own party programs, from which the common program would undoubtedly emerge as a compromise. The PCF's *Changer de Cap: Programme pour un gouvernement démocratique d'union populaire* of October 1971—that party's first governmental program—adhered closely to its "new line" concerning a peaceful and parliamentary "French road to socialism."<sup>17</sup> New, not because it differed from the Thorez line after World War II, but because it said that a transitional period called a *démocratie avancée* had to be established between capitalism and socialism via a Popular Front government. This period would be characterized by a more complete extension of democratic rights

in the political, economic, and social domains to the great majority of French people. It would be transitional because its object would be to eliminate the monopolistic nature of French capitalism and the personal power abuses of the Gaullist Fifth Republic. Only then could the alliance move on to a complete rupture with the capitalist system in general.

In many respects, *Changer de Cap* previewed the PS's program of March 1972 (*Changer la Vie: Programme de gouvernement du parti socialiste*), although the latter is long-range in intent, and hence a good deal more comprehensive and imaginative. The leftward swing within the PS, led by the CERES, former PSU members, and "May 1968" newcomers, found considerable expression in the program.

The negotiations on the Common Program, from late April to late June 1972, rarely were easy and agreement was by no means assured. Finally, at the end of an all-night bargaining session, the two parties produced on June 27, 1972, a *programme commun de gouvernement*; one of the most important political events for the left since 1920. The program contains just under a hundred pages<sup>18</sup> of detailed legislative proposals, whose contents can be summed up as a *democratization* of French life. Not surprisingly, the first section to be agreed upon concerned social matters. These proposals simply deepened the parties' commitment to a notion that most modern industrial nations have long since accepted—that one function of government is to provide for and coordinate the satisfaction of certain basic needs of the people, be they jobs, social security, health care, housing, transport, education, culture or leisure. The program did move ahead in one area in particular—namely, the emancipation of women and their place in society.

The second section—on economic matters—spelled out the triple axis of *autogestion* or democratic management, collective ownership, and democratic planning. With regard to the first, although all the section's specifics reinforced the PS's intent, the Communists refused to accept the term *autogestion* as the final goal of economic democracy. Particularly at the time the concept had vague anti-Communist connotations—partly because it meant rejection of the centralized *étatisation* associated with the USSR, and partly because the PS was purposely seeking to assert its independence in matters of socialist theory. So despite the ac-

<sup>16</sup> Laurens and Pfister, p. 233.

<sup>17</sup> This new line was first enunciated in their 1968 *Manifeste de Champigny*, and developed in Waldeck Rochet's *L'Avenir du Parti communiste français* (Paris: Grasset, 1969).

<sup>18</sup> The Socialist edition runs 96 pages, the Communist edition 192 pages. The only difference is in the prefaces. The Socialist version gives some brief but valuable information concerning the negotiations themselves.



tual consensus, the Common Program at this point was forced to resort to its only listing of divergences: "... the intervention of the workers in the management and direction of the enterprise will take new forms—which the *Parti socialiste* sees in the perspective of *auto-gestion* and the *Parti communiste français* in the continuous development of democratic management. . . ."<sup>19</sup>

Concerning collective ownership of the means of production, the program stressed that this could take several forms—nationalizations, societies of mixed economy, cooperatives, mutualities, local public services. Nevertheless, a minimum threshold of nationalizations would have to be crossed to break the grip of monopolistic *féodalités* on the national economy. Using the same general criteria as the British Labour Party in 1945, the program would nationalize the following "commanding heights of the economy"—the entire banking and financial sector, the sectors of underground resources, arms, space and aeronautics industries, nuclear and pharmaceutical industries, and a large portion of electrical and chemical industries. Altogether this meant that nine (specified) companies would be nationalized outright and four (also specified) nationalized via state financial participation. The rest of the economic section presented no problems and stipulated the need for a democratically decided plan of broad national priorities, an agricultural and small and medium firms (PME, *Petites et Moyennes Entreprises*) policy favoring cooperative structures, a fiscal and monetary policy concerned with socially beneficial investment and redistribution of incomes through more progressive taxation.

The third section of the Common Program—on institutions and liberties—represented a vindication of the Mitterrand strategy over that of the former party leader Savary. Savary and his allies had insisted that a common governmental program could be drawn up only after a preliminary ideological discussion had elicited certain democratic guarantees from the PCF. The Mitterrand/CERES motion at the Epinay congress in 1971, however, stated that these guarantees would be written into the eventual common governmental program, and would therefore be binding on the PCF once it signed the legislative contract. Thus in the Common Program we find a commitment to respect the verdict of universal suffrage (alternation of power), to respect and extend personal liberties (freedom of speech, association, etc.), to ensure the plurality of parties and their inde-

pendence from the State, as well as a commitment to the establishment of a Supreme Court for purposes of judicial review, *habeas corpus*, an ombudsman for liberties and to strengthen the legislative powers by revising the dozen Constitutional articles most flagrant in their invitation to presidential abuse.

Agreement was most difficult to reach on foreign policy. In the end, however, divergences were resolved—by Communist concessions. On at least five major issues, the Communists accepted the Socialist position: (1) the agreed-upon program calls for the renunciation of the strategic—but not tactical, as the Communists wanted—nuclear striking force; (2) it does not call for the destruction of existing stockpiles of nuclear weapons, as the Communists wanted; (3) it does not call for immediate and unilateral withdrawal from NATO, as the Communists wanted; (4) it ensures the possible participation of the French armed forces in a system of European collective security, as the Socialists insisted; and (5) it ensures continued and active French participation in the EEC, as the Socialists insisted. The only Socialist concession<sup>20</sup> was the de-emphasis—in this five-year contract—on their long-range goal of political unity of Europe.

Initially, the Common Program appeared to benefit both parties.<sup>21</sup> The Communists' long-sought political integration was real and their fears of a Socialist return to a third force option (i.e. center-left coalition) were greatly assuaged. Clearly, the Socialists had predominated at the negotiating table. They had secured essential democratic guarantees and had brought the PCF further from its USSR alignment through the program's European security commitment. For both parties, the program represented the first clearly enunciated *programme de rupture* with capitalism and a tangible commitment to a democratic and peaceful transition to socialism.

As such, the Common Program dominated the campaign preceding the March 1973 legislative elections. Criticism of the document cen-

<sup>20</sup> It might be thought that the Socialist conceded on the extension of nationalizations. While the PCF had approached the negotiations with a list of 26 companies it wanted nationalized, the Socialists came with 6 such names. The 13 companies decided upon in the Common Program, however, were (with one exception) either directly or indirectly included in the section on nationalizations in the PS's own program of 1972.

<sup>21</sup> The *Radicaux de gauche*, a small splinter group from Jean-Jacques Servan-Schreiber's Radical Party, joined the Popular Front alliance in July, when they ratified the Common Program and contributed a short annex thereto.

<sup>19</sup> Socialist edition, p. 48; Communist edition, p. 111.

tered on two specific charges. An initial one concerned the old Red menace theme of "Communists in government." But this tactic may not have proved so effective as expected owing to several facts at which Mitterrand hammered away: de Gaulle himself originally brought Communists into his postwar government; since the days of the FGDS, Mitterrand had consistently excluded the possibility of the Communists' holding any of the key ministries (Prime Minister, Foreign Affairs, Defense, Interior); by December 1972, 59 per cent of the population, according to the polls, would find acceptable the fact of Communist participation in government;<sup>22</sup> finally, Mitterrand was generally regarded as the one man on the left who could keep the Communists in their place as minority partner.

So the right turned instead to a more cogent criticism of the Common Program—how did the left propose to finance such an ambitious program? Dutifully, Socialist and Communist economists in January 1973 presented a five-year budget to show where funds would come from (to pay for social security increases, public services and facilities, and nationalization costs), but much of the financing depended on a continuing healthy (8 per cent) growth rate, which of course was problematic. More broadly though, many on the left felt that the *jeu de chiffres* (numbers game) so dear to Giscard d'Estaing was an incomplete yardstick. The real question for them was, did the Common Program answer as honestly and as completely as possible the needs and priorities of the collectivity?

Despite the existence of the Common Program and unprecedented leftist unity, the left still fell short of unseating the Gaullist majority in the March 1973 election, although the majority's margin of seats was reduced considerably.<sup>23</sup> One important consequence arose, however, from within the still-unquestioned alliance. For the first time since the war, the democratic socialists had nearly matched the PCF's total vote on the first round—with 20.36 per cent for the UGSD (*Union de la gauche socialiste et démocrate*, a first-round electoral coalition comprising the PS and *radicaux de gauche*) and 21.25 per cent for the PCF. By contrast, the figures for 1967 were 18.96 per cent for the FGDS and 22.51 per cent for the PCF; for 1968, 16.53 per cent for the FGDS,

20.02 per cent for the PCF. In the Fourth Republic, the PCF had averaged around 25 per cent, the SFIO 15 per cent. Whether the Socialists' performance was due to the Common Program, Mitterrand's leadership image or the credibility of a renovated PS, the fact remained that, as the CERES was quick to note, "it was by adopting the most leftist program in its history that the PS obtained its highest number of votes."<sup>24</sup>

### François Mitterrand

Another major factor in the realignment was François Mitterrand himself. Along with de Gaulle, he is the man most responsible for uniting the left into a viable opposition force. Mitterrand's accomplishment was no sudden accident. His general strategy had been decided on a decade and a half ago, his success had been long and difficult in the making. To know him, to understand his philosophy and his strategy, it is best to consider together Mitterrand's *Ma part de vérité* (which is especially good for his wide-ranging personal opinions and detailed analysis of the 1965–69 period), Borzeix's *Mitterrand lui-même* (which gives a good deal of insight into Mitterrand as a person and a politician), and Mitterrand's *La Rose au poing* (an expressive work of reflection and explanation of the Common Program and socialism).

The biographical picture which emerges from Borzeix's book is that of a man of bourgeois Catholic background; a student of law, political science, and literature; a prisoner-of-war escapee who returned to Resistance work and then to a long (and sometimes controversial) career in the ministries of the Fourth Republic. May 1958 abruptly changed that comfortable course. Mitterrand's reaction to de Gaulle's take-over accounts to a great extent for his remarkable "second career" in Fifth Republic politics. Unlike his Radical-Socialist and Socialist colleagues, Mitterrand read the Gaullist phenomenon as a long-term challenge that necessitated basic changes in the left opposition. As early as 1959, Mitterrand formulated his strategy of democratic socialist regroupment and renovation, augmented by a new Popular Front alliance with the French Communist party.<sup>25</sup> In pursuit of the first aim, Mitterrand flirted in 1960 with the PSU, became part of the growing club movement instead, in 1963–64 established contacts with anti-Molletist SFIO members in the *Colloques*

<sup>22</sup> See the SOFRES poll in *Le Monde*, December 12, 1972.

<sup>23</sup> The majority seats fell from 360 to 262; the left's rose from 93 to 177. This, despite a first round total of 45.3 per cent for the united left, as compared to 38.48 per cent for the majority.

<sup>24</sup> Editorial in *Frontière*, April 1973, np.

<sup>25</sup> See his article in *La Nef*, April 1959; entitled "Front Populaire, coalition immorale?"; cited in Borzeix, p. 189.

*socialistes*, saw the tide turn in 1965 as he unexpectedly forced de Gaulle into a presidential runoff, and transferred his newly found national recognition and prestige into an enhanced leadership hold on the recently-created *Fédération de la gauche démocrate et socialiste* (FGDS, which confederated the SFIO, CIR and Radical Party).

By 1966 the time had come for Mitterrand to attempt his second aim, an alliance with the Communists (who, after all, had unconditionally supported Mitterrand's presidential candidacy). The electoral accord struck for the 1967 legislative election enabled both FGDS and PCF candidates to gain large numbers of seats; then came the February 1968 *Accord*—a document which went the furthest ever in detailing the convergences and divergences between the two major leftist groups in France. Surprisingly, Mitterrand places this successful February 1968 *Accord* (rather than the more commonly accepted date of May 1968) as the “beginning of the end” of the FGDS. This was because he felt the *Accord* represented the limit to which Guy Mollet and the SFIO would go, structurally and ideologically. Mitterrand had pushed for fusion of the three FGDS member groups from the beginning. Mollet, in Mitterrand's opinion, was only using the formation as a “ford” at which to cross the difficult waters created by Mitterrand's leadership rivalry. Ideologically, Mitterrand felt the February 1968 *Accord* had pushed Mollet to the limit because of his social democratic stance—that is, his insistence on a program basically unchanged since the early years of the Fourth Republic and characterized, for example, by its very limited call for nationalizations, by a narrow trade union mentality with regard to economic demands and an absence of interest in economic democracy, and by a resolutely Atlanticist foreign policy.

These criticisms may sound strange from a man who never had been in the mainstream of democratic socialist ideology. Yet, these three books show the extent to which Mitterrand has moved leftward from his basic liberalism of the 1950s to a quite radical socialism in the 1970s. His conversion began in the 1950s, impelled by a falling away from the Church and ministerial agony over problems of decolonization, and it was fully realized in the 1960s by his growing belief that the root-evil of de Gaulle's republic lay not just in personal power, but in the very nature of the capitalist system itself. Mitterrand's reasons for rejecting liberalism parallel very closely those of Pierre Mendès-France in 1959, as well as many other European liberals-turned-socialists reaching

back to Jean Jaurès and Keir Hardie. They were convinced that liberal measures (such as anti-trust legislation) have never been and cannot ever be effective in battling the monopolistic nature of capitalism because they fail to strike at the core of the problem—“economic conditions, the forms of production and of property.”<sup>26</sup> If economic life is to operate in the service of society and not for the profit of individuals, Mitterrand believes, collective ownership is essential. But, basically rejecting Marxism, Mitterrand observes that “many socialists believe in a kind of mechanical law according to which a mere change in economic structures will raise them from one system to another. This error leads them to confuse the end and the means.”<sup>27</sup> The end, of course, places Mitterrand on familiar *autogestion* ground: collective ownership must prepare social structures which will put an end to “the worst wastage engendered by the capitalist system: that of men's creative facilities, their sterilization, the alienation of workers.”<sup>28</sup> Thus, Mitterrand rejects capitalism, not as a scientifically-condemned historical phase, but because it betrays certain moral principles—liberty, dignity, justice, solidarity. “The role of socialism is to liberate man from the constraints and the anguish that, in a capitalist society, have him by the throat. There can be no happiness for those who, all their lives, seek only to subsist.”<sup>29</sup> The error of those on the right, continues Mitterrand, is that they perceive liberty as something of a “paradise lost.” But liberty, for a socialist, does not exist in a natural state and he must instead “go out in search of ways to secure his liberty.”<sup>30</sup> Therefore, says Mitterrand, “to organize society in order to liberate the individual, I know of no other mission for socialism.”<sup>31</sup>

Mitterrand's critics in turn reply that if he conceives of socialism in such a way, it is indeed strange for him to make an alliance with a party whose comrades in the Soviet Union and Eastern Europe have not only failed to change the social structure, but have sacrificed individual liberties to a higher *raison d'état collectif*. Characteristically, Mitterrand meets head-on the question of the Communists' trustworthiness at the beginning of *La Rose au poing*. He recounts a friend's reminder to

<sup>26</sup> The quote is by Jean Jaurès, and is cited in *Ma part de vérité*, p. 172.

<sup>27</sup> *La Rose au poing*, p. 223.

<sup>28</sup> The quote is by Pierre Joxe, *Dire*, March 1969, p. 27, and is cited in *Ma part de vérité*, p. 184.

<sup>29</sup> Interview in *Le Nouvel observateur*, September 3, 1973, p. 47.

<sup>30</sup> *La Rose au poing*, p. 143.

<sup>31</sup> *Ibid.*, p. 146.

him, early in 1972, of "the long-humiliated about-to-be-executed cortège of democrats, liberals, and socialists of Eastern Europe who had thought it possible to collaborate with the Communists, who had advanced several steps in that short path dear to Lenin, and who, at its end, had met with prison, the gallows, or a bullet in the neck."<sup>32</sup> In pursuing an alliance with the PCF and in signing the Common Program, writes Mitterrand, "all the many questions that arose in my mind boiled down to just one: Had I served liberty?"<sup>33</sup> Because he believed the answer was "yes," Mitterrand wrote this book to explain why his friend's fears of the Communists' intentions were exaggerated, to justify the Common Program as the restorer of French liberties, and to plead eloquently the socialist case.

Mitterrand's first contacts with the Communists date back to the war—"in the *stalags*," then in the Resistance, where "they taught me not to close my eyes if I didn't want to be crushed by their formidable machine. A difficult equilibrium to preserve between vigilance which permits nothing and confidence which permits all. I am still there."<sup>34</sup> After the war, Mitterrand sat in Cabinets containing Communist ministers. Once the Cold War began, stresses Mitterrand, the various Communist parties quietly left the ten Western European governments in which they participated. Then came the Fourth and early Fifth Republic years during which the PCF remained isolated. Mitterrand considered it a scandal at best, dangerous at worst, that one-fourth of the French voters remained outside national political life so long. By the 1970s, he was in a position to act on his conclusion that "millions of French people who work, produce, serve their country and love it"<sup>35</sup> should no longer be excluded from national public life because they vote Communist. Only a Popular Front alliance could integrate the Communist party and voters completely into the political system, and, alternatively, offer to the PCF voters the option of a viable democratic socialist party.<sup>36</sup> Each step toward integration, observes Mitterrand, has furthered the PCF's progress of destalinization. In defending the principles of political democracy as found in the Common Program,

for example, the Communist party will become so impregnated with those very principles that to abandon them would risk the loss of a good many voters. Thus, "in reality the PCF is itself a prisoner of unity, it cannot go back now, and if it does, it will only see that outside the enchanted circle of unity, it is reduced to a marginal force."<sup>37</sup> Finally, Mitterrand simply feels that a Socialist-Communist alliance in France is a natural one. He regards the Common Program, then, as a "meeting place where those who have been torn apart by a half century of history have once again found each other."<sup>38</sup> The program's historical dimension in Mitterrand's mind is confirmed by his recollection of Léon Blum's words to the 1920 Tours congress: "All of us, although separated, remain socialists; in spite of all, we remain brothers, brothers who have been separated by a cruel quarrel, but a family quarrel, and whom a home may someday reunite."<sup>39</sup>

No, our alliance is not strange, continues Mitterrand, especially when our critics know the Socialists have never shrunk from outspoken criticism of the USSR (particularly during the long 1973 legislative election campaign) and when even the PCF has publicly disassociated itself from some of the Soviets' more blatantly repressive measures. What we *do* find strange is the record of the right with regard to the status of liberties in its *recognized* and *accepted* allies' internal politics. In *La rose au poing*, Mitterrand cites pages of painful Amnesty International documentation concerning actions taken by a dozen of France's allies in denying basic democratic liberties to their domestic opposition.<sup>40</sup> Torture, executions, massacres—not only did these acts evoke no condemnation nor even an outcry on the part of the French government, observes Mitterrand, but indeed they usually coincided with further sales of arms, a voting of further credit, further technical aid. Can the French right really be so concerned about Prague and liberty if it cares only for those on the right side, never for the opposition? But let us stick to France's own domestic liberties, says Mitterrand. Once again, the question is—liberty for whom? Mitterrand (backed by a thorough research team) examines closely the situation in France and the Common Program's provisions in the realm of institutional, political and personal liberties, and, most lucidly, economic lib-

<sup>32</sup> *La Rose au poing*, p. 7.

<sup>33</sup> *Ibid.*, p. 11.

<sup>34</sup> *Ma part de vérité*, p. 22.

<sup>35</sup> *La Rose au poing*, p. 21.

<sup>36</sup> Mitterrand frankly admitted in a speech to his colleagues in the Socialist International meeting in Vienna, June 28, 1972, "Our fundamental objective is to remake a great socialist party from the terrain occupied by the PCF itself in order to demonstrate that out of five million Communist voters, three million can vote socialist." Cited in *l'Express*, 17-23 July, 1972, p. 18.

<sup>37</sup> *Ma part de vérité*, p. 141.

<sup>38</sup> *La Rose au poing*, p. 34.

<sup>39</sup> *Ibid.*, p. 43.

<sup>40</sup> For instance, Argentina, Brazil, Uruguay, Iran, Turkey, Spain, Portugal, Vietnam, Indonesia, Libya, Uganda, Central African Republic, Chad, Greece, not to mention Haiti, Angola, Yemen, and Paraguay. *La Rose au poing*, pp. 47-63.

erties and rights. For, contrary to his rightist critics, Mitterrand perceives the Common Program as neither Socialist nor Communist—but as a “new code of liberty and property.”

“It proposes to the French people a five-year experiment during which a leftist majority will put an end to the lawless regime which governs us, will institute a code guaranteeing public liberties, will eliminate monopolies in the key sectors of the economy, will prepare for workers’ control in industry, will decentralize administrative and political decision making, will answer to basic economic demands, will practice a foreign policy of collective security, and will attempt to outline the contours of a model of civilization in which the individual will finally find the means to exercise his responsibility.”<sup>41</sup>

But, the right persists in asking, what if the Communists are not truly sincere in accepting a document that is neither Socialist nor Communist? “The Socialists have no way of sounding out the hearts and souls of the Communists.”<sup>42</sup> The only thing we can do is “to create conditions such that they have to act as though they were sincere.”<sup>43</sup> Over the past ten years, Mitterrand has gone far in creating those very conditions.

#### The Communists’ “Democratic Challenge”

Communist Secretary-General George Marchais, in *Le Défi démocratique*, parallels Mitterrand by justifying the Common Program from his party’s point of view, explaining it more fully, and answering straightforwardly “the question thought to be the problem of problems for the Communists: democracy and liberty.”<sup>44</sup> And therein lie the book’s virtues and its sins. For despite an open and earnest analysis that scarcely differs from the Socialists’, several jarring Stalinist concepts remain in Marchais’s book to remind us of the great gap separating those two strategically aligned parties.

Marchais begins by developing and illustrating the universal leftist critique of capitalism—“In France today there are those who produce social riches and those who benefit from them. They are not the same.”<sup>45</sup>—and discusses how the Common Program would set out to change that relationship. In fact, Marchais continues to speak for the left in general as he moves into the section on liberty. He deals with the standard bourgeois statement about freedom in the West—“One protests and rightly so against

repression, but at least one *can* protest.” Fine, says Marchais, but one can protest by what means? The vote? A sham, he maintains, when in 1958 the UDR with *fewer* votes than the PCF wound up with *twenty times* the number of seats; when in 1968, the right with only 3 per cent more votes than the left ended up with four times the number of seats. The French people may vote Communist, but “between the moment they cast their ballot and the moment the Assembly convenes, some strange hocus-pocus defrauds them of their vote” (p. 87). Despite constitutional guarantees for the right to strike in protest, continues Marchais, a multitude of impediments have been thrown up by the Gaullist regime. Nor can public protest meetings be held when a Gaullist-dominated City Council will not rent the appropriate public hall. The right to demonstrate risks CRS police brutality. Access to TV is controlled by a government which displays blatant favoritism. “In sum, everybody has a certain number of rights and liberties. But some have the means to exercise them, while others are deprived of those means” (p. 91). At any rate, Marchais continues, the Communists have another conception of liberty. That is the possibility not only to protest but for each person to realize himself to the fullest. “And there is no liberty unless all people have that liberty” (p. 92). There follows a clear and precise two-page restatement of what can be considered the heart of socialism—whether it be utopian, scientific or syndicalist. “Man becomes man by his work. It is by work that the human race can know, master, utilize nature. Thus if work is enchained, if its fruits are confiscated by others, man becomes a stranger to his work, to his own nature and cannot fully know his liberty” (pp. 93–4). That is why Marchais writes that “freedom is the freedom to work and to choose one’s work, it is the freedom to learn and to cultivate oneself, the freedom to find housing, to enjoy leisure time and a happy old-age, the freedom to bring up with decency the number of children one chooses, the freedom of opinion, expression, association, the freedom to participate in the management of one’s workplace and in the affairs of one’s village, town, country . . .” (p. 96). Such freedom can only be assured by socialism, and therefore, in the eyes of the Communists, socialism and freedom are inseparable, socialism “being a superior degree of freedom” (p. 97).

Only as we enter the final section of Marchais’ book are we reminded of the chasm separating the PCF’s and PS’ fundamental beliefs. For it is in discussing the necessity of the French Communists to pursue their own

<sup>41</sup> *Ibid.*, pp. 33–34.

<sup>42</sup> *Ibid.*, p. 128.

<sup>43</sup> *Ma part de vérité*, p. 71.

<sup>44</sup> *Le Défi démocratique*, p. 9.

<sup>45</sup> *Ibid.*, p. 11. This is a simple but basic theme, also stressed by Mitterrand in his 1974 presidential campaign.

uniquely French road to socialism that Marchais reveals a view of the Soviet model that comes nowhere near to the Socialist view. Do the Socialists say, for example, that the Soviet worker is freer than his French comrade basically because of his greater social and economic security? That the Soviet Union is exemplary in terms of its citizens' democratic participation in all aspects of society because at any one time more than 25 million citizens are permanently associated in work committees of their local soviets; that the right to criticize is alive and well owing to the "what isn't working" columns that fill the Soviet press; that the trade unions in the Soviet Union have extensive rights; that it is normal to have only one party in the Soviet Union because "class oppositions have totally disappeared" (pp. 163 ff.)? Marchais acknowledges that the development of socialism in the USSR and Eastern Europe has not been without faults (though he doesn't list them). But, he explains, "the dramatic violations of socialist legality committed by Stalin" were due to the siege mentality of a Soviet people resisting subversive attempts via Western imperialism, Hitlerism, and the Cold War. What counts in the long run is the *general tendency* of socialism, and that tendency is "a constant development and perfection of democracy" (p. 165). Above all, Marchais writes, in the USSR and Eastern Europe "socialism has already accomplished a liberating mission without equal in the world of humanity" (p. 167).

In effect, Marchais envisages a French model of socialism which differs from that of the Soviet Union *not* because the latter has betrayed the original Marxist vision, but because "the original character of each nation" will determine its own road to socialism. In this regard, it is perhaps surprising that Marchais writes that to think there would be a general uniformity of socialist countries "is to imagine there exists a mechanical link between the means of production and the level of economic development on the one hand, and institutions and mode of life on the other hand. Well, it just isn't like that. The mode of production does determine certain general traits of society, a given level of economic development carries certain very general consequences—but nothing more" (p. 179). Which means either we are all Marxists or no one is.

Such revisionism should not obscure the "unrevisable" role of the PCF within the Socialist-Communist *démocratie avancée* transition period. Marchais sees three conditions for socialism: the collective appropriation of major means of production and exchange, the exercise of political power by the working class,

and the activity of a party that acts as the avant-garde of that working class, i.e., the Communist party. Nothing in scientific socialism, writes Marchais, says that the construction of a socialist society is dependent on the presence of one socialist party at the head of the government. Scientific socialism only says that there can be no movement to socialism without a party representing the interests and objectives of the working class. Now the PCF is one of several leftist parties—it contains men and women who are no different from their fellow French citizens, who do not form any particular "countersociety" with all its sinister implications.<sup>46</sup> What the PCF *does* have, and which sets it apart, is a "scientific theory—historical materialism"—which allows the party "to unravel events and to orient its actions with sureness toward the triumph of the working class objectives. It allows the Communists to understand better the situation at hand and in its complexity and originality, it allows them to resist modes and passing illusions."<sup>47</sup>

But this scientific theory demands a key corollary. Here, Marchais conveniently takes up the point about the PCF's real and probably permanent uniqueness—democratic centralism. If internal democratic discussion is the condition for arriving at a correct decision, agrees Marchais, once that decision is taken, discipline of action must take over or the party will lose all efficacy. Internal democracy cannot mean organized factions, because factions are the negation of democracy in that they crystallize positions which "set" opinions and hinder the free confrontation of ideas. Thus, factions result in a search for compromise within the party instead of the restatement of a "scientific" policy. Also, they imply the quest for tactical agreement in order to gain control of the leadership positions.<sup>48</sup> Perhaps this defense of democratic centralism is the most revealing of all Marchais' thoughts. For Marchais, historical materialism means there *is* only one possible answer, and that once that is perceived, the resulting course of action cannot be challenged validly. The Socialists may value extensive debate in order to arrive at what they consider the "best" choice—the Commu-

<sup>46</sup> Obviously, Marchais wants to refute the common assertion, made for example by Annie Kriegel, that the Communist party members form a "closed society." By way of illustration, he notes that neither he nor Maurice Thorez *ever* raised a clenched fist as a greeting; nor does he personally follow the habit of some younger party members who automatically use the familiar form of address, *tutu*, toward all PCF members!

<sup>47</sup> *Le Défi démocratique*, p. 193.

<sup>48</sup> *Ibid.*, pp. 201-2.

ists need debate only until the "correct" line becomes evident to all.

All of this helps explain how Marchais and the PCF perceive the Common Program. They regard the unity efforts over the last ten years as having been *instigated* and *led* by the PCF and claim the PCF finally managed to push a reluctant SFIO in a leftward direction, thereby enabling the signing of the Popular Front agreement. Although the Common Program is only a very preliminary step down the road to socialism, the Communists feel that it has enhanced the PCF's position as "watchdog" on the left and guardian of the will to proceed to socialism, and therefore has strengthened the Communists' predominant hold on the French left. Above all, they feel confident of their strategy since, according to their philosophy, the presence of a Communist party is the *sine qua non* of socialism's success.

### Conclusion

Despite varying perceptions by which the PS and PCF each feels it is the dominant partner of the Popular Front alliance and will therefore reap the benefits more fully, what are the facts? In terms of a *program*, the Common Program's completion was contingent on major concessions by the Communists, and the program's thrust and provisions are practically identical to the Socialist program. In terms of *elections*, the Common Program and unity campaigns have unquestionably benefited the Socialists more than the Communists. One of the most important aspects of the 1973 legislative election was the near-equal balance in votes between the PS and PCF. More revealingly, the PS made an incontestable breakthrough in expanding its national voter implantation, while the PCF votes, though stable, tended to become more concentrated into fewer departments.<sup>49</sup> In terms of *presidential politics*, the May 1974 election saw Mitterrand endorsed unanimously by his party, with complete unconditional support (and active campaigning) from his PCF allies. In terms of *governmental power* arising from the possibility at some future date of a Mitterrand victory and a leftist legislative majority, the Communists are remarkably quiescent in their demands. Even after Mitterrand stated that his Prime Minister would be a Socialist but that

the Cabinet would "reflect the presidential majority" (i.e., some Ministers from the PSU ranks, which supported his candidacy but not the Common Program), Marchais said that the six or seven ministries destined for the Communists need not include any key ministries. Finally, and perhaps crucially, since all the previous indicators may be determined by it ultimately, *membership strengths* have undergone dramatic changes. The PS continues to grow—from 75,000 in 1969 to 105,000 after the March 1973 legislative elections, to 150,000 at the January 1975 party congress.<sup>50</sup> In contrast, the PCF membership has remained stable in the 1960s and 70s, officially at around 400,000–425,000, although most non-Communist estimates put it as realistically more like 275,000–300,000.

In conclusion, then, by accomplishing virtually everything that the PCF had demanded of the Socialists over the last ten years—the transformation of the SFIO into an "authentic socialist party," union of the left, and a common governmental program—Mitterrand has come to pose a fundamental threat to the PCF's predominance on the left. Indeed, that fact was reflected in the PCF's actions following the September 1974 by-elections in which the Communists lost votes in four of the six constituencies concerned and in which the UGSD gained in all six. Faced with the dilemma of breaking an alliance they had so painstakingly sought (at the risk of alienating many of their voters and of returning to political isolation) or of maintaining the alliance in which the Socialists seemed likely to be the senior partner, the Communists grudgingly continued to accept the latter. Nonetheless, from their October 1974 congress until the spring of 1975, they carried on a vitriolic campaign against their Socialist ally. Ostensibly, the "vigilant" criticism was to prevent the PS from reverting to a social democratic stance by joining with Giscard in a reformist coalition government; for all practical purposes, this line of attack was to quiet internal rank and file discontent with the party's seeming subservience to the Socialists and with its post-May 1974 outstretched hand policy toward the Gaullists and Catholics.

With no scheduled national elections until 1978, the unity of the French left remains intact for the present and a review of the above publications may have helped to clarify why the Socialist-Communist alliance could endure and why the accompanying democratic socialist renaissance could conceivably transform the politics not only of the left but of France herself.

<sup>49</sup> The number of departments in which the Socialists received less than 10 per cent of the vote fell from 9 in 1967 to 1 in 1973—the number in which the Communists received less than 10 per cent rose from 7 in 1967 to 10 in 1973. The Socialists' totals rose in 14 of the 21 regions, usually by 4 to 5 per cent—the Communists gained in only 3 of the 21 regions, and by an overall 1 per cent. See *Dossier sur les forces politiques et les élections de mars 1973*, published by *Le Monde*, Paris: 1973, p. 45.

<sup>50</sup> *Le Monde*, February 4, 1975.

## BOOK REVIEWS

**Welfare Versus Freedom.** By Evan Bitsaxis. (Athens, Greece: Olympic Editions, Ltd., 1972. Pp. 383. \$10.00.)

*Welfare Versus Freedom*—written in English—is by a Greek lawyer who also earned a Ph.D. degree in economics. In his varied career, Bitsaxis served in a number of Greek and American government posts, including an internship with the U.S. Bureau of the Budget.

The volume under review surveys classical political theory and modern welfare economics in a way that fosters the reader's understanding of both these fields. And it is Bitsaxis's perceptive review of the scholars in these areas which constitutes the principal value of the book. The primary focus is on Plato, Aristotle, Pigou, Keynes, and Galbraith. Of course, the Greek classics are staples of the political science curriculum, but major works of modern political economists are often neglected. Bitsaxis provides edifying summaries of such works as Pigou's *The Economics of Welfare*; Keynes's monumental *General Theory of Employment, Interest, and Money*; and many of Galbraith's principal contributions. Shorter—though still penetrating—analyses are supplied for works by such luminaries as Abram Bergson; I.M.D. Little; Pareto; Simon Kuznets; and Anthony Downs. An excellent detailed bibliography on social welfare issues closes Bitsaxis's helpful volume.

Bitsaxis calls for the development of a virtuous society as the critical objective of government. Virtue is defined as incorporating such traditionally accepted values as bravery, straightforwardness, self-sacrifice, and liberty. Classical political theory stressed these same values, but in Bitsaxis's interpretation, the ancients failed to advocate governmental promotion of materialistic well-being through such means as full employment and equitable distribution of income policies. "There is," the author argues, "no 'welfare norm' of help to modern economic theory to be drawn from the stream of Greek Philosophy" (p. 35). Bitsaxis urges that governmental job-creation and income-redistribution norms and policies are essential as conditions precedent to full societal development of the aforementioned virtues.

Bitsaxis insists that governmental adherence to economic science is essential to material well-being. "Proper economic thinking aided by sensible mathematics," the author maintains, "... will certainly show us the way to rationality" (p. 327). In this connection, Lord Keynes is presented as the "giant" intellect in economics. His "pump-priming" theory is advocated as

a most efficacious full-employment mechanism."

Despite his deferential regard for economic thinking, Bitsaxis often dwells upon the inadequacies of economics, portraying that discipline as being in the developmental stage and thus exhibiting dramatic intelligence gaps. The shortcomings of economics are particularly acute insofar as the nature and control of inflation are concerned. In this area the author tells us "... economists only agree that something has to be done. No more" (p. 309).

As it has been for multitudes of political scholars and practitioners alike from time immemorial, the central political question for Bitsaxis is how to balance liberty and order. In short, how much freedom and how much regulation are necessary for the public good? To Bitsaxis, the democratic rule-making mechanism militates against a proper approach to this and the other perplexities of government. Modern democracy, he feels, is excessively guided by narrow-minded and selfish interests. Consequently, an efficient maximization of the general welfare is barred. Such a maximization requires rule by intellectual elites—by scientists. To quote: "The logic which leads to a dentist and not to a plumber, when a problem is a toothache, will drive us sooner or later to a Government of Scientists, for they are closer to the truth whether political, economic, or intellectual" (p. 141).

The advocacy of rule by an intellectual elite is not a new idea in political thought. Yet Bitsaxis fails to demonstrate that the intelligentsia does in fact have those special governmental talents he ascribes to it. Nor does Bitsaxis devote any analysis to that traditional main current in democratic thought alleging that popular direction in political affairs is a foundation of beneficial government.

Compounding these deficiencies is Bitsaxis's complete failure to probe the merits of the pluralist school. This school urges that the public interest is advanced not through rule by an intellectual elite but essentially through the promotion and achievement of compromises among competing particular interests in society, and by deference to the primary dimensions of popular opinion.

Shortcomings aside, *Welfare Versus Freedom* offers public policy students helpful reviews of major contributors to the field of political economy—reviews which would be particularly useful to novitiates seeking a broad survey of the literature. Additionally, the volume entices and prods the reader to grapple with the kaleidoscope of questions associated with such



core political issues as the role of government in promoting a virtuous society, and the balancing of liberty with order. Importantly, the author's shortcomings in attacking these core problems challenge the reader to do better.

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**The ABC of Communism.** By N. Bukharin and E. Preobrazhensky. Introduction by E. H. Carr. (Baltimore, Md.: Penguin Books, 1969. Pp. 481. \$1.75.)

**The ABC of Communism.** By N. Bukharin and E. Preobrazhensky. Introduction by Sidney Heitman. (Ann Arbor, Mich.: Ann Arbor Paperbacks, 1966. Pp. 422. \$2.95.)

**Imperialism and World Economy.** By Nikolai Bukharin. Introduction by V. I. Lenin. (New York: Howard Fertig, 1966. Pp. 173. \$9.50.)

**Historical Materialism: A System of Sociology.** By Nikolai Bukharin. Introduction by Alfred G. Meyer. (Ann Arbor, Mich.: Ann Arbor Paperbacks, 1969. Pp. 320. \$3.25.)

For many years after his execution, Nikolai Bukharin was remembered almost only as the chief defendant in the notorious Moscow Purge Trial of 1938. The result was to obscure his central and representative role as a Soviet founding father, and to distort an important era in Soviet history.

Until his political defeat by Stalin in 1929, Bukharin was the acknowledged official theorist of Russian Bolshevism and the international communist movement. More important, after Lenin's death in 1924, Bukharin became co-leader of the ruling Soviet party and the leading representative of a non-Stalinist form of Soviet development. To understand the nature and political strength of his domestic programs is to recognize as myth, promoted by Stalin and Trotsky alike, the commonplace notion that the factional struggles and debates of the 1920s revolved around a choice between Stalinism and Trotskyism. As most Bolsheviks, including Stalin, understood, the real alternative to Stalinism in "building socialism" was the gradualist policies originating with Lenin's NEP in 1921 and associated later with the party's Bukharinist wing. Based on political moderation, a combination of plan and market, and evolutionary development, they are policies revived in recent years by anti-Stalinist reformers in Eastern Europe and the Soviet Union.

These three reprints are only part of a current revival of Bukharin's publications. (Two of his major works have recently appeared for the first time in English translation: *The Economics of the Transformation Period* [New York: Bergman Publishers, 1971], a famous

theoretical treatment of the party's policies during the Russian civil war of 1918–1921; and *Imperialism and the Accumulation of Capital* [New York and London: Monthly Review Press, 1972], a critique of Rosa Luxemburg's theory. Basic statements of his ideas on Soviet development in the 1920s, notably *Put' k sotsializmu* [*The Road to Socialism*] and *Politicheskoe zaveshchanie Lenina* [*Lenin's Political Testament*], await translation.) They do not relate directly to his programmatic ideas of the 1920s, but they do indicate the theoretical work and reputation that preceded and enhanced his political role during that crucial decade. Though different in nature and purpose, the three books, and especially *The ABC of Communism and Historical Materialism*, became his most widely read and translated works. Together they formed the doctrinal education of a whole generation of Soviet and foreign Communists and were largely responsible for Bukharin's initial fame as the ideological spokesman of the Bolshevik revolution.

*The ABC*, however, should not be read as a specifically Bukharinist document. Written in 1919 with Evgenii Preobrazhenskii, another young Bolshevik leader and later Bukharin's main intellectual opponent in the debates of the 1920s, it was designed as a popular exposition of the party's newly adopted program, as "an elementary textbook of communist knowledge" accessible to "every worker and peasant," rather than as an original contribution to Marxist thought. The text follows that of the party program, detailing each point, omitting no important subject from the rise and collapse of capitalism to the situation in Soviet Russia and the future socialist organization of all aspects of social life. Comprehensive and plainly written, the book was a great success, quickly becoming a party canon and the most famous handbook of pre-Stalinist Bolshevism.

What gave *The ABC* its extraordinary appeal, and recommends it even today, is its encyclopedic chronicling of the party's utopian expectations and militant optimism during the heady civil war years. Fighting for survival against White and foreign armies, the Bolsheviks had improvised a set of extremist policies known ambiguously as "war communism," a hopeless tangle of military expediency and ideological fervor. By 1919 no one could separate the two elements, and indeed historians still debate which predominated. Though shortly outdated by events, *The ABC* speaks in the voice of this "heroic period" in the revolution, and thus provides scholars and students today with a vivid sense of Bolshevik thinking during those critical and formative years. Both

of these new editions include the 1919 program as well as useful scholarly introductions, one by Sidney Heitman, an expert on Bukharin, the other by E. H. Carr, whose wide-ranging essay has the additional virtue of dispelling the charge that the great British historian lacks sympathy for utopian and losing causes in history.

While *Imperialism and World Economy* and *Historical Materialism* also became orthodox texts, they were more original and distinctly Bukharinist achievements. *Imperialism* retains its interest for two reasons. Written in 1915, it presented the first systematic theory of neocapitalism and imperialism by a Bolshevik, developing the now famous formulation—monopoly capitalism leads to imperialism, imperialism to war, and war to proletarian revolution—a year before its enshrinement in Lenin's *Imperialism, the Highest Stage of Capitalism*. Lenin's debt to Bukharin's book is clear, as is Bukharin's important role in the shaping of Communist ideology on the eve of the Russian revolution. (There is an even more striking relationship between Bukharin's 1915–16 writings on the state and Lenin's *State and Revolution*, written in 1917). If nothing else, this reminds us that "Leninist theory" was in fact the product of several minds.

Equally interesting, however, is the subtle but significant way Bukharin's conception of neocapitalism differed from Lenin's. Influenced by Rudolf Hilferding's *Finance Capital* (1910), and to a greater degree than Lenin, Bukharin portrayed national monopoly capitalism, or "state capitalism," as a system that through monopolization and trustification had virtually overcome the internal crises analyzed by Marx as the sources of socialist revolution. In *Imperialism*, and more explicitly later, Bukharin suggested that only external pressures of international war could now bring about revolutionary breakdowns in advanced capitalist systems. His innovative treatment of capitalism's internal reorganization and the new interventionist role of the modern state place him early and eminently among the disparate theorists, from Hilferding to Galbraith, who have perceived, with Marxist alarm or liberal relief, the rise of "organized capitalism" since Marx. During the wartime years of 1915–16, Bukharin's conception remained a little-noted theoretical difference between him and Lenin. But in 1928–29 it became a major issue in the fateful struggle between Stalinists and Bukharinists over Comintern policy, part of Bukharin's opposition to Stalin's claim that Western capitalism was on the verge of Communist revolution.

*Historical Materialism* was Bukharin's most

"academic" book. Published in 1921, it was intended to serve three related purposes. One was to provide a popular exposition of Marxist social theory. Done systematically and with some flair, this gave *Historical Materialism* its great popularity, and, as Alfred G. Meyer points out in his perceptive introduction, makes it "an important landmark" between Engels's *Anti-Dühring* and the present-day Soviet compendium, *Fundamentals of Marxism-Leninism*. Bukharin's second and third purposes worked against this more conventional one: he wanted to answer Marx's most formidable critics, especially those representing the emerging schools of modern sociology, and to make his own original contribution to Marxist social theory.

As Seymour Martin Lipset has observed, *Historical Materialism* "represents the one sophisticated effort by a major Marxist to come to terms with the emerging body of sociological theory and research." (Introduction to Robert Michels, *Political Parties* [New York: Free Press, 1962], p. 27, n. 22.) Because he was an authentic "seeking Marxist," *Historical Materialism* shows also the influence on Bukharin of Marx's intellectual rivals—among them, Durkheim, Pareto, Weber, and Michels. Not the least evidence for this is suggested by the subtitle. In presenting historical materialism as a "system of sociology," Bukharin went against a substantial antisociological bias in earlier Marxist thought and at the same time established in Soviet thought a counterpoint to the subsequent Stalinist tradition, which proscribed the whole discipline of sociology for a quarter of a century.

His original contribution grew out of this effort to come to grips with modern sociology. To purge Marxism of what he regarded as its lingering, unscientific Hegelianism, Bukharin recast the concept of dialectics and social change in terms of a mechanical equilibrium model of society. His mechanism and reinterpretation of the Hegelian triad as equilibrium theory (original equilibrium; disturbance of equilibrium; re-establishment of equilibrium on a new basis) made *Historical Materialism* highly influential and controversial. And despite its crudities and inconsistencies, it remains an interesting chapter in contemporary social thought, relevant, for example, to ongoing disputes among social theorists and to the contention that the "choice of an equilibrium model logically precludes a revolutionary ethic. . . ." (Cynthia Eagle Russett, *The Concept of Equilibrium in American Social Thought* [New Haven, Conn.: Yale University Press, 1966], p. 53.)

That these and other of Bukharin's writings

are being reprinted in translation is testimony to their importance as historical documents and chapters in the history of Marxist ideas. That they are still proscribed in the Soviet Union is further evidence of the present leadership's profound conservatism and disdain for its own revolutionary and intellectual heritage.

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**Les Origines du Gauchisme.** By Richard Gombin. (Paris: Editions du Seuil, 1971. Pp. 186. 6F.)

The social upheaval in France of May-June, 1968, when the French Communist Party was seriously outflanked on its left for the first time, produced considerable interest in those extreme left-wing radicals who are called *gauchistes*. Several books on the far left have been published recently, among them this book by Richard Gombin, a researcher at the Fondation Nationale des Sciences Politiques.

Gombin's book is an effort to describe the intellectual origins of the ideas that Gombin believes are central to the thinking of the people and groups who constitute the far left in France. The book treats in brief compass criticisms of the bureaucratic character of the Soviet Union, particularly those set forth during the 1950s in the review *Socialisme ou Barbarie*; revisions of Marxism proposed in the review *Arguments*, also during the 1950s; Henri Lefebvre's critique of daily life, which appears to have been adopted by the *Internationale situationniste* group and diffused as a form of cultural revolt; and the emphasis on *autogestion* by means of councils. (When employed in the context of industrial organization, *autogestion* means workers' control; when employed in a more general sense, as it often is by the far left, it is best translated as participatory democracy.) In a final chapter, Gombin discusses those ideas in the light of the confrontation politics of the late 1960s and admits that no clear relationships between those ideas and the shock tactics of that period have been established.

Gombin defines the *gauchistes* as "that fraction of the revolutionary movement which offers, or wants to offer, a radical alternative to marxism-leninism as a theory of the working-class movement and its development" (p. 18). The definition is highly restrictive and eliminates a large portion of the far left, including the *Parti socialiste unifié* (PSU), which is excluded because it combines revolutionary aspirations with reformism, as well as various dissident communist groups such as the Trotskyists and the Maoists. Gombin excludes the

dissident communists because they operate within the marxist-leninist tradition as opposed to trying to replace it; "they offer an alternative," he says, "but an alternative to the direction of the party and not to marxism-leninism" (p. 19). That distinction seems strained and not fully consistent with the book. Gombin refers to Lukacs and Lefebvre as fountainheads of *gauchisme*, but it is doubtful that they were any less interested in influencing the conduct of communist parties than was Trotsky, whose followers Gombin does not count as *gauchistes*, although he believes that Trotsky provided *gauchisme* with "its point of attack: the Soviet bureaucracy" (p. 36).

Gombin emphasizes the more obscure, esoteric sources of *gauchisme* over the broad, historically evident currents of thought to which its main tenets, particularly *autogestion* in the form of workers' control, are related. There is much in the thought and behavior of the people on the far left that calls to mind Proudhon (minus his puritanism and contempt for women) and revolutionary syndicalism, which dominated the early years of the French working-class movement. Gombin mentions Proudhon in passing, and he refers briefly to the similarities between certain *gauchiste* themes and revolutionary syndicalism, but he questions whether the latter may be regarded as a source of *gauchisme* because there is no clear indication of how that tradition might have been passed on to contemporary actors. The question of how historical traditions are transmitted is perfectly legitimate, and in the case of revolutionary syndicalism, the answer is most likely to be found in the genealogy and sociometry of the leaders of the French trade union movement. But the question applies equally to some of the works which Gombin does treat as sources of *gauchisme*. How many *gauchistes* have heard of Anton Pannekoek, to whom Gombin attributes central importance in the development of the notion that workers' control should be exercised through councils?

The main elements of *gauchisme* that emerge from this book are alienation, adopted from the early writings of Marx and employed as a critical tool, and *autogestion*, which is never described in concrete institutional terms but which is proposed as a general solution to the problems of social and political organization. Gombin writes sympathetically and almost wholly uncritically about this current of thought, which seeks to put everything into question all at once, but never doubts its own concepts.

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**The Evolution of Dialectical Materialism: A Philosophical and Sociological Analysis.** By Z. A. Jordan. (New York: St. Martin's Press, 1967. Pp. xvi, 490. \$13.45.)

Professor Jordan's polemic is aimed against the twin pillars of the edifice of Marxist theory: (i) the science of historical materialism (HM) and (ii) the philosophy of dialectical materialism (DM), the epistemological meta-theory of the science. He conducts the polemic via a two-pronged attack:

(1) He rejects the view that a monolithic doctrine can be extracted from the works of Marx, Engels, Lenin and Stalin. He splits the monolith by displaying theoretical ruptures, first between Marx and Engels, second between Marx-Engels and Lenin-Stalin (the latter rupture being uneasily bridged by Plekhanov). The claim here is that since Marx was not a philosophical materialist, he could not have been the father of the philosophical doctrine of DM; and since Marx had no coherent theory of history, if he was the father of HM, then HM too is incoherent.

(2) He rejects DM (now orphaned) on numerous grounds, most importantly because it allegedly infringes Popper's principle that a theory must be refutable if it is not to be mere metaphysics.

The following two arguments, from the first prong of the attack, are representative of the general level of the polemic:

(a) "Anthropological naturalism" (pp. 16-64). Jordan distinguishes materialism from naturalism. According to him, *anthropological naturalism* asserts "the priority of external nature" but abolishes the distinction between man and nature. From this arises the phenomenological view that the concept of nature is "a social and historical concept," i.e., the "social construction of reality." He defines *materialism* in reductivist terms: in history it "identifies the absolutely primary factor of history with some extraneous physical factor"; in social science it claims that "every sociological statement is logically reducible to a conjunction of psychological statements which themselves are reducible to statements about physiological or chemico-physical states of the human organism" (p. 54). Having thus characterized the two positions, he concludes that since Marx did not maintain the latter, and since the former can be extracted from certain of his texts, Marx was an anthropological naturalist, not a materialist. The beauty of this conclusion for Jordan is that it effectively immunizes Marx's theory from having any effect on historical or sociological explanation, since anthropological naturalism is essentially a non-

causal, nonscientific ideology. But the conclusion does not in fact follow from the premisses presented, because anthropological naturalism is not the only alternative to reductive materialism to be found in Marx's texts. On the contrary, his mature texts provide the elements of a theory different from either reductive materialism or anthropological naturalism, namely the theory of determination by the mode of production. If by semantic fiat Jordan forbids us to call this theory materialism, nothing is lost (a rose by any other name . . .). The important principle is that we should not be driven into the innocuous vacuity of naturalism for fear of the unacceptable consequences of reductivism. (Note the innocent contempt for periodization of Marx's works, as Jordan extracts "Marx's philosophy" from selected passages of the *1844 Manuscripts*, *The German Ideology* and *Capital* I.7.)

(b) Marx and history (pp. 297-318). Here Jordan returns to the central concept of HM, determination by the mode of production. He distinguishes two theories within Marx's writings, the one "methodological," the other "metaphysical."

The *methodological* theory, according to Jordan, is not concerned with the ontological relationship of base to superstructure, but rather with "the principle of evolution and conformity of human behaviour to natural laws of co-existence and succession." Its basic assumption is that men's behavior and ideologies correspond to their material production and more specifically that ideological domination corresponds to economico-political domination. Posed in such general terms the theory is clearly untestable. Yet when in his political texts Marx makes the theory more specific, by demonstrating connections between the economic, political and ideological levels as reflected in the political class struggle, Jordan deems these developments of the theory illegitimate, and thus condemns the theory to the realm of untestable non-science (pp. 305-6). But even if Marx had appeared to produce a testable theory, Jordan could have trumped his card: "If Marx believed that his theory of history could be validated by testing the conclusions derived from it, as a hypothesis of natural science is tested, he was surely on the wrong track; it is clear today that no historical hypothesis can be validated in this way" (p. 305). Since Jordan apparently identifies scientific method with the hypothetico-deductive method (e.g., pp. 196, 200, 316), it follows that history for him is not a science, indeed that it cannot yield knowledge in the normal sense at all: "Marx held the naive belief, common

at the time [sic] . . . that one and only one point of view can truly fit historical facts . . ." (p. 316).

The *metaphysical* theory (Jordan's name for the theory which causally relates base to superstructure) is rejected, first, because it cannot be validated, and second, because it employs theoretical entities (productive forces and productive relations) which are unreal: ". . . distinguishable from each other in thought only, not in fact . . . The construct of productive forces has a connotation but no denotation" (p. 310). This latter objection has now been largely overcome by E. Balibar, G. A. Cohen, and others, in their distinction between economic capacities and juridical rights and powers. To Jordan's statement: ". . . the modern supporters of the Marxian theories should be able to answer this objection" (p. 310), the answer is that Balibar already had in 1965. The first objection, concerning validation, still stands in need of a full theoretical reply, but one can be sure in advance, from the arguments cited above, that Jordan will rule out any such attempts as illegitimate. He has produced the perfectly self-validating prediction.

In this "philosophical and sociological analysis," philosophy is used to attack the logical structure of the theories, and sociology to explain how such illogical systems came to be imposed. But the sociological component is lightweight, consisting of the single reductivist notion of "socio-centric cosmology." The DM of Plekhanov, Lenin and Stalin is explained as a crude anthropomorphism which "first ascribes certain distinctively social attributes . . . to the universe, and having construed it as a socio-cosmic structure, derives from the order of the universe the basic characteristics of human society" (p. 206). This account has a certain intuitive appeal but lacks serious theoretical credentials. Jordan finds the real (?) origin and point of return of the ideological system in "a universe brimming over with strife, conflict and contradictions," which "offers self-evident advantages for a social and political theory based on the concept of class-antagonism": this "was the case with Plekhanov and continues to be so in the Soviet philosophy of today" (p. 205). The implications of the "explanation" seem to be first that there is no class-antagonism in capitalist countries since DM is not dominant in them, and second that the situation facing Plekhanov was identical, in relevant respects, with that facing Soviet philosophers today. Since these conclusions are evidently false, the "explanation" must be abandoned or radically modified, otherwise Jordan's own comment applies: "A theory that

does not take any risk of being refuted has no scientific content" (p. 196). (In fact the first steps towards relating ideological developments under Stalinism to the class struggle have been made by N. Poulantzas in *Fascisme et Dictature*, a work firmly based on the fundamental concepts of HM.)

The book is excellently documented; the logical distinctions are lucid and many of the refutations of specific theses are more elegant than those of earlier critiques. But the polemic as a whole is profoundly ambivalent, reflecting the ambivalence of the author's own theoretical stance. His attitude towards history is totally skeptical, involving epistemological relativism and the assumption that history cannot fulfill the criteria of scientific discourse (pp. 308, 316). Yet elsewhere he champions the scientificity of history when he denounces the imposition of extraneous "methodological principles upon the natural, social and historical sciences" (p. 395). He uses relativism to exclude the very possibility of HM; he uses Popper's falsificationism to exclude DM and also to cope with the more specific theses of HM. Yet relativism and falsificationism are incompatible. Perhaps the ambivalences all stem from the curious conception of philosophy presented on p. 269: "Philosophy in the Socratic sense is a way of life characterized by devotion to truth and love of wisdom. Philosophy in a more pedestrian sense means submission to an intellectual discipline, the acceptance of logical rules and factual evidence, which enable others to ask questions. . . ." These criteria are designed to exclude Stalin from the class of philosophers, which they succeed in doing. But they fail to mark off philosophy from any other rigorous intellectual practice. The formulation is entirely empty and symbolizes the lack of any coherent account of the role of the epistemologist in political theory. Without such an account, this kind of *philosophical* polemic is doomed to failure.

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**Dualistic Economic Development: Theory and History.** By Allen C. Kelley, Jeffrey G. Williamson and Russell J. Cheetham. (Chicago, Illinois: University of Chicago Press, 1972. Pp. 339. \$12.50.)

This volume represents a stimulating attempt to review some of the major theories of dualistic development, to present the authors' (henceforth K-W-C) own effort, and to evaluate it by reference to the Japanese historical experience. Since Japan represents one of the few available historical laboratories for an ex-

amination of competitive development theories claiming relevance to the contemporary developing world, the effort made in this volume is an entirely reasonable one, meriting the applause of both theorists and policy makers. Essentially three explanatory models are featured: the Fei-Ranis classical dualistic model, the Jorgenson neoclassical dualistic model, and K-W-C's own neoclassical dualistic model. Not surprisingly, their model wins hands down, Jorgenson places second, and Fei-Ranis is lucky to show. It is, nevertheless, more in a spirit of sorrow than wounded pride that I enter my reservations with respect to the present volume.

One critical comment of interest to the general reader is that the effort to survey the relevant development literature is, at one and the same time, too ambitious and not ambitious enough. Many bases are touched—Uzawa models, Chenery's work, uncertainty, consumption models—but mainly once over lightly, with neither the full scope of these contributions nor their relevance to the question at hand here fully demonstrated. While the specialist may find this only mildly irritating, it will antagonize those readers who may be looking for a less cavalier treatment of at least some of the important related discussions of dualistic growth and development.

But the main focus of this volume, on which it has to be judged, is the presentation of the K-W-C model and its performance in contrast to the two competitive entries. I found the summaries of the Jorgenson and Fei-Ranis attempts to model dualism both illuminating and essentially fair. Aside from the basic question of what precisely is meant by "dualism" in the neoclassical case (surely the wage-marginal product relationship on p. 37 was intended to be reversed), I have no particular quarrel with their bare-bones description of the two competitive models. The finding of Marglin and Dixit that both a classical and a neoclassical model of the Jorgenson type can be seen as consistent with the Japanese historical record—depending on the specific production function assumptions, etc. made—are quoted approvingly, (which should help put that particular phase of the *Methodenstreit* to rest). But since the volume claims to present a superior version of the neoclassical model in terms both of theoretical structure and of a superior fit to the historical Japanese experience, it is well to examine that claim.

The K-W-C view of the world is basically that full employment is observed—to which they are forced to add the assumption that the equilibrium wage is always in excess of the

caloric minimum—a convenient but not necessarily accurate piece of neoclassical baggage (think of Java or Bangladesh). It is true (p. 41) that, in a dynamic context (i.e., when agricultural productivity is rising), changes in the wage rather than the applicability of the marginal productivity theory vs. classical/institutional theories are relevant. Nevertheless, it is still necessary to demonstrate the superiority of both the building blocks and the descriptive powers of their model.

First, the model itself differs in three major ways from the Jorgenson and Fei-Ranis versions, that is, capital instead of land is considered the main cooperating factor in the agricultural sector of the dualistic economy. Second, the rule of consumer demand is brought in explicitly through the stipulation of dualistic demand conditions. Third, special types of technology change are assumed for the agricultural and non-agricultural sectors. There is very little empirical defense of this "revisionism" concerning the building blocks. For instance, with respect to the introduction of capital into the agricultural production function, K-W-C have surely committed a worse crime by omitting land (see the work of Ohkawa and Rosovsky). I believe the dualistic demand conditions to be overspecified—a somewhat technical point. Moreover, since the specific (factor-augmenting) type of technological change assumed is not defended in terms of any extra-model inductive evidence, one suspects it was no accident that such assumptions were necessary to generate the needed (and unusual) constant wage pattern in a neoclassical world.

So much for the machinery. Other questions arise when the Japanese data are fed into it. Given initial conditions based on an amalgam of contemporary developing country parameters, the model is then permitted to generate such common indices of growth as labor productivity, savings rates, sectoral employment, output distribution, etc. which are then compared to the actual historical values. The correspondence is found to be "close," which naturally encourages the authors to enshrine their model as superior.

The issue of "closeness of fit" is difficult but can be addressed in at least two ways: first, while I do not wish to dispute the authors' judgment, it is not clear by what standards an unfavorable verdict would have been rendered (e.g., even when they undertake a nontrivial shift from an equilibrium to a disequilibrium version of their model, the fit remains "good"). Second, I believe that a very large number of sensible competitive models would give a

"close" approximation to the stylized facts of Japanese economic history, just as a variety of modern growth models have been harnessed to "explain" the stylized facts of the advanced economy. Models are indeed "a dime a dozen"—and an inventive mind can find a substantial number of combinations of assumptions on the production function, on technology, on factor growth rates, etc. to yield any "desired" outcome. What is less easy and more costly is the defense of a particular model in terms of the underlying richness of the behavioral assumptions made. And it is here that we unfortunately find the K-W-C candidate wanting, in spite of the claims made for it.

In the final analysis, a volume of this kind must be judged in terms of the new ground it breaks. Unfortunately, there is relatively little to point to here. The hitherto neglected open economy dimension, involving trade, technology transfer and foreign capital, which is certainly of central importance to an explanation of dualism in the Japanese case, remains unaddressed and unresolved. No attempt is made to break down the Japanese historical record into analytically meaningful subphases such as a classical period followed by a neoclassical and/or a land-based phase followed by unskilled labor-based growth phase. The demographic dimension is treated conventionally, i.e., exogenously, a special disappointment given the unusual expertise of the authors in this field. All in all, the present authors must be applauded for presenting an interesting, if highly selective and incomplete, survey of the current literature on the very important subject of economic dualism; they fall short in their claims with respect to the presentation of a superior paradigm; I join them in the call for more work, including more of their own perceptive contributions.

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**The Pure Theory of Law.** By Hans Kelsen. Translated by Max Knight from *Reine Rechtslehre*, 2nd edition, 1960. (Berkeley: University of California Press, 1970. First published 1967. Pp. 356. \$3.75, paper.)

**What is Justice: Justice, Law, and Politics in the Mirror of Science.** By Hans Kelsen. (Berkeley: University of California Press, 1971. First published 1957. Pp. 397. \$16.00.)

These reprints of two of the most celebrated works of the late Professor Hans Kelsen will constitute a valuable addition to the library of any student of legal or political theory. *The Pure Theory of Law* was the last full-length account given by Kelsen of the famous posi-

tivist legal theory with which his name has been associated for more than half a century. Although the principal features of the theory remained unchanged throughout the many re-statements of it made by Kelsen, there were important topics on which he changed his mind from time to time. *What is Justice* is a collection of essays in which the conclusions of his theory of law, and the theory of norms upon which it is based, are applied to a variety of topics within the traditional arena of political philosophy—justice, natural law, absolutism and relativism, coercion, obedience, "normative" and "scientific" judgments. In *What is Justice*, however, the version of his theory which is applied is not that contained in *The Pure Theory of Law*, but that appearing in an earlier work by Kelsen—*General Theory of Law and State*, 1945.

There are important differences between these versions of the theory. For example, in *General Theory of Law and State* and in *What is Justice*, Kelsen is at pains to repudiate the imperative theory of law: we do not need the concept of the individual human will in order to understand the nature of legal rules (norms). But in *The Pure Theory of Law*, although the identification of "norm" with "command" is still denied, yet we are told that every norm is the content of an act of will.

"It is incorrect, therefore, to characterize norms in general, and legal norms in particular, as the "will" or the "command" of the legislator or state, if by "will" or "command" a psychological act of will is meant. The norm is the *meaning* of an act of will, not the act of will" (*Pure Theory of Law*, p. 10).

Nor can *The Pure Theory of Law* necessarily be taken to represent Kelsen's last word on his own theory. He apparently approved this translation of a work in German published in 1960, but it would be bold to affirm that this approval entails a repudiation of anything published since 1960 in which Kelsen expresses opinions inconsistent with those of *The Pure Theory of Law*. For example, in an essay published in 1962, he asserts that which is denied in his other writings, namely, that it is possible for a legal scientist to describe two contradictory but simultaneously valid norms as parts of one and the same legal system ("Derogation," in *Essays in Honor of Roscoe Pound*, R. Newman, ed.). For a detailed bibliography of the works of Kelsen published in English, see *Essays in Honor of Hans Kelsen* (California Law Review ed., 1971).

The central contention of Kelsen's pure theory is that "law" is to be understood as a system ("order") of abstract, normative proposi-

tions ("norms"). Law cannot be identified with any selection of datable occurrences in social life, nor with any set of statements of fact recording or predicting such occurrences. The philosophical proof for this contention is to be found by examining categorical, descriptive statements about the law. Statements conveying information about the law cannot be transmuted, without loss of meaning, into statements about past, present or future events. If I visit your town and ask you whether parking is permitted in the main square on a Saturday afternoon, you may tell me that it's illegal. You have not committed yourself to predicting that it is more or less likely that, if I do park, my car will be towed away or I will be fined. You may indeed go on to say: "Actually, the regulations are seldom enforced"; or: "The police can always be bribed." You have only committed yourself to the assertion that the normative proposition—"parking is prohibited in place X on Saturday afternoons"—is one which forms part of the present law of the country (having its origin in a legislative source authorized, ultimately, by the constitution of the country, and not so far having been repealed).

This view seems so obviously correct that it might be regarded as trite, were it not for the wealth of legal literature in which it has been denied. In this literature, the justifiable skepticism engendered by the many occasions on which eminent lawyers and judges express decided, but contradictory, views as to what the law requires, has been generalized into the manifestly false proposition that it is never possible to describe the law—in the sense of an abstract rule—about anything. (See, e.g., J. Frank: *Law and the Modern Mind*.) According to such skeptics, "law" ought to be equated with what judges and other officials do, or with statements describing or predicting what they do. It seems to be conceded, however, that, as things are, lawyers speak in terms of Kelsenian "law"; albeit they do this—so the argument runs—only as part of the gigantic social hoax, that knowable rules of law can ever clearly indicate what officials ought to do. Even from the standpoint of extreme skepticism, therefore, the "law" which Kelsen analyzes is the "law" which legal science purports to describe.

The motivation of this skepticism is to shift the center of legal interest away from rules to the social environment which produces and uses them. It is questionable whether this aim is furthered by denying that the nature of law is what Kelsen said it was. We must know what "law" is if we are to assess its effects on so-

ciety: "The function of the legal norm for the sociology of law is to designate its own particular object, and lift it out of the whole of social events. To this extent, sociological jurisprudence presupposes normative jurisprudence" (*What is Justice*, p. 270).

One of Kelsen's principal failings was that he did not appreciate that lawyers ("legal scientists") may have professional information to give even where they cannot make categorical statements about the legality or illegality of conduct. Owing to the absence of binding precedent and legislation directly bearing on a topic, it may not be possible to point to a "valid norm"; yet it may be possible to make informed guesses as to the norms which the superior courts will, retrospectively, create. The guesses will be informed by pointing to those free-floating principles and policies embedded in legislative source materials which the courts are known to use as triggers for their legislative choices. (Cf. R. M. Dworkin 35 U. CHI. L. R. 14 [1967].)

Kelsen's central contention is marred by his attempt to pray in aid of it the moral philosophical cleavage between *is* and *ought*. No such support is needed; and whereas one regularly meets descriptive statements expressed in terms of "legal" or "illegal," "descriptive ought-statements" are unfamiliar and conceptually odd.

This is typical of a tension in Kelsen's writings between sensible insights (worthy of logical exegesis), and exotic pieces of philosophizing. It can be seen in his treatment of legal theory's oldest trip-wire—the concept of validity, and in his most famous invention—the "basic norm" ("grundnorm").

Kelsen investigates the way in which legal norms may be made the basis of relative value judgments. That they are so used is undoubtedly true: the values of legality and constitutionality form part of the artillery of political controversy, and it is because these values are role-values for officials that even a sociology of law with an institutional focus must presuppose the Kelsenian concept of law. Yet Kelsen—as a consequence of his neo-Kantian epistemology—is intoxicated with the idea that "valid" is an absolute quality, the equivalent for norms of "true" in the case of factual propositions; and this leads him to assert that conflicting legal and moral norms cannot both be valid. However, he revised this view in subsequent works; *vide* his *Essays in Legal and Moral Philosophy* (D. Reidel Publishing Co., 1973).

Kelsen investigates the essential presupposition which must underlie any attempt to de-



scribe the law. He finds it in the sources existing in a historical constitution, or in customary practices, which are necessarily assumed to be the only ultimate sources from which valid legal norms can be derived. Legal science differs from ethics by its presupposition of a finite list of sources, hierarchically arranged; and by the fact that its utility depends on the sources it presupposes being those which are successfully used in any territory. These truths Kelsen expounds picturesquely, when he speaks of the presupposition of a (by and large effective) basic norm. This single concept has produced more literature about Kelsen than anything else he wrote, and his view that the basic norm changes when there is a revolution has been used by judges in Pakistan, Uganda and Rhodesia to justify their change of allegiance. (I have discussed these "revolution" cases in 29 C. L. J. 103 [1971].)

The term "pure," which Kelsen applies to his theory of law, can also mislead. It has a suggestion of superiority. By it, however, Kelsen merely intends to demarcate analytical jurisprudence, which must take as its context only the normative order which is the subject-matter of descriptive legal science, from moral or political evaluations, or sociological inquiries, which may have other contexts. To this end he attempts to distinguish the sense which concepts like duty, right, property, sanction, state, person and so on, have within the law from any sense given to them by other disciplines.

The contribution of analytical jurisprudence to legal and political thought may be valued high or low. As an exponent of it, no name has a wider international reputation than Kelsen's, and deservedly so.

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#### **Means and Goals of Political Decentralization.**

By Lennart Lundquist. (Lund: Studentlitteratur, 1972. Lund Political Studies, 12. Pp. 191. SK. 30.)

This doctoral thesis from the University of Lund in Sweden is an attempt first to analyze and define verbally the concept of decentralization and its elements, and then to apply these notions and definitions to a "content analysis" of the Swedish debate in the 1850s, and again in the 1960s, about the greater centralization or decentralization of municipal governments in regard to their structure and their tasks.

In the first, conceptual part of the work, Lundquist begins with David Easton's definition of politics as the authoritative allocation of values and applies it to the processes of po-

litical and administrative control. He then distinguishes several main stages of "the control process": 1. goal formulation; 2. prognosis about effectiveness of means and changes in the situation; 3. decision; 4. "steering" as the process by which the decision is transmitted to an "implementer"; 5. implementation by the implementer through the administrative process; 6. review, both as to the correctness of the manner of implementation and to the attainment of the outcome intended by the decision maker; and, though with less emphasis, 7. re-steering for correction.

Lundquist pays particular attention to the steering of the implementation process by the decision-making authority. Steering can be direct, through issuing specific orders and general rules, or indirect, through controlling the recruitment of personnel for the implementing agency, determining the organizational form of that agency, controlling its financial resources—or the flow of information to it—or some combination of these methods.

In terms of scale and territory, Lundquist distinguishes the "society," which corresponds to an entire country with its "political system" at that national level from its "political subsystem" of district or local government and politics.

From these distinctions, Lundquist derives his two concepts of "deconcentration" and "decentralization." In cases of concentration, he says, decisions are made and implemented centrally, at the level of the entire society and its national political system, whereas, conversely, in cases of deconcentration, decisions are still made centrally, but their implementation is dispersed and delegated to local agencies of the subsocieties of the central political system.

Genuine decentralization then differs from mere deconcentration in that the agencies at the subsociety level are primarily directed by, and responsible to, the political subsystem of that subsociety. Decisions here are made locally, their implementation being subject primarily to local steering, supervision, and review.

A fourth analytic chapter offers an overview of verbal "analysis categories" for the subsequent content analyses. These categories seem generally well conceived and clearly presented.

The next chapter presents a content analysis of the decentralization debate of the 1850s, based on about 1500 pages of Riksdag proceedings and about 200 press articles of the period. The analysis endeavors to show the distribution of themes, values, and schools of thought. The treatment is mainly qualitative; only one chart (Fig. 15, p. 135) indicates

roughly the quantitative distribution of themes. The findings are modest.

In his sixth chapter, Lundquist identifies a merger school which stresses the need for greater resources and capabilities for local government. These are to be attained by the voluntary merging of several small, rural communities into larger units of local government, more fit to cope with a variety of emerging problems.

A second though smaller group of spokesmen is identified by Lundquist as the transfer school since it stresses the possible transfer of additional functions to the communes. This school doubts the general efficacy of mergers. Rather, it stresses the goal of close contacts between local government and the population, to be fostered through small communes and spatial proximity to those whom they are to serve.

A third, still smaller group is identified by Lundquist as the localization school which proposes mainly the transfer of substantial resources from the state to the communes, so as to compensate them for their reduced autonomous tax base and for the increased services required of them.

The author concludes that the debate in the 1850s was mainly concerned with sketching a single "ideal type" of decentralization in order to present demands for greater decentralization and popular participation against the then highly centralized Swedish government. In the 1960s, however, these general goals had become part of the prevailing political consensus, so that the debate now had to hinge upon the best methods for preserving and enhancing these agreed-on values. In this change, Lundquist sees a shift from a normative to an instrumental emphasis which he interprets as evidence for a larger debate on the alleged "death of ideology" in recent politics.

Lundquist's conceptual analysis is stimulating and potentially useful. It refers to a large body of professional literature, including much American writing, but this literature is often used with very limited understanding. As the author frankly admits, he has produced a taxonomy; he has done so with a taxonomist's mind, reminding us of Linnaeus rather than of Darwin. His approach most often remains close to the surface. As a model, it lacks dynamics; as description, its sense of history and politics is weak. It is innocent of mathematics, either as a technique or as a way of thinking. Some of the works Lundquist cites include mathematical analyses, but there is no evidence that he understands them.

His claim to have taken the first step toward a theory of decentralization is questionable.

His description of the variables he proposes is not of the precision necessary for stating statistically testable hypotheses or making rigorous deductions. The logic of many arguments is quite loose. There is much opinion and speculation, and assumptions are not always clearly distinguished from conclusions. His conclusions are not easily verifiable by other analysts.

Despite these substantial weaknesses, Lundquist has done a prodigious amount of work. His rich compilation of ideas, problems, and evidence from public policy debates is likely to stimulate and help theorists to proceed with theory building. In a field that has long suffered from vagueness and neglect, he has made a worthwhile contribution.

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**The Earlier Letters of John Stuart Mill, 1812-1848.** Edited by Francis E. Mineka. Introduction by F. A. Hayek. (Toronto: University of Toronto Press, 1963. 2 volumes. Pp. xxvii, 784. \$25.00.)

**The Later Letters of John Stuart Mill, 1849-1873.** Edited by Francis E. Mineka and Dwight N. Lindley. (Toronto: University of Toronto Press, 1972. 4 volumes. Pp. xlvii, 2083. \$95.00.)

(Note: These two collections form volumes 12-17 of the *Collected Works of John Stuart Mill*, edited by J. M. Robson and others.)

Liberalism, individualism, utilitarianism—these are among the labels often used to describe John Stuart Mill's philosophic position. Yet it is awkward to classify him in this way, for he qualifies the views that seem to justify these labels and displays marked ambivalence with regard to them. Indeed, ambivalence is one of his distinguishing characteristics. He finds merit on both sides of many arguments. Of course he is liberal, yet he searches for a rationale for authority. He thinks of himself as a radical, yet he sometimes appears to be quite conservative. He is a utilitarian, yet sharply criticizes his Benthamite inheritance. He defends laissez-faire principles, but Sidney Webb was persuaded that had he lived another decade he would have been a Fabian Socialist. Always a democrat, he was an elitist as well. These mixed views had many sources. He was genuinely and remarkably open-minded. He cultivated a deliberate eclecticism that led him to combine the outlooks of Adam Smith and the St. Simonians of his father and Tocqueville; of Bentham and Coleridge. But above all, his mixed views resulted from his determination to hold on to old views even after they were undermined by dis-

lusionment and new understanding. He described this process in the *Autobiography*: "I found the fabric of my old and taught opinions giving way in many fresh places, and I never allowed it to fall to pieces, but was incessantly occupied in weaving it anew." The result was his attempt to combine what he called the philosophies of the eighteenth century and the nineteenth and his wish to be "many-sided," mixing the many "half-truths" by which he was surrounded.

The main outlines of his mixed views are revealed in his major writings, but a full, detailed statement, along with evidence on how his ideas changed and developed, is to be found only in the vast quantity of subsidiary writings—in the many essays and reviews published in the periodical press and in his letters, which in many cases are long, discursive, didactic, and revealing. The letters, therefore, provide important evidence that supplements the main corpus of his writings.

This is perhaps best illustrated by Mill's correspondence on democracy and the suffrage. Here we can trace the development of his views during four decades from the simple majoritarianism as advocated by his father and Bentham to a combination of universal suffrage with provision for minorities, especially the minority of the educated—one is strongly tempted and probably justified in saying the minority of intellectuals—who were not only to be represented, but were to have authority and leadership as well. Of course this understanding of "true democracy" was presented in *Considerations on Representative Government*. But the shifting path leading to it is to be found in the articles comprising "The Spirit of the Age" and in such essays as those on Samuel Bailey, Tocqueville, and John Austin—and in the lengthy letters to the St. Simonian d'Eichthal, to Tocqueville, Grote, Comte, Carlyle and many others. It is in these letters that we also find abundant evidence of Mill's brief rejection of majoritarianism and his frank defense of authority for an intellectual elite. And the letters also document the gradual modification of Mill's formulation of the relationship between the rival claims of the people and those with moral and intellectual authority. One can trace the shift from belief in rule by the intellectually best as a matter of propriety and efficiency to a later concern that the educated have a political role so as to avoid the suppression of their ideas and values. By 1860 it had become "a race against time, for if the American form of democracy [rule by "numerical majority"] overtakes us first, the majority will no more relax their despotism than a single despot

would" (*Later Letters*, p. 672).

Oddly enough Mill's greatest hero in this connection was Thomas Hare, whose ideas on proportional representation appeared to Mill as a magic-like device that allowed for the representation of minorities (and therefore the educated) yet without sacrificing majoritarianism. Hare had "for the first time, solved the difficulty of popular representation; and by doing so, to have raised up the cloud of gloom and uncertainty which hung over the futurity of representative government and therefore of civilization" (*Later Letters*, pp. 598–99). Hare provided "the real basis of a reconciliation between Radicalism and Conservatism" (p. 608). During much of his life Mill had been "troubled by the difficulty of reconciling democratic institutions with the maintenance of a great social support for dissentient opinions. Now," he told Hare, "your plan distinctly solves this difficulty" (p. 653). And, "If the Americans would but adopt your plan (which I fear they never will) the bad side of their government and institutions, namely the practical exclusion of all the best minds from political influence, would soon cease" (p. 654).

While these letters supplement what is otherwise known about Mill's intellectual development, their greatest importance is for what they reveal about him biographically. Mill, after all, scooped his biographers by putting out his own version, which was highly selective and necessarily partial. Yet it has shaped the conventional interpretation of his life and thought. The letters provide additional information that greatly modifies what the *Autobiography* tells us on such subjects as his relations with his family and with Harriet Taylor, and they reveal a good deal about his political ambitions and his gradual shifts in opinion as he reacts to new friends and reflects upon events.

The first characteristic of biographical significance to notice is the restraint, control, and lack of spontaneity in most of the correspondence, including the letters to Harriet. In view of the value he placed on spontaneity in *On Liberty*, this is especially noteworthy. The letters give the impression of having been composed with great care, and in fact many were written from drafts. And in the case of letters written to members of his family and to estranged friends they appear to have been written with the care that usually goes into diplomatic communications. Compared with typical letters of, say, Hume or Macaulay or Carlyle they are without humor, passion, and verve—indeed, without much affect. Many are didactic; all are intellectual creations.

These volumes also reveal the changing character of Mill's political ambition. He was elected to the House of Commons in 1865, but his parliamentary ambition had been at its height during the 1830s when his appointment with the East India Company prevented his seeking election. The letters reveal how much his thoughts were focused on parliamentary politics and how much he wanted to lead the Radicals in bringing about a realignment of parties so they would reflect the struggle between aristocratic and democratic forces. The failure of this enterprise, combined with Mill's disillusionment with simple majoritarianism, led to a redirection of his ambition. The end was still political, but the means became intellectual. The improvement of mankind was to come from moral regeneration, a new spiritual impulse, and from education, rather than solely from changes in institutions. This gave the intellectuals special responsibilities, and circumstances provided them with opportunities, for "great historical change . . . [was] either dawning or approaching its crisis" (*Later Letters*, p. 739). Within this context of expectations, ideas would be particularly influential, and this justified Mill's special provision for men of intellect and his own sense of mission.

Undoubtedly the most important biographical gap filled by the letters concerns Mill's relationship to Harriet Taylor. (It is a matter for regret that this edition is without Harriet's letters to Mill, for which one must go to Professor Hayek's *John Stuart Mill and Harriet Taylor* [Chicago, 1951].) Mill's extraordinary letters to her help us to evaluate his extravagant claims for her as a source of his ideas on the woman question, on economic organization, and on liberty. There is also evidence to support Ruth Borchard's argument that Harriet became for Mill a needed authority figure to replace his father. Mill sought her advice on all matters, trivial as well as serious, and even after she died it was, he said, "my principal anxiety . . . to do as exactly as I am able what would have been done if I had still my darling to guide me" (*Later Letters*, p. 671). Indeed, for the author of *On Liberty* this is surprising, though not the statement that his "entire faith in the ultimate possibilities of human nature was drawn from her own glorious character" (p. 601).

As a byproduct of the connection with Harriet, one finds evidence of Mill's atrocious conduct toward his sisters and mother, as well as to estranged friends such as Sarah Austin. The facets of his life that are revealed in these volumes seem endless. We find him struggling with serious illness—his own and his father's,

his brother's, his wife's—and its anxieties. He shows himself to be the most enterprising of travelers—walking prodigiously and even climbing Parnassus and ignoring the threat of brigands as well. His accounts of such travels in Greece (to the historian Grote) pleasantly convey his fascination with the locale that he recalls from legend and history, and these are among the most attractive passages in these volumes (*Later Letters*, pp. 779–781). His first reaction to Darwin's *Origin of Species* is noteworthy: "Though he cannot be said to have proved the truth of his doctrine, he does seem to have proved that it *may* be true which I take to be as great a triumph as knowledge and ingenuity could possibly achieve on such a question" (p. 695). And one can only be disappointed but not surprised by his judgment of Hegel: "conversancy with him tends to deprave one's intellect" (p. 1324).

A final word should be said about the fine work by the editors. The notes are filled with explanations of allusions and events and with biographical details; although inconspicuous, they are available when wanted—just what such notes should be. One regrets that it is necessary to use two indexes; convenience would have been served if the index to the *Earlier Letters* (1963) had been incorporated into the index for the *Later Letters* (1972). The introduction to the *Later Letters* by Professors Mineka and Lindley is itself an important contribution to the literature on Mill's life, especially in its emphasis on Mill's taste for social isolation, which becomes important in view of the radical individualism of his social and political doctrines. This collection, which consists of almost 2,400 letters, most of them not before published, is the result of continuing and often ingenious searches over thirty years, beginning with Professor Hayek's efforts during World War II (described in his Introduction to *The Earlier Letters*) and continuing with the work of the editors. The result is a finely executed and important publication that will have an enduring influence on scholarship.

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**The Politics of Communication: A Study in the Political Sociology of Language, Socialization, and Legitimation.** By Claus Mueller. (New York: Oxford University Press, 1973. Pp. 226. \$7.95.)

Some of the most exciting research in the social sciences in recent decades has dealt with language, much of it focusing upon unconscious constraints upon thought and perception. The application of this work to political

analysis is challenging and important, for it promises to throw new light upon virtually every facet of political science. The linguistic reshaping of cognition and behavior underlies a great deal of opinion change and many of the formal and informal interactions that take place in policy-making institutions. For political scientists, the challenge is to learn how to analyze the ties between language and our conventional subjects of study. Lévi-Strauss understandably regards linguistics as the basic social science.

About half of Claus Mueller's book deals with political language. Though brief and highly selective, his analysis is trenchant, especially in its exposition of the devices through which governmental regimes employ distorted communication to direct and constrain thought and behavior. There is also a discussion of "arrested communication," stemming from the limits a restricted speech code places upon the capacity to communicate, quite apart from governmental constraints. Professor Mueller presents lists of terms given new meanings under the Third Reich, and others that have acquired new meanings in East Germany in recent years. Though they make the point dramatically, these obvious examples may unintentionally create the impression that it is chiefly dictatorial regimes that employ distorted language to magnify support. All governments do so, and the effects may be especially powerful when skepticism is minimized by a setting of democracy and a distorting technique that is subtle or unintentional.

Because Mueller is so interested in the crude politicization of everyday life in totalitarian countries, he understates some of the most pervasive and subtle uses of distorted communication and legitimation in all societies. He recognizes, for example, that socially coerced political participation in totalitarian countries helps keep the individual powerless by shaping his thinking and legitimizing the regime. But socially coerced politicization that is touted as "self-government" curtails freedom, dignity, and meaningful dissent everywhere. It does so in extreme form among inmates of "progressive" mental hospitals, prisons, and schools in the United States; and in the measure that voting is a ritual of participation, politicization has something of this consequence for all voters. ITT has influence with little formal participation, and the resident of a "therapeutic community" participates all day without influence. The truly influential are free to avoid involvement in ritualistic politics. In this instance, as so often, political language confuses reason and shapes conformist behavior.

Some of the most significant contemporary research on language is being done by Basil Bernstein. He has identified a "restricted" language code common to the poor that severely inhibits their capacity to challenge conventional ideas and to develop as individuals. In contrast, the upper-middle class benefit from an "elaborated" language code that facilitates these talents. Mueller's chapter on "Social Class as the Determinant of Political Communication," based on Bernstein and related work, seems to me the best in the book, though some attention to the criticisms of Bernstein would have made it even better.

The volume discusses a rather wide range of other topics, including the bearing of the mass media and of ideologies upon maintenance of the status quo, the decline of legislatures and the ascendance of executives, contemporary challenges to governmental authority, and the role of intellectuals and professionals in these challenges. These commentaries are perceptive and enlightening, though rather diffuse.

In the end the author seems more impressed with the crisis of authority than with the legitimating processes he has analyzed: "In advanced capitalist societies riddled by affluence and poverty, the absence of effective legitimating rationales constitutes a problem to which the political system has no answer" (p. 182). It is a difficult question, but I suspect that he seriously underestimates the efficacy of language codes and other legitimating devices in advanced capitalist societies. The very national crises that constantly recur help legitimate political authorities and economic elites, for they justify augmentation of executive power and inequality of sacrifice; and though they are rarely "solved," they are inevitably succeeded by new crises with the same consequences.

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**The Correspondence of Jeremy Bentham. Vols.**

I and II edited by T. L. S. Sprigge; Vol. III edited by I. R. Christie. (London: Athlone Press, 1968. Vol. I: Pp. xlv, 383; Vol. II: Pp. xiv, 542. £12.00, set; Vol. III: Pp. xxxiv, 647. £12.00.)

Jeremy Bentham is the most influential of classical political theorists yet paradoxically the most neglected. Utilitarian assumptions pervade economics, the most developed social science and the model for others; they are encapsulated in the rational choice approaches increasingly applied in sociology and political science, and they provide a framework for such founding fathers of the contemporary discipline as Lasswell and Dahl. They are also

endemic in the practical criteria applied within the "administrative culture" of Britain and probably the United States; indeed most British politicians could be said to be practicing Utilitarians.

Why then the neglect of Bentham? There are three reasons, all declining in cogency at the present time so that we may well see a revival of attention within the next decade. The first is the intense dislike which Bentham's want-regarding libertarianism and rationalistic critique of established institutions arouses among philosophic idealists and conservatives. Since most people's ideas about his position are derived from the straw man in idealist writings, this hostility has been distinctly harmful to Bentham's intellectual reputation. It has led to acceptance of John Stuart Mill's version of Utilitarianism as a stronger defense than Bentham's, when in fact it subverts the whole argument by distinguishing higher from lower pleasures.

The second reason for neglect of Bentham stems from a kind of intellectual snobbery which concentrates on the pure fount of an intellectual tradition rather than on the expositor who imposed its present form. It is unfortunate for Bentham that the fount is in his case so great a philosopher as Hobbes. Certainly Bentham placed little value on production of an original philosophy, being content to borrow and adapt where he could (from Helvetius for example) to provide foundations for his main evaluation of law and other political institutions. In the process, however, he had to supply a complete statement of his fundamental assumptions, which because of his concern with practice had to be operational in form rather than literary. Hence his surprisingly modern concern with measurement, which makes the crucial transition from hedonistic philosophy to modern social science.

It must be admitted, third, that Bentham also suffers from his failure to systematize and discipline his own work. No other major thinker left so many alternative drafts of important books unpublished on his death. In spite of redactors like Bowring and (more able) Dumont, and sympathetic modern editors like Ogden and Stark, most remain unknown. An enlightened attempt to remedy the situation by publishing a properly edited and collected edition of all Bentham's writings is now being made by the Athlone Press of the University of London, under the general supervision of Professor J. H. Burns. The splendid volumes under review are part of this enterprise (taking the correspondence from the earliest letters

up to 1788). A new edition of "An Introduction to the Principles of Morals and Legislation" has also been published, with a manuscript work, "Of Laws in General," in which Bentham discusses related problems.

None of these volumes can be commended too highly for their careful assembly of a definitive text, illuminating yet restrained comment, and annotations which place each document in its proper place in Bentham's intellectual development. They are essential to any library that claims adequate coverage of political thought, and most economically priced for what they offer, arousing the highest expectations about the thirty-odd volumes still to come.

The correspondence itself, of course, cannot offer any startling new insights into Bentham's thinking. What it rather contributes are the detailed practical concerns which (more in Bentham's case than in most others) produced his theoretical work and set his intellectual agenda. For those whose image of Bentham is taken from the end of his life—the Hermit of Queen's Square—it will be something of a shock to find him a young intellectual about town, closely concerned with his brother Sam's advancement, though less so with his own. His brother's mechanical and business pursuits as naval architect and Russian administrator, provided a constant stimulus to Bentham's theoretical generalizations. The Panopticon itself—Bentham's grand scheme for a completely controlled prison environment (also applicable to schools!)—emerged from his brother's concept of an industrial "inspection house" on Prince Potemkin's estates. Thinking which foreshadows the Theory of Fictions occurs in the mathematical briefs prepared for his brother's education in the 1770s. The precise form taken by his early legal writings (and hence of his most readable general works, the Fragment and Introduction) of an adjustment of rewards and penalties through laws imposed from above, owes much to the Bentham's concrete plan of presenting a complete reasoned law code to Catharine II. Jeremy himself made a two and a half-year visit to Russia to join Sam from 1785 to 1788, and was not unfavorably impressed by what he saw there and by prospects for reform under a benevolent despot.

Relations with his father are also illuminated by the correspondence. This most radical and subversive thinker of modern times, scrutinizing every institution and practice by its potentiality to maximize human wants, was also a dutiful eighteenth-century son, abandoning his one love affair because of his father's disap-

proval. This episode did not undermine his filial sense of love and duty. On one point only (the most important from his point of view) did he resist his father: he must devote himself to reforming the law rather than practicing it as it stood. A letter of 14 October 1772, written at the age of 24, stands as a tactful declaration of independence on this point—accepted, albeit reluctantly, by Jeremiah. Psychoanalytic biographers might ponder the secure background of family relationships from which radical Utilitarianism emerged.

The correspondence as a whole aptly reflects the way in which, for Bentham, detailed practical problems were viewed as manifestations of general social processes, which required both empirical description and public moral control. Indeed Bentham did not distinguish the enterprises: society could both be interpreted as men's pursuit of want-satisfaction and manipulated so as to maximize it. A few years ago such an intimate link with moral concerns would have been dismissed for getting in the way of value-free political science. Now that natural science itself appears riddled with values, this viewpoint, and criticisms of Bentham based on it, appear passé. Empirical political scientists are increasingly asking how moral concerns can legitimately be introduced into their work. For this problem, Bentham, a congenial exemplar, may yet provide the basis of a solution.

IAN BUDGE

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**R. H. Tawney and His Times.** By Ross Terrill. (Cambridge, Mass.: Harvard University Press, 1973. Pp. x, 273. \$15.00.)

It ought to be a source of some discomfort to a good many British political theorists that the first full-length appraisal of the work and ideas of R. H. Tawney should have to emerge from Harvard rather than from Balliol or the London School of Economics. It ought to be a source of rather more discomfort that Ross Terrill is so much more sympathetic to Tawney, both intellectually and politically, than recent political theorists in Britain have been.

*R. H. Tawney and His Times* falls into three sections of roughly equal length, the first biographical, the second theoretical, and the third devoted to an assessment of Tawney's present importance. The biography is neatly and unobtrusively done; it is Tawney as he would have presented himself, with not much fuss about the details of his *vie intérieure*. Here, we see the educator, the driving force behind the Workers' Educational Association, the econom-

ic historian, the socialist gadfly on the Sankey Commission, the sage of the London School of Economics and recipient of innumerable doctorates. Dr. Terrill is perceptive and amusing about the odd phenomenon of upper-middle-class English socialism, with its total contempt for the bourgeois virtues of tidiness and thrift and its unconcern with material comfort, and he comments wryly on the faint air of disapproval with which Tawney watched the newly prosperous postwar working class accumulating its consumer durables; but he insists quite fiercely that although Tawney was marked by the extraordinary highmindedness of his Balliol background, he was free of the unconscious paternalism that all too often accompanied it.

The only complaint to be leveled against Terrill's picture of Tawney is that it makes it difficult to see quite why Tawney exerted the influence he did. Terrill, I suspect, likes Tawney much better when he is exciting W.E.A. classes in the Potteries than when he is a Reader at the L.S.E.; the trouble is that this makes the last forty years of Tawney's life something of an anticlimax, and one never quite knows if this is indeed how Terrill sees it.

Tawney's crucial contribution to socialism, so far as Dr. Terrill is concerned, was his picture of socialism as "fellowship." This is an elusive notion, and Dr. Terrill does well not to strain after too precise a blueprint; rather, he points out those things which are inimical to fellowship as Tawney conceived it, and whose abolition was what building socialism must amount to. Thus, gross inequalities of wealth or income cut men off from one another; functionless ownership, an ethic of profit rather than public purpose, tended to create such inequalities and was anyway divisive; but too much faith in the virtues of *groups* rather than *individuals* was equally inimical to fellowship, a fact which escaped the guild socialists but did not escape Tawney. A Christian emphasis on the infinite worth of every human creature lay behind Tawney's socialism, but not the Christian emphasis on hierarchy, for he believed that only egalitarian relationships could be relations of fellowship.

It is half a century since Tawney's best-known works were published; obviously, it is an open question whether they possess more than a historical curiosity value. Dr. Terrill makes the case against Tawney's relevance with some vigor; Tawney wrote as though there were no problem for Britain in pursuing a socialist path in lonely isolation, but fifty years after, Britain's dependence on the rest of the

capitalist world makes such an assumption absurd; again, the Christian context in which he placed his ideas has lost much of its relevance, and the other great creed of our time, Marxism, has ceased to be an option which can be contemplated as calmly as Tawney contemplated it. But these are reasons why Tawney has temporarily lost his audience; they are not reasons for rejecting Tawney's ideas. And Dr. Terrill ends by claiming that the humanism, idealism, and moderation in Tawney's socialism are precisely what keeps it alive. Moral ideals are taken seriously, because the rest of us take moral ideals seriously, and Tawney had no vision of leading ignorant dupes to a utopia of his own devising; equally, socialism was not to be expected as the gift of history but striven for by men who were united by their moral ideals; and they had to travel the democratic road to socialism, not because it was tactically the right thing to do, but because it was the socialist path. In short, what Dr. Terrill argues is that Tawney's socialism is not currently fashionable, but that, as so often, being unfashionable is very different from being worn out.

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**Psychological Bases of War.** By Heinrich Winnik, Rafael Moses, and Mortimer Ostow. (New York: Quadrangle Books Inc., 1973. Pp. 261. \$9.95.)

It would be surprising indeed if a conference made up of psychoanalysts, some American, some Israeli, called at this moment in history could solve the age-old problem of war. Indeed, according to the editors of these proceedings of a meeting of the Israel Psychoanalytic Society, they didn't expect to make more than a modest contribution to the prevention of war. Unfortunately, it is doubtful whether they have even achieved that aim, which is a pretty lofty one if you think about it. As contributor after contributor notes, psychoanalysis is concerned with individuals rather than groups, and psychoanalysts are peculiarly incapable of breaking away from the constraints of their own discipline.

Conference proceedings almost never seem to succeed in print. This particular volume is a specially sad one; it is almost like a collected confessional. Perhaps this is its main value: it throws some light on the effects which prolonged conflict has on the minds of liberal, earnest, introspective people unwillingly caught up in it. The contents list includes "The Subjectivity of Israeli Psychoanalysts in Discus-

sing War," "Vicissitudes of Aggression Viewed Developmentally," "Psychoanalytic Implications of Reactions of Soldiers to the Six-Day War," "Cultural Patterns of Aggression," "Some Aspects of Children's Aggressive Behavior During States of Illness and Recovery." Each of these topics (and others) is tackled by one speaker and then immediately opposed by "Another View." The conscious efforts of many contributors to be "objective" are very apparent, but some relapse into simple self-justification of the Israelis' position. In some articles the self-consciousness borders on the embarrassing: "analysts should be able to discuss any theme in an objective scientific manner and to put personal emotions aside."

Even making allowances for some of these understandable difficulties, this book does not contribute a great deal to the literature on warfare. Psychology is a much bigger topic than psychoanalysis, so even the title is misleading. It may, on occasions, be apt to generalize from individual examples, but, for my taste, psychoanalysis is much too prone to do this and it is a philosophical error when dealing with a matter as complex as warfare to generalize from one conflict situation. It is an error largely to omit discussion of the other disciplines which are pertinent to the study of the psychology of war—psychology, sociology, history, anthropology, ethology, biology, and so forth. Of course psychoanalysts gathered together should talk primarily about psychoanalysis, but not as though their subject existed in a vacuum. Very few references are cited by contributors, although most authors constantly remind us of the writings of Freud and his essay "Why War?" written in 1933. This preoccupation with the Great Master and with other theoretical psychoanalysts operates to the detriment of psychoanalysis and gives it a religious flavor.

I suspect that political scientists will be disappointed if they read this volume. Unless they are completely unaware of how psychoanalysts view aggressive behavior in individuals they will not find much new information. Furthermore the book has been produced with that remarkable lack of concern for the reader's problem which is the hallmark of conference proceedings. Only four of the 25 articles have a reference list: the remaining authors who quote other authorities presumably do not expect their work to be of serious academic interest. There is no index, so we cannot delve, or use the book for reference. My copy was bound upside down, but I hesitate to interpret the symbolism of this.

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**Shelter and Subsidies: Who Benefits from Federal Housing Policies?** By Henry J. Aaron. (Washington, D.C.: The Brookings Institution, 1972. Pp. 238. \$7.95.)

**The Politics of Land Use: Planning, Zoning, and the Private Developer.** By R. Robert Linowes and Don T. Allensworth. (New York: Praeger Publishers, 1973. Pp. 166. \$13.50.)

In reading *The Politics of Land Use* it becomes clear that Mr. Linowes and Prof. Allensworth condemn the resistance of too many communities to the winds of change, but fail to point out that planning commissions lack independence from the political system, often forcing the competent planning professional into pacifying a vociferous minority to the detriment of the public interest.

Land use controls, say Linowes and Allensworth, are almost the exclusive responsibility of local governments rather than of interjurisdictional bodies. They blame local rule for racial discrimination, economic segregation, and social class division, and they advocate an upward shift in some parts of the country for planning decisions as long as metropolitan planning does not go hand in hand with metropolitan government (p. 37). These two authors tell us that zoning is "rigged" (p. 64) because it serves only the protection of the single-family dwelling from "incompatible" uses (p. 65), is anti-development (p. 67), exclusionary (large-lot requirements effectively eliminate the poor and lower-income groups), and discourages diversity. The Euclidean principle looks perfectly "atrocious in practice" (p. 75). Zoning, finally, is prohibitive (excludes certain land uses).

According to Linowes and Allensworth, the use of land for single-family dwellings is by no means economically more advantageous to the community than industrial, commercial, and apartment uses are. Servicing a single-family home costs \$17,000, compared with \$6,000 to \$9,000 for certain multi-family developments (p. 79). They advocate cluster zoning, planned unit development zoning, new-town zoning, vertical zoning (providing for different uses within the same structure), and site approval. The suggested contract or conditional zoning has been rejected by other authorities because of its weak social accountability.

The federal government, over the years, has been attacked for its detrimental influence on high-cost construction and high-priced housing (p. 98). Public housing (p. 98) has conspicuously failed to produce relief for the neediest. In order to achieve this desirable goal,

in my opinion, we should stop locking in massive numbers of poor minorities in the least desirable locations without expert management, and we should emphasize instead superior quality, beauty, and social mix. If attractive buildings were located both in the suburbs and the inner city, the massive resistance of the single-family dweller could be broken. For this untenable situation the authors hold responsible primarily the narrow-minded leaders of the citizens associations and citizen groups who pretend to be progressive elements working for the advancement of the public interest (p. 38). Moreover, they place much blame on present zoning practices, which amount to no plans at all (p. 37), do not serve broad community needs (p. 44), serve class ends (p. 46), and do not adequately protect open space. The authors properly attack the monotony of cheap suburban housing, but do not discuss the necessity for a pleasant visual atmosphere as an important social burden on any viable community.

Among the innovative land-use controls capable of bringing about needed change, the authors recommend a Court of Ecology which would have the power to review all basic community-level planning and land-use control decisions. As attractive as this suggestion may appear, the practical difficulties would militate against this proposal.

In summary, the Linowes and Allensworth volume is a timely contribution to the continuing debate on a national land use policy, but we could have expected substantiation by case histories from the zoning attorney's extensive experience rather than the often repetitive and unnecessarily verbose statements.

On the other hand, Henry J. Aaron, senior fellow at the Brookings Institution in Washington, D.C., amply documents the presentation in his book and fills a gap in the abundant literature on the subject of housing. Although the data are in part several years old, the validity of his critique of our national housing policies for the last thirty-seven years would not be the least bit diminished by more recent statistics. In examining the rationale for a housing policy, Aaron questions the most conspicuous omission of specific support for housing in such income-maintenance programs as welfare agencies (which divide payments in allowances for housing, food, clothing, and other commodities) or all federal transfer payments (e.g. veterans' benefits and pensions) (p. 3).

The author tells us that the value and importance of shelter to the buyer is determined by a wide range of residential services (public schools, stores, parks, public transporta-

tion, neighbors, and other amenities) as well as access (distance from jobs, stores, recreation, and the quality and price of the transportation network), attributes of neighbors, and very significantly the probability of crime (p. 5).

The housing markets of all households are linked by a process called "filtering" (p. 7); in other words, changes in the supply of one kind of housing affect other housing markets as well. Aaron convincingly demonstrates the often harmful effects of the financial intermediaries who determine not only the architectural styles and the racial composition of neighborhoods but also the location of the residences. He holds that one of the most sinister factors in the deterioration of the inner city has been the practice of "red-lining" by lending institutions; this practice directly abets the migratory trend to the suburbs by declaring territory off limits and by denying loans and insurance regardless of the purchaser's income or credit rating. Government policies have failed to produce the badly needed "green-lining" (abandonment of the practice of "red-lining") of America. They have frequently "altered housing markets by creating or destroying neighborhood ties through establishing attendance boundaries for public schools or construction of highways or preservation of open space" (p. 9).

The present methods reflect a collective judgment that general welfare will rise if some groups have more goods and services even at the expense of others. Economists—and Aaron is an economics professor at the University of Maryland—have long held that "from the family's standpoint a cash transfer would leave them at least as well off as would subsidized housing, and usually better off" (p. 10). Subsidies for construction, borrowing, or other housing costs do not benefit principally families with inadequate incomes. Instead, they "directly assist groups whose incomes and housing are adequate by most standards" (p. 11). A strong believer in the marketplace as the arbiter of economic decisions, Aaron argues very persuasively against those who regard "housing as a special kind of commodity in that many families care far more about how others are housed than about how they are clothed, entertained, or otherwise provided for" (p. 11). The Administration proposals in September 1973 for direct cash assistance to homeowners conceivably could achieve our goal of a decent home for all Americans. The chances for such legislation appear to be very slim, at least at the present time.

Whereas Linowes and Allensworth put the blame for the present dilemmas on the Ameri-

can sacred cow, the single-family home, the Aaron book emphasizes the fractionization of our society. "No market mechanism exists to induce the coordinated action that would benefit all. Each person pursues the selfish, beneficial short-run strategy to the long-run detriment of all" (p. 13).

It seems to me that in the development of desirable neighborhoods, builders and developers are not only frustrated by obsolete building methods, union policies, credit practices of the lending institutions, and myriad government insurance and subsidy regulations but also by the vaguely defined term "public purpose" which alone permits condemnation proceedings and thereby acquisition of larger land areas needed for planned developments. Efforts to reduce costs and to make available shelter for the lower-income groups have been foiled by the myopic laws administered by a vast and frequently thoughtless bureaucracy. The responsibility for this situation must be shared, however, by contractors resisting industrialized housing, by unions obstructing labor-saving devices, by greedy land speculators aided by the capital gains tax provisions and allowable interest deductions (questioned by Aaron on p. 55), and by the credit policies of the government and lending institutions which dominate the housing services more than the price of most other capital and consumer goods.

Both Aaron's criticism and my own suggest the need for radically different housing policies and income distribution. Aaron pleads for many institutional changes, for amendments of the tax laws directly benefiting intermediaries but not achieving desirable social goals, and for a re-examination of our public housing goals and policies. In addition to the few concrete changes in the tax structure suggested by Aaron (e.g., net imputed rent—p. 71) he could have elaborated on the many possible tax incentives and disincentives which because of their complicated nature elude most observers. He should also have exposed the often conflicting policies of the Farmers Home Administration and the Federal Housing Administration—agencies that should have been combined long ago into one agency working for a balance between rural and urban environments as envisaged by the recent Rural Development Act.

Government officials, planners, and developers will find Aaron's scholarly work particularly helpful in spotlighting some of the less obvious results of government policies and the socially unacceptable practices of the private sector in our society.

JOSEPH L. STEVENS

*University of Virginia*

**Political Science and State and Local Government.** (Washington, D.C.: The American Political Science Association, 1973. Pp. 141. \$2.75, paper.)

This book resulted from a conference to review APSA programs in the field of state and local government that had been funded by the Ford Foundation, and to consider future research and teaching in the field. As in other conference volumes, the pieces are mixed in both focus and quality. Three summarize research in selected areas of state and local government; three others deal with teaching approaches in the field; and two shorter items offer summary comments on the other papers and some general recommendations of their own.

The mixture of intellectual focus and style that appears in the papers reflects some of the range in the state-local field as a whole. Lawrence J. R. Herson writes in a literary style about the various intellectual currents that have focused on cities over much of the 19th and 20th centuries. He finds ingredients of pragmatism, invention, radical rejection, and ideology on the historical map; muses about the continued lack of agreement about policy substance among those who write about cities; and closes with "a question that is both elementary and ultimate: Given society's present and future needs, how is the city to be defined?" (p. 22). Such a query seems to fit Herson's style of dealing with the range of literature that he fits into his essay. Yet even while it is tantalizing, the question seems more an impediment than an aid to understanding urban issues.

Charles O. Jones takes on the studies of state and local policy process and output with a systematic survey of six major journals over the 1963-1972 period, classifying and assessing policy-oriented research in 24 categories. This is the most original piece in the whole volume, and is likely to attract the most attention. Jones faces up to the complexity in the literature at issue, finds that it may have raised more problems than it has solved, but considers it useful even if it has only begun the task of illuminating the problems inherent in analyzing policy process and substance. He also considers three representative works of "professional" (i.e., normative) writing: by Dewey, Lowi, and Dror, and finds them similarly helpful in pointing to the problems inherent in advocacy. In conclusion, Jones alludes to "those days of old when political scientists could judge and advocate without being so damn precise" (p. 46), but he sees no simple route that can ignore complexities we have already found.

In other essays, Alan Rosenthal writes about

research focused on state legislatures; Earl M. Baker assesses the Intergovernmental Personnel Act of 1970 for political science education; Peter Boussein and Clara Penniman have separate pieces on public administration training programs; and Samuel C. Patterson and Ralph K. Huitt offer brief and insightful papers to summarize conference themes.

As these essays offer their descriptions, evaluations, and prescriptions for research and training programs in the state and local field, they show several of the strains apparent in this part of the discipline. There are sentiments for giving high priority to the training of government employees or to the kinds of substantive policy research desired by policy makers; and other sentiments for giving the highest priority to the theoretical understanding of how the policy process works. There is some pooh-poohing of behavioral research, and no little advocacy that other political scientists do research that seems important to the conferees. For the most part, however, the participants find some merit in various approaches. It seems to have been an agreeable conference.

The conference organizers may find their narrow queries about APSA programs answered by these papers, but the volume cannot claim to represent the full spectrum of current interest in "state and local government." The participants did not include any of the political scientists who have published prominent policy studies focusing on the urban arena. Such participants might have reminded certain conferees that systematic studies of policy issues already exist in the literature, and that others are underway. The conference might have given more direct consideration to some of the fascinating quarrels about the measurement of political culture and public opinion in state or local policy making; or about the measurement of benefits and costs distributed by state and local policies; or to the studies dealing with courts, prosecutors, and police. In what this volume lacks, as well as in what it contains, there is some insight into the need to reconsider "state and local government" as a field of political science.

IRA SHARKANSKY

*The University of Wisconsin, Madison*

**Reframing the Constitution: An Imperative for Modern America.** By Leland D. Baldwin. (Santa Barbara, Calif.: American Bibliographical Center—Clio Press, 1972. Pp. xiv, 145. \$15.00, cloth; \$5.50, paper.)

*Reframing the Constitution* combines arguments for remaking the American political structure with a full and detailed draft of a new constitution. The author, an emeritus pro-

fessor of history, provides little in his arguments which would startle political scientists, but his proposed constitution, despite many flaws, is an imaginative and instructive effort which could make sections of this book useful as teaching tools.

The first five chapters are largely historical and descriptive, providing few clues to Baldwin's structural prescriptions in Chapters 6-8. Chapters 3 and 4 do outline the proposed 14 regional states, but the consolidation is discussed largely in terms of sectional traditions. Parts of the historical argument rely heavily on the political science of James M. Burns, V. O. Key, and the 1950 APSA Committee on Political Parties. As a result, there is no consideration of current behavioral studies analyzing the impact of structural variables. For example, to what extent is the "antiquity" of state constitutions, as hypothesized on page 23, "one element" in their lack of political viability? Nor is the absence of behavioral data compensated for by any depth of philosophical analysis: democracy is tested "against a standard of flexibility and the degree to which it is open-ended" (p. 6) without systematic consideration of the concept of flexibility; the framers of 1787 are given only brief historical treatment; and superficial reformist phrases crop up. Thus it is "the complexities of the present era" which make inherited constitutional forms "inadequate for the solution of modern problems" (p. 18), and it is Congress which throws "obstacles in the way of effective administration" (p. 64). Baldwin approaches his work as an historian with a liberal faith in the value of institutional change and a populist distrust of wealth. He concludes that New York City "has pretty well dictated national policies, using Chicago and San Francisco as branch offices" (p. 56), and his criticism of John Marshall (p. 70) implicitly evokes V. L. Parrington.

It is the proposed constitution which provides for fascinating reading and speculation. (See pp. 82-100, 104-111, 118-142.) A unicameral Congress places the president within a parliamentary framework. A 100-member Senate reminiscent of the House of Lords assumes *inter alia* the investigative functions of the old Congress, and its 50 Law Senators are responsible for the judicial functions of the old Supreme Court. Parties are recognized and incorporated in the constitution. But what emerges is more of what already exists in our present system: the new president would be more powerful than any prime minister, because minority government would be constitutionally proscribed (pp. 87, 136); the Senate

would provide a stronger and more independent national check than any existing institution; and the new group of states, stripped of much existing authority, would by dint of smaller numbers and regional organization be better able to press their demands upon national authorities. Baldwin may have wrought so well that all elements in the existing system gain—and conflict and deadlock are escalated.

Regardless of the merits of the proposed document (must the metric system be enshrined in the Constitution?), Baldwin's goal is to direct attention to the need for constitutional change. A converging goal of political science is to teach and test hypotheses about the effects of political arrangements. Thus a course in constitutional law could take Baldwin's proposals and analyze their political impact, moving beyond case law and the behavior of judges to raise constitutional questions central to an understanding of political behavior. A seminar in American or comparative politics could also use material from this book to sensitize students to the need for linkages between constitutional structure and political behavior. In this way present-day political science may be able to address and help answer the central question posed by this book: would the effort to frame a new United States Constitution be worth the trouble?

CARL BAAR

*Brock University, Ontario*

**Race and Politics in New York City: Five Studies in Policy-Making.** Edited by Jewel Bellush and Stephen M. David. (New York: Praeger, 1971. Pp. 202. \$7.50.)

Case studies that are rich in description and written with a larger analytical purpose can nicely reveal the limitations of even the most widely accepted political theories. Such is the aim of *Race and Politics in New York City*. This volume of five case studies and an introductory essay challenges many of the assumptions and conclusions of the pluralist school of political analysis. Drawing upon controversies that occurred in New York City in the 1960s, the contributors question the pluralistic nature of New York by illustrating how the city's political system has remained largely resistant in the face of demands made by the then newly mobilized black community. The essays relate New York's experience with the Community Action Program, Lindsay's futile attempts to set up a civilian review board and provide scattered site housing, the bitter school decentralization battle, and the failure of neighborhood poor to establish community mental health centers.

Unlike many of the school's critics, it is not the pluralist methodology which the eight contributors to this book take to task; rather, they employ the pluralist's own case study technique to identify three major flaws in the pluralist design. First, Professors Bellush and David contend that political scientists in the 1950s who characterized New York as exemplary of an open and responsive, if troubled, political system were basing their ideas on overly optimistic assumptions. Even when group primary goals are clearly threatened, not all groups are able to mobilize and secure a public decision that reflects at least some accommodation to their demands. Lack of information, scanty political resources and skills, and the unwillingness of officials to bargain are forms of systematic discrimination against blacks. As the case of scattered site housing suggests, black citizens had a major interest in getting housing outside of the ghetto, but they were dwarfed by the activity and access of white neighborhood groups. Too often, the editors contend, pluralists have neglected to go beyond their own beliefs about what participants in a controversy should reasonably expect and examine the extent that the actors themselves are satisfied with the "resultant." From this perspective blacks appear to be regular losers.

Second, the pluralist model is not particularly useful for understanding race politics in the 1960s. Pluralist writers, particularly Herbert Kaufman and the late Wallace Sayre, utilized a decision-making model which emphasized the activities of the leaders of small organized groups and uncovered the alliance-building strategies which seemed to be the key to most political decisions. Yet, given the unsuccessful record of blacks to find effective allies during the conflicts elaborated in *Race and Politics*, the pluralist pattern is hardly a universal tendency. Instead, the mobilization of broad social classes, overt clashes of interest and changes in the expectations of black and white masses appear more crucial elements in the evolution of big city politics in the "post-pluralist" era.

Third, the pluralists are criticized for short-sightedness. Attempting to demonstrate that no single elite could dominate decision making by revealing different functional arenas of participation, pluralists made little effort to determine the distribution of influence within the islands of decision. As John Lindsay discovered during the civilian review controversy, reform is unlikely when the police bureaucracy and their allies have the resources to close the system to antagonists.

Although most of the criticisms are hardly

novel, their concise elaboration and the well-organized case study illustrations make this an interesting and informative book. It does, however, have some significant limitations. Given the highly ambitious scope of the work, the five cases could hardly back up all the hypotheses which the editors articulate. Nevertheless, the contributors could have attempted a more conscious testing of the volume's theoretical critique. As it is, each case tends to conform to a general pattern; the major participants and stakes are described, the strategies and activities of key actors are identified, and then the policy outcomes are evaluated in terms of the distribution of rewards. Although each study exhibits basically similar concerns, the theoretical significance of some of the essays is often quite indirect. For example, only one of the case studies (the civilian review board controversy) provides much data about changes in mass behavior even though the editors themselves argue that the pluralists concentrated excessively on the activities of elites. Ironically, the other cases focus largely on elite interest-group behavior in order to describe the conflict between the white and black communities.

Since this book concentrates on only the more visible racial controversies in New York, it cannot provide a testing of pluralist findings. Yet it does point to an important universe of political conflict which pluralism—and every other established theory—has neglected. On the whole, this is an excellent book for students of urban politics because it effectively combines well-written substantive material with good, solid theoretical criticism.

PAUL KANTOR

*Fordham University*

**Congressional Hearings on American Defense Policy, 1947–1971: An Annotated Bibliography.** Compiled by Richard Burt. Edited by Richard Burt and Geoffrey Kemp. (Lawrence: University of Kansas Press, 1974. Pp. xvi, 377. \$5.95.)

This listing of defense-related hearings, primarily those held by the Armed Services Committees and parallel Appropriations Subcommittees of each house, is a useful addition to the gradually improving bibliographic resources available to social scientists and policy analysts. Some hearings have been omitted, but the exclusions are generally peripheral to the volume's main focus.

A more serious lack is the result of the editors' choice not to include committee reports and related materials. For some, an indication of committee recommendations or posture as contained in the appropriate report would be

the most useful and needed information. To a lesser degree, some users will also lament the failure to include related documents and special studies.

The entries are organized chronologically by sessions of Congress, subdivided by committee and then by specific hearing. While this arrangement is probably the best possible, the usability of the bibliography would be markedly enhanced if it contained both subject and witness indexes. As it stands, most researchers would need to thumb through the entire contents to locate materials on particular facets of defense policy, or on the appearances of specific individuals as witnesses.

Each entry contains a listing of witnesses, and a brief *précis* of the subject of testimony given. Some annotations are restricted to topics discussed, while others give an indication of positions taken by witnesses. Obviously, more complete commentaries would be desirable, but undoubtedly were not feasible because of temporal and economic constraints.

The National Security Education Program of New York University, sponsors of this and several related volumes, is to be commended for its efforts to improve the quality of national security policy research. Researchers of whatever ideological or policy persuasion will find that the flaws of this straightforward volume are considerably outweighed by its merits.

WILLIAM I. BACCHUS

Washington, D.C.

**State Legislatures: An Evaluation of Their Effectiveness.** By The Citizens Conference on State Legislatures. (New York: Praeger Publishers, 1971. Pp. 498. \$21.50.)

This is the third volume produced by the Citizens Conference on State Legislatures dealing with the technical capabilities of the fifty state legislatures. It is the basic document on which the earlier studies—*Summary Report: An Evaluation of the Fifty State Legislatures* and *The Sometimes Governments*—were based. Part I of this volume is directed mainly to the interests of scholars of the state legislative process, since it describes in detail the research design and assumptional bases for the evaluation and ranking of the fifty legislatures. Part II, which carries specific recommendations for legislative improvement in each state, is likely to be of most interest to state legislators of a reformist bent and to civic organizations critical of the general institutional design of state legislatures and dedicated to their improvement.

The technical capability of the state legisla-

ture, this study holds, turns on the extent to which it is functional, accountable, informed, independent, and representative (FAIR). Hoary, for the most part standard, prescriptions for the improvement of legislatures constitute the evaluative standards of FAIR. For example, a legislature scoring high in functionality will possess, among other things, adequate time for the scheduling and management of legislative work, multi-purpose staff support for leaders and individual members, adequate chamber and committee room facilities, a membership of reasonable size with a moderate number of committee assignments per member, a variety of organizational and procedural devices for expediting legislative work, and a diffused and constrained leadership (the latter characteristic presumably serving to maximize the opportunities for initiative on the part of individual members).

A legislature notable for its independence will be able to control the frequency, duration, and agendas of legislative sessions, have a strong measure of independence from the executive branch (particularly in terms of its access to information and analysis), possess the capability for program review, governmental oversight, and program and expenditure audits, and function under statutes and rules that provide for comprehensive regulation and registration of lobbyists and that minimize the possibilities of conflict of interest and dilutions of legislative interest.

The overall ranking of the states is based on an aggregation of their scores on each of the FAIR schedules. The California legislature ranks first, followed by New York, Illinois, Florida, and Wisconsin. The legislature ranking lowest in technical capability is that of Alabama, though Wyoming, Delaware, North Carolina, and Arkansas are not notably better. Six of the ten states at the bottom of the list are southern. Some legislatures that rank rather high on one dimension (e.g., independence) rank comparatively low on another (e.g., representativeness). Nevertheless, the pattern is clear: states achieving a high ranking overall are typically high on all counts, while states ranking low overall typically are low on all counts. California, for example, places no worse than third on any schedule, while Alabama is no better than 41st on any schedule.

It should be noted that each component in the FAIR ranking system was valued equally, though variable weights were assigned the subcriteria within each component. For example, the most heavily weighted subcriterion in the functional schedule was committee structure, while the most heavily weighted subcriteria in

the accountability schedule were those concerned with explicit rules and procedures and records of voting and deliberations.

Political scientists whose interests lie in the comparative study of state policy will find the chapter on the correlates of legislative capability especially instructive. In general, those states with impressive rankings on the FAIR scales tend to be characterized by affluence, a high level of urbanization, and a well-educated and politically active citizenry. The political systems of these states tend to be distinguished by strong governors, professional legislatures, competitive and cohesive political parties, and strong local governments.

So much for the focus and findings of the study. What else may be said of it? In the first place, this study has produced the most comprehensive body of information available anywhere on all fifty legislatures. Additional data on each legislature are available in data banks established at the Citizens Conference and at the Institute of Governmental Studies of the University of California at Berkeley.

Second, the study ought to be evaluated on its own terms. It is, pure and simple, a comparative study of legislative organization, structure, and procedures. Except tangentially, it is not concerned with legislative leadership, the distribution of power in the legislature, legislative party organizations, interest group involvement in legislative decision making, or executive-legislative relations. One may well have doubts whether a geared-up, professional legislature necessarily better attends to public problems, better serves the interests of the citizenry, or passes "better" public policy. The quality of legislators, of state party organizations, of legislative leadership, and of state politics at large may be more critical to overall legislative performance and outcomes than a pastiche of reforms involving staff, chamber size, informational systems, interim committees, or anti-limbo provisions, to mention but a few. But to bootleg in this point is to be less than fair to FAIR. All that the CCSL staff is arguing—implicitly some of the time, explicitly most of the time, unpretentiously all the time—is that certain structural and procedural arrangements *increase the probability* that legislatures can deal effectively and responsibly with state problems. Moreover, the authors of the report doubtless would agree that obviating structural and procedural defects in the institution is only one way out of several of rescuing disabled legislatures from their malaise.

WILLIAM J. KEEFE

*University of Pittsburgh*

**The Planning—Programming—Budgeting System in Three Federal Agencies.** By Joon Chien Doh. (New York: Praeger Publishers, 1971. Pp. xx, 192. \$13.50.)

An account of the Planning—Programming—Budgeting (PPB) experiences of federal agencies is much needed. For almost a decade, writers on all sides of the issue have forcefully debated the purpose and implications of PPB, and their arguments have been driven more by passion than by evidence. The controversy between those who see PPB as a means of bringing more rationality to public choice and those who see it as violating cherished political values has been quieted by the demise of PPB in the early 1970s. But PPB was possibly the most significant administrative innovation of the 1960s, and the issues associated with it merit empirical investigation.

Unfortunately, Dr. Doh's study of three federal agencies (Agriculture, HEW, and NASA) does little to fill the void. Fully half of this brief book is a rehash of the old arguments, along with prescriptions of what PPB is supposed to do. The other half offers the inadequate yield of some 100 interviews with program and fiscal officers in the three agencies and the Bureau of the Budget. On the basis of the impressions of these people, Doh rates the extent to which the PPB materials have been used as well as the quality of the documents. These pseudodata are paraded in place of the real thing, as support for a dozen or more firm statements about whether PPB has progressed more in one or another of the agencies.

The flimsiness of the method is most apparent in Chapter 6, "The Quality of PPBS Documents," in which the author employs three separate tables rating with statistical exactitude the NASA, Agriculture, and HEW submissions to the Bureau of the Budget. At first glance, the evidence appears to be impressive, but the reader has to retreat some 80 pages to recall that only ten Budget Bureau officials were interviewed. In all likelihood, some of these officials never read a PPB document, and not more than one or two read (and was in a position to compare) the documents of all three agencies.

Doh devotes an entire chapter to PPB experiences in twelve bureaus within the three specimen agencies. The chapter is barely five pages long and does not contain a single scrap of evidence, but this does not deter the author from passing judgment on the PPB results in each of the agencies.

An examination of PPB implementation is no easy task, and the author is to be commended for trying. There are no reliable "before" and "after" comparisons, no sure method of testing the penetration of budgeting by the new procedures, no accepted way of distinguishing between the forms of PPB and its spirit. For these reasons, there have been few attempts to gauge the impact of PPB on resource allocations and policy decisions. The approach selected by Doh utilizes four "process" criteria: (1) the relationship of planning, analysis, and budgeting; (2) the use of analytic studies in budget making; (3) the quality of the PPB documents; and (4) an evaluation of agency capability. There is nothing inherently wrong with these criteria, but they require a great deal of observation and detailed scrutiny of an agency's decisional structures. Impressions filtered through catch-as-catch-can interviews are no substitute for a careful inspection of administrative decisions.

One finding that emerges from Doh's work suggests limitations in both his method and the concept of PPB. Doh ranks NASA last in terms of PPB implementation, yet that agency was most advanced in the application of analytic techniques to its decisional processes. This suggests that Doh focused excessively on the forms rather than the purpose of PPB, and that PPB itself concentrated excessively on conjoining analysis and budgeting. As I have argued in *Budget Innovation in the States*, analysis might have greater opportunity to flourish when it is freed from the constraining routines and traditions of the budgetary process.

Doh's book was published in 1971, at a time when the future of PPB was uncertain. In search for some clues about the Nixon administration's position, Doh read unwarranted optimism into such passing events as the appointment of a particular individual to the White House staff, a few words in the annual budget message, and the establishment of a national goals staff. PPB was officially terminated in June 1971. It had lived the unexamined life and was found wanting.

ALLEN SCHICK

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**Opening Up the Suburbs: An Urban Strategy for America.** By Anthony Downs. (New Haven: Yale University Press, 1973. Pp. 185 and appendix. \$7.95.)

Anthony Downs's plan for *Opening Up the Suburbs* is almost certain to spark a major controversy about housing policies on the periphery of American cities. Writing in the style of an essayist, Professor Downs outlines the

major arguments for introducing low- and moderate-income households into predominantly middle- and upper-middle-class suburbs. Students who expect to find an objective analysis of housing conditions in metropolitan areas are likely to be disappointed; in this volume, Downs has assumed the posture of an advocate for a specific strategy to attack residential segregation, which lies at the core of many metropolitan problems.

Downs's proposal forms the central focus of the book. It is an important proposal, because it may very well assume a high position on the agenda of conventional liberal solutions to urban ills. Basically, his plan calls for "dispersed economic integration," or the promotion of incentives and requirements for mixing low- and moderate-income households in suburban neighborhoods. His ideas implicitly seem to reflect a growing realization that the simple creation of "equality of opportunity," e.g., by providing some low-income households with increased resources to purchase better housing, is an inadequate strategy; it may be increasingly necessary to resort to quotas, allotments, and subsidies to overcome the persistent and endemic effects of poverty and racism.

Downs scores some excellent points in demonstrating how both the "trickle-down" process and municipal codes governing construction and zoning have worked to the benefit of upper-income groups and to the disadvantage of low-status segments of the population. Moreover, his plan for "opening up the suburbs," though vague in some details, generally seems to be a feasible and workable strategy for achieving his goals. But he leaves one crucial question unanswered: How will this proposal affect the efforts of low-income groups to gain increased political power and influence? In Chapters 7 and 15, he examines possible opposition to his plan; but he is primarily concerned with criticisms which might emanate from middle- and upper-class suburbanites rather than with objections from persons at the bottom of the socioeconomic ladder. Concerning the number of low- and moderate-income households which might be allowed to enter the suburbs, Downs states:

It must be absolutely large enough to give the incoming households a sense of identity and solidarity, to make their votes important to local politicians, and to be noticeable to local middle and upper-income residents. But it must also be a small enough percentage so that middle-class behavior patterns remain dominant . . . (p. 123).

Since his proposal implies that low- or moderate-income households will compose consid-



erably less than a majority of suburban communities and that buildings designed for such groups will form only 10 to 20 per cent of new construction, it is unlikely that his plan would enable low-income residents to become either a cohesive political nucleus or a serious threat to prevailing middle-class values in American suburbs.

Fundamentally, therefore, Downs's plan to improve the plight of low-income groups, as well as similar proposals by other liberal commentators, must rely upon the benevolence of middle- and upper-class citizens. In asserting the need to maintain "middle-class dominance" of suburban neighborhoods, Downs is simply more candid about his own values than other writers have been. His perspective eventually leads him to conclude, in a final analysis of the political prospects of his proposal, that its chances hinge upon the altruism of policy makers and that it is likely to fail to gain adoption. Because of its critical dependence on the benevolence of high-status groups, similar pessimistic forecasts could be made regarding the ultimate success of his plan, even if it were adopted.

In describing the discriminatory effects of the operation of the existing housing market and in raising vital issues about institutional means of achieving social and economic integration in the suburbs, Downs has performed a commendable service. But his proposal fails to resolve the crucial political problem of enabling low-income groups to become a viable and potent political force in metropolitan areas. This problem increasingly may become both a major source of contention among political scientists and a critical factor in determining the fate of cities. *Opening up the Suburbs* clearly has not closed the debate concerning some of the most pressing questions of American politics.

HARLAN HAHN

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**The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Vol. I, Antecedents and Beginnings to 1801.** By Julius Goebel, Jr. (New York: Macmillan Co., 1971. Pp. 865. \$30.00.)

Oliver Wendell Holmes, Jr. was in many ways a very remarkable man. One of the truly great Justices of the U.S. Supreme Court for thirty years, he died in 1935 at the age of 93. Quite characteristically, the Justice left his residual estate, amounting to about \$263,000, to the federal government. It seems equally characteristic of the legislative process that Congress took twenty years to make up its mind as to what to do with the money. At long last,

in 1955, Congress adopted an act creating the Oliver Wendell Holmes Devise Fund, which included the original bequest plus a single appropriation in lieu of interest. The statute set up a permanent committee consisting of four persons appointed by the President for eight-year terms, with the Librarian of Congress serving *ex officio* as chairman. The enabling act provided that the main project to be financed by the bequest was to be a history of the Supreme Court. Paul A. Freund, Carl M. Loeb University Professor at Harvard, was selected as editor-in-chief, and plans were made for a history of the Court to take the form of eleven hefty volumes.

The late Julius Goebel, Jr., George Welwood Murray Professor Emeritus of Legal History at the Columbia University School of Law, wrote the first volume, tracing the "antecedents and beginnings" of the Court down to 1801. At the same time, Volume VI of this series was published, this being Part One of a two-part survey of the period of reconstruction and reunion, 1864-88, written by Professor Charles Fairman. Some idea of the scope of this history is reflected in the fact that Goebel does not get around to mentioning the names of the first Justices appointed to the Court by President Washington until p. 552 of his volume. Fairman's Part One of his history of the reconstruction period runs to 1540 pages. Volume II (by George L. Haskins) will cover only the first 14 years of the Marshall Court; Volume III (by Gerald Gunther) will deal with the last ten years of the Marshall Court. Volume V (by the late Carl B. Swisher), which has just appeared, analyzes the Taney Court. Volume VIII (by Philip C. Neal and Owen M. Fiss) will cover the period 1888-1910. The late Alexander Bickel was scheduled to have written the next two volumes, concerned with the periods 1910-1921 and 1921-1930, respectively. Volume XI, by Professor Freund, will review the period 1930-1941. It is understandable that this history should end with 1941, but a great deal has happened since then, and one may hope that some money will be left over from the Holmes Fund, or found in some other place, to bring the narrative down to a much later date, at least to the end of the Warren Court.

Goebel was a very erudite scholar with an astonishing grasp of the history of Anglo-American law. The book is very heavily annotated, and however weary the reader may get, he should not skip over the annotations, since they are full of valuable bibliography and historical lore. For example, in one place, referring in the text to a minor event in early

American history, Goebel not only points out, in the footnote, that there are three books on the subject, but also tells you which is the best. In addition, this is a very handsome book, elegantly printed and bound, and replete with many pictures and facsimiles of various historic documents or legal papers. But since Goebel overwhelms with detail, the reader is well advised to take him a little bit at a time.

The book consists of seventeen chapters, together with a statistical analysis of the business of the Supreme Court from 1789 to 1801, and substantial notes on manuscript sources. Professor Goebel concludes with a "select bibliography" running to 16 pages. The first chapter deals with the transition of the law from the old world to the new. The second reviews the traditions of judicial control over legislation. In this connection it is worth noting that Goebel does not even bother to refute the well-worn argument that judicial review represented a usurpation of power by the courts. The third chapter deals with the principle of constitutional authority in the new states, while the fourth is concerned with the national judicial authority during the period of the Continental Congress. Chapter V deals with the Constitutional Convention of 1787, with special emphasis upon its treatment of the judiciary. The next four chapters recount in great detail the struggle over the ratification of the Constitution, but much of this section of the book is concerned with the judiciary only tangentially. Chapter X recounts the story of the framing and adopting of the Bill of Rights, Chapter XI reviews the Judiciary Act of 1789, and the next chapter analyzes in detail the Process Acts which followed, and which most histories ignore altogether. Chapters XIII and XIV deal with the Circuit Courts, and the last three chapters review appellate practice, and the political and constitutional issues with which the Court was concerned from 1789 to 1801.

By the end of the August, 1800, Term of Court, with which this volume ends, the Supreme Court had disposed of 79 cases which had been appealed either from the Circuit Courts (72 cases) or the state courts (7 cases). One of the main points made by this book is that much happened during the pre-Marshall period which mattered a great deal. During its first eleven years, Goebel concludes, the Court displayed both professional acumen and moral courage. In the admiralty cases, for example, he argues that the Court went about its business without being deterred by the pressure of foreign powers, or by "factious American opinion," or by political sentiment. The Court dealt with several important constitu-

tional law issues during this brief early period such as the suability of states in federal courts the validity of advisory opinions, and the scope of the federal taxing power (in the carriage tax case). But even more important, it was during these early years that the Court made a lasting contribution in working out basic problems dealing with the regulation of appellate practice.

This volume is replete with fresh insights into aspects of early American legal and constitutional history, but one example will have to suffice for purposes of this review. On February 21, 1787, the Articles of Confederation Congress adopted a resolution advising the states to send delegates to a convention "for the sole and express purpose of revising the Articles of Confederation," and reporting back to Congress and the state legislatures. Many historians have referred to this resolution as a congressional "call for a convention," but Goebel points out very convincingly that this is incorrect. In fact, all but five states had already decided to appoint delegates to a constitutional convention before the resolve of Congress was put into circulation. He makes the interesting point, therefore, that the source of the delegates' authority lay in the enabling legislation of the states, and not in the resolve of the Congress. Furthermore, the restrictive words, "for the sole and express purpose of revising the Articles of Confederation," appeared in the enabling legislation of only three states, Connecticut, Massachusetts and New York. Clearly, he argues, Congress was not delegating authority, because it had in this particular none to delegate. In fact, most state enabling statutes were cast in general terms and did not contain specific instructions. It follows that far from violating its "instructions," "the Philadelphia Convention was assured a greater freedom to act in the national interest than any assemblage yet convened" (p. 202). Authors of American government textbooks, please take note.

Space permits only one more example of this impressive work, to illustrate the tremendous effort that went into the research, and the author's grasp of detail. Goebel makes the point that the most onerous task of the early Justices was to ride the circuits as Circuit Judges, and in this connection he notes that President Washington would have been well advised to consider the health and vigor of each candidate he considered for a place on the Court. Then in a long footnote he summarizes what is known, or what he could discover, concerning the health of the Justices who sat on the Court during its first eleven years.

It turns out that Jay suffered from rheumatism; Cushing had a growth on his lip; Blair in his own words was afflicted with "a rattling distracting noise in my head," which Goebel guesses was an inner ear malady that led to his resignation; Rutledge suffered from gout; Iredell missed the February 1794 term of Court because of illness; Patterson wrote in the spring of that year that he "nearly went out of my head"; both Chase and Ellsworth were afflicted with gout and renal stones; and James Wilson, as is well known, suffered a complete nervous and physical breakdown in 1797. Unfortunately, Goebel could find nothing about the aches and pains of Bushrod Washington and Alfred Moore, though he carefully notes that they were Adams appointees.

This is obviously a distinguished contribution to American legal history. Himself a fine scholar, Holmes must be nodding his approval somewhere up there, where all great Justices finally go.

DAVID FELLMAN

*University of Wisconsin*

**Urban-Rural Conflict: The Politics of Change.**

By Harlan Hahn. (Beverly Hills, Calif.: Sage Publications, 1971. Pp. 276. \$10.00.)

Professor Hahn argues that while "conflict between the residents of urban-rural areas long has been a prominent feature of American politics" (p. 11), "published studies of American state politics have tended to neglect the role of urban-rural cleavage" (p. 14). And he views the increasingly substantial literature on party competitiveness in state politics as slighting the important effect urban-rural differences have on party competition.

The bulk of this volume consists of a political history of "the Republican tradition" in Iowa, from the founding of the Republican party in the State in 1856 through the 1964 election. As the symbol of Northern unity and loyalty during the Civil War, the Republican party became the only acceptable vehicle for political expression for the following seven decades. During this period, the party developed a remarkable capacity to absorb the rival demands of rural and urban residents within its own organizational framework, discouraging third parties or the revival of the Democratic party, dominant in the early days of statehood.

The factional strains between rural and urban interests in the Republican party increased in the 1920s, and the Roosevelt landslide of 1932 offered the Democratic party as a new alternative in Iowa politics. But, in 1938, Iowa became a one-party Republican state for

another twenty years. However, in the mid-1950s, when the Republican coalition of small-town residents and urban business interests refused to budge on the issues of legislative reapportionment and the distribution of road-use taxes, urban voters increasingly began to see the Democratic party as a more appealing option than Republican stand-pattism.

Hahn analyzes the Democratic percentage of the two-party vote in the Iowa elections of 1948 through 1964 and concludes that "small towns have evidenced relatively steadfast Republican leanings, large cities have reflected a growing Democratic trend, and the farm vote has demonstrated a volatile political independence that could swing an otherwise evenly divided electorate toward either party" (p. 99). With increasing urbanization in the State, Hahn sees "this politics of geography" as presaging a significant political realignment in Iowa. The five elections since Hahn concluded his study in 1964 appear to support his prediction. Today, both U.S. senators are Democrats, as are five of the six U.S. congressmen, and the Democratic Party controls both houses of the State legislature. Moreover, in the 1974 election, the Democrats captured one or more county courthouse offices in 75 of the 99 counties—the largest inroad into the traditional small-town Republican strength in Iowa's history.

This "size-of-place" theory of state politics is reminiscent of Epstein's seminal study in Wisconsin in 1958 and of Adamany's reconfirmation of the thesis in the same state in 1964. The 1958 study by Masters and Wright in Michigan, however, explicitly rejected the size-of-place analysis. Pending further studies, it can only be asserted that the Wisconsin and Iowa findings may be typical of some traditional Republican states in transition from small-town society and agricultural economy to increasingly urban and industrial dominance.

In the final chapter, the author reviews published political studies of the other 49 states, employing the urban-rural perspective of interpreting Iowa politics, hoping that "modest but significant progress can be made in formulating a comparative model of general applicability" (p. 220). From this comparative examination, Hahn concludes that "the specific features of Iowa politics have seemed to provide an adequate benchmark for the analysis of state political processes." It is evident that this analysis and its central conclusion are severely strained, when it is noted that twelve of the quoted studies were published in the 1940s and eighteen in the 1950s. Particularly disturbing is the repeated description of "contemporary" realities of state politics, expressed

in the present tense, based on Key's comparative studies of 1949 and 1956. This is hardly the way to build a model for "the politics of change."

C. EDWIN GILMOUR

Grinnell College

**Environmental Issues: Population, Pollution, and Economics.** By L. G. Hines. (New York: W. W. Norton & Co., 1973. Pp. ix, 339. \$9.75, cloth; \$3.25, paper.); **Small is Beautiful: Economics as if People Mattered.** By E. F. Schumacher. (New York: Harper & Row, 1973. Pp. 290. \$3.75, paper.)

Environmental deterioration is related to three kinds of events: *depletion*, *pollution*, and *ecological disruption*. Phenomena of nature (destructive winds, floods, droughts, and much else besides) contribute to environmental damage; but severely degenerative changes in our era are mainly attributable to human activities. Prevailing economic practices progressively *deplete* the "capital" latent in nature: e.g., fertile land, stored water, fossil fuels, other minerals, etc. Accelerative depletion of this capital contributes to *pollutions* of land, water, and air. Depletion and pollution *combine* to disrupt the systemic processes that sustain life, including human life.

These phenomena are all accelerative, doubling in rate and magnitude in shortening periods. It is generally conceded that many of the resultant impacts on individuals and communities are dysfunctional and that some are severely injurious. But experts disagree whether environmental depletion, pollution, and disruption pose serious threat to the human condition and prospect. Controversy also focuses on whether scientists and engineers can circumvent the "limits of the earth" and thereby enable humankind to dominate nature completely and with impunity. From here the debate fans out in various directions: among others, arguments regarding probable costs of maintaining a decently livable habitat; impacts of these costs on distribution of income and wealth and on other values; environmental responsibilities of public authorities and of entrepreneurs and other private persons and organizations; criteria for deciding what and how much environmental repair and protection should be undertaken; determining priorities among environmental, military, social and other claims on insufficient disposable resources; how to deal with depletion, pollution and ecological disruption of the oceans and other problems that transcend the unilateral capabilities of any and all governments; indeed, how to make any headway at all beyond one's own frontiers in

our politically fragmented, tribalistic world in which national polities differ enormously in natural-resource endowment, stage of technological-economic development, ratio of population to accessible resources, and other salient respects.

Even the partial listing above indicates how deeply public authority is involved: in defining objectives, setting standards, gaining compliance, allocating resources, mobilizing public support, and much else in addition. A few political scientists have addressed themselves to the problems of environmental protection and management; and more are turning their attention to these concerns. But the politics of environmental management is still left largely to natural scientists, engineers, and economists. With a few notable exceptions, they have treated environmental politics simplistically—or bypassed political options and obstacles altogether.

The authors of the books reviewed here are both economists. It is noteworthy that Hines exhibits the characteristic bias of marketplace economics against governmental intervention in production and distribution. It is still more noteworthy that Schumacher scarcely touches the central issue of how to achieve the radical reforms which he deems imperative. Nevertheless, both books should be interesting and useful for political scientists. For example, Hines gives a comprehensive exposition of the expanding role of public authority in strategies for reduction and control of water pollution (chap. 10), air pollution (chap. 11), and disposal of solid wastes (chap. 12). He also includes a detailed case study of the interactions of public authorities and private parties, and of the uses and misuses of cost-benefit assessment, in the development of hydroelectric projects in the Snake River, Idaho (chap. 7). Schumacher demands changes in attitudes and practices that would almost certainly entail major expansion of governmental control over individuals and corporations. In short, both books pose problems for students of government who concern themselves with the substance of environmental politics.

The books reflect a core of common concerns, as well as contrasting perspectives, presuppositions, and prescriptions. Hines is primarily interested in abating pollutions, but does not ignore the salience of depletion. Schumacher gives more attention to depletion and ecological disruption, without ignoring the importance of pollutions. Hines speaks from a perspective close to, though mildly critical of, the party line of hard-core marketplace orthodoxy. Schumacher rejects most of the core

doctrines of received marketplace theory, "calling its every assumption into question, right down to its psychological and metaphysical foundations" (p. 3).

Hines's stated objective is "to appraise environmental pressures in the United States, to determine the role of the government and of the market economy in environmental deterioration and its abatement, and to examine the range of corrective actions that are available . . ." (p. vii). He reviews the historical roots of today's environmental crisis, assessing the contributions of industrialism and of population growth. He deploys the tools that marketplace economics offers for arresting further environmental deterioration. He neither rejects nor defends very strongly the thesis that continuing economic growth is a necessary condition of environmental repair and protection. But he does summarily dismiss the alternative prescription of a steady-state economy, calling its advocates "well meaning but heroically naïve cranks" (p. 77)—which many of them manifestly are not! The book concludes with a sketch of worldwide issues, and a quick look toward the future, predicting that "the year 2000 will usher in an era with many more people, with the world still divided between rich and poor, with industrial output rising, but the natural environment more barren save for the abundance of man and his many works" (p. 330). One wishes the author had examined a little more closely whether all these conditions can occur simultaneously upon our increasingly crowded, depleted, polluted, and ecologically disrupted planet.

To this question Schumacher answers emphatically *no*. Production and acquisition of material wealth, he says, have become the "highest goals . . ." (p. 277). These reflect a philosophical posture (sanctified in both Marxist and marketplace theory) that recognizes no concept of "enough," only "more" (p. 23). Relentless pursuit of "more," he asserts, is acceleratively ruining our habitat, eroding communities, uprooting and disorienting people, and propelling humankind along a course that is injurious and self-defeating, to say the least (p. 31).

No business, Schumacher contends, could squander its capital and survive. Yet this is precisely what individuals, corporations, and governments are doing with "capital" extractable from the earth (p. 13). He emphasizes in particular the accelerative consumption of fossil fuels. These "are not made by men; they cannot be recycled. Once they are gone, they are gone forever" (p. 15). To seek escape via nuclear-fission energy is "to 'solve' the fuel

problem" by creating an "environmental and ecological problem of . . . monstrous magnitude" (p. 18).

In the larger context of the world community, Schumacher finds the energy policies and practices of the "rich" populations especially injurious to the "poor." The former are "stripping the world of its once-for-all endowment of relatively cheap and simple fuels," with devastating impact on the prospect for relieving hunger and destitution in most of the Third World (p. 25).

There is a great deal more in this hard-hitting book. One must read it to experience the thrust of Schumacher's indictment of "growthmania" and the economic structures through which this ethic is given expression.

These books are prime exhibits of the intellectual chasm that separates scholars of unimpeachable competence and genuinely shared concerns whose philosophical outlook and experience reveal to them widely variant images of the present and expectations of the future. No one today possesses the knowledge and clairvoyance to assert dogmatically which, if either, picture the future will confirm. My own hunch is that Schumacher's theses and speculations will be closer to what happens than Hines's will. I am, however, not an economist, only an ecologically oriented student of politics.

HAROLD SPROUT

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**The Law of Treason in the United States: Collected Essays.** By James Willard Hurst. (Westport, Conn.: Greenwood Publishing Corporation, 1971. Pp. 291. \$12.00.)

The literature on treason against the United States is extremely sparse. Therefore, this book by Professor James Willard Hurst is welcomed, both because of its able scholarship and because it is virtually one of a kind. While this work is, for the most part, a collection of previously published articles and essays, it is definitely not a scissors-and-paste job. Professor Hurst became involved with his subject during World War II when the Navy Department loaned him to the Solicitor General's Office in order to prepare background material for the first treason case heard by the Supreme Court, *Cramer v. United States*, 320 U.S. 730 (1943). From that research came a series of articles, organized on a chronological and developmental theme, that first appeared in law journals during 1944 and 1945. These constitute the first five chapters in the collected work. The final and more recently completed chapter, "Treason Cases and Doctrine, 1945–1970," is

an up-date of the research and an effort to draw some generalizations. Since the various parts stem from a common core of research and from a sustained interest of the author, this book has coherence that is not always found in collected works.

Treason, like impeachment, has both legal and political dimensions that make it an appropriate subject of inquiry by the two most relevant disciplines—law and political science. Combining historical and analytical approaches, Professor Hurst deals with a variety of subtle and complex legal questions growing out of treatises, constitutional provisions, and cases relating to treason. But in a larger sense, his work is about politics for it explores the permissible boundaries of political conflict and competition, and the reciprocal obligations of individuals and states. The latter characteristics make the book relevant to political science.

One of the essential themes in Professor Hurst's work is an elaboration of the restrictive policy of the Founding Fathers embodied in Article III, Section 3, of the Constitution. This provision narrowly defines both the elements of the crime of treason and the quantum of proof necessary for conviction. The clear intent was to limit Congress to providing a penalty for the offense and to deny the judiciary the discretionary power to create "constructive treason"—that dread practice from English tradition. But why was treason singled out for such special treatment? It has been commonly suggested that the Founding Fathers were sensitized to the dangers of abuse by their recent vulnerability to charges of treason had their efforts at revolution failed; however, the painstaking documentary search of Professor Hurst points to a higher order of concern and perhaps a better explanation for the restrictive policy. There is evidence that James Wilson (who had witnessed excesses attending earlier trials for treason in Pennsylvania), Benjamin Franklin, George Mason, and other influentials at the 1787 Convention in Philadelphia viewed the limitations on treason as the cornerstone in a constitutional plan to protect political discourse and legitimate competition among domestic political groups. The objective was to strengthen the integrative forces of national politics by reducing the corrosive effect that the abuse of treason power would have. To these men, then, the constitutional policy on treason contained an unarticulated premise which was later made more explicit in the words of the First Amendment.

If these were the expectations of the Framers, then history must record that their high hopes were only partially realized. True, the

narrow definition of treason still stands, and as a result less than forty prosecutions have been initiated by federal authorities. It might also be said that since *United States v. Aaron Burr*, 1807, and excepting two famous state prosecutions (Thomas Dorr by Rhode Island and John Brown by Virginia), no treason trial has created widespread alarm nor posed any threat to ordinary political processes.

On the other side of the ledger, political decision makers quickly learned how to circumvent the restrictive treason policy by the enactment of sedition and espionage legislation. The Constitution was less than ten years old when Congress passed the Alien and Sedition Act of 1798; thus, despite the treason clause and the First Amendment, the young nation was subjected to the divisive forces that the Framers had hoped to avoid. Nor have subsequent generations been spared the tribulations of alleged subversion and sedition. And it may be noted that Julius and Ethel Rosenberg were not and probably could not have been charged with treason, but they were convicted and executed for a violation of the Federal Espionage Act.

Since there has been no great volume of cases nor "hue and cry" on treason, why be concerned with this subject in the 1970s? Perhaps it is a part of our getting back to basics. Whether intended or not, it fits the current mood to reconsider the fundamentals of our political system, and the policy on treason is indeed fundamental. This book is not likely to make the best-seller list, but, then, who could have predicted the sales volume of Raoul Berger's *Impeachment: The Constitutional Problem* at the time of its publication? For those who wish or need to learn about the legal and political nature of treason in the United States, Professor Hurst has provided an excellent source.

MARVIN R. SUMMERS

*University of Wisconsin, Milwaukee*

**Compliance and the Law: A Multi-Disciplinary Approach.** Edited by Samuel Krislov, Keith O. Boyum, Jerry N. Clark, Roger C. Schaef-er, and Susan O. White. (Beverly Hills, Calif.: SAGE Publications, 1972. Pp. 391. \$10.95.)

This is a collection of articles, most of which have appeared before in the pages of the *Law and Society Review* and elsewhere. A couple of these essays are already in wide circulation and well known to practitioners; others are less well known and likely to remain so. All bear on the elusive concept of legal obedience—how, when, and why can we expect people to

conform in their behavior to what the law requires.

Like most collections of this kind, the general subject of inquiry is about all that links the essays together. The utility of such a volume lies not in its theoretical integration but in the diversity of efforts and the breadth of research it represents. And in that respect this is a good, and useful, book. It provides samplings of compliance studies from several disciplines. As a group, these studies remind us that the empirical investigation of legal compliance is still in its infancy. Of course, the subject is as old as the law itself, and the problem it represents is endemic to all organized societies. But it is only recently that enough empirical work was under way even to contemplate a volume like this.

For the political scientist one message of these essays is clear enough: the theoretical payoff of single case compliance studies is distinctly limited. However interesting such case studies may be, the antecedents of compliance are to be found in more general inquiries about citizen attitudes toward the law and perceptions of the legitimacy of institutions and policy outputs, studies of personality types and group behavior, and other long-term factors. This is not to deny the validity of situational factors which are generally described in cost-benefit terms. There is as yet no general theory of law-abidingness which could explain the presence or absence of compliant behavior. But there is enough evidence to suggest that a predisposition to obey the law, either expressive of, or related to, a general feeling of obligation toward the system, is indeed characteristic of most citizens, and probably associated to some degree with actual compliance. Thus if we are to do more than merely describe how citizens respond to a particular law or Supreme Court decision, we must understand something of the compliance phenomenon generally.

Willingness—or ability—to comply with the law is a necessary focus of impact/outcome studies. Yet as the editors of this volume clearly note, the concepts of impact and compliance are quite different. Impact comprises a wider range of phenomena over a longer period of time; compliance or noncompliance may be a part of impact; indeed it may determine impact. But rarely will compliance complete the impact equation.

The identity between compliance and impact is close where, as in the school prayer cases, the cessation of the prohibited behavior (prayers) fulfills all of the stated or reasonably inferable goals of the policy source—the Supreme Court. Yet even in this relatively dis-

crete policy area one could trace the indirect impact of the prayer decisions *and* the initial response to them to the generation of new sets of policy demands and decisions in related fields. Did the prayer cases clear away enough debris to facilitate further attacks on the accommodationist church-state position? Or, alternatively, did it facilitate the accommodationist position (e.g., support Parochial plans) by satisfying those who sought some symbolic reassurance of a separation between church and state? What impact did school prayer controversies have on local politics generally? Did the widely reported lack of compliance with these decisions affect dispositions toward law-abidingness generally? Or was noncompliance self-excused as a rule exception justified (or rationalized) by a perceived lack of legitimacy in the policy itself? As these questions suggest, and as Richard Johnson and others have noted, any policy decision normally evokes a range of responses, of which compliance is but one. But it is often the initial response and may well set the tone of what is to follow.

Not only is compliance merely one part of a larger set of policy impacts, but often it is itself a continuum of responses within flexible limits, rather than a dichotomous variable. This is one of the most difficult problems to be met in compliance research. When laws are not clear or specific enough in what they require, the determination that a citizen is not complying is necessarily subjective. Even when laws seem clear enough on their face, official discretion in enforcement renders subjective the citizen's obligation to obey and his calculation as to the possible costs of noncompliance.

Two additional major problems of research are reflected by and discussed in this book. The first flows from the assumption that, almost by definition, the behavior of most people most of the time conforms to the law. It would be easy enough to establish that a large portion of conforming behavior is just—and only—that. The law is simply superfluous to individual choice; it operates at most as a very remote reminder of theoretical consequences which might ensue if certain actions are taken. Technically such conforming behavior is not in—or more properly, “resulting from”—compliance with the law.

Some have argued that compliance with the law should refer only to behavior which is consciously and knowingly responsive to the law. But this is a very narrow view. It excludes a whole range of ways in which the law might indirectly be the moving force in securing, or setting the conditions for, compliant behavior. Beyond that, few people really “know” what

the law is in a technical sense; they may simply assume that what they think is improper is also illegal. Often they are correct. Traffic laws generally are probably an exception. Thus, it makes some sense in the study of compliance with traffic laws to distinguish between compliance and mere conformity. But to do so generally would be to restrict unnecessarily the focus of research and our understanding of the complexities of law as an instrument either of social control or social change. Law does not operate only on the knowing few.

A second problem of research, as the authors point out, has both theoretical and empirical referents. We would expect some difference between citizens' attitudes toward the obligation to obey the law and some objective measure of their law-abiding behavior. Yet there are relatively few studies, and few opportunities in research, to measure compliance objectively at the individual level. The predominant empirical research efforts have dealt with traffic law violations precisely because of the ease of measurement. But it is very difficult to generalize from traffic law compliance to the more important and more political interfaces between law and behavior. One way of measuring the degree of compliance with "other" laws is by asking citizens directly. But here there are failures of memory, and possibly considerable intentional inaccuracy stemming from a fear of embarrassment or even exposure to legal sanctions. We have no way of verifying subjective scales of reported non-compliance. Thus, any conclusions drawn about the convergence of attitudes and behavior with respect to legal compliance are at best provisional.

As in so many other areas of research, longitudinal studies provide a unique perspective on the impact of law on behavior. Yet these are difficult to construct, and in most cases we are left with quasi experimental situations which, at least at the aggregate level, attempt to measure legal impact. Richard Lempert's article in this collection, already a minor classic in its field, is rich in suggestions for measuring legal impact generally and compliance in particular.

The very fact that a collection like this is so noteworthy is itself worthy of note. Compliance with the law is, after all, a paradigm of the meaning of political power. It is surprising how little effort has been directed toward a subject of such central concern. What little concern political scientists have shown has come primarily from two fields within the discipline—political philosophy and public law. As important as these two subfields are, and as important as legal compliance is to them,

it is a subject worthy of much wider attention by political scientists. One hopes that this book and other current research is but a harbinger of things to come.

JOEL B. GROSSMAN

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**Rebellion in the University.** By Seymour Martin Lipset. (Boston, Mass.: Little Brown and Co., 1972. Pp. 310. \$3.95, paper.)

S. M. Lipset's volume on American student activism provides a useful summary to the large amount of research on students done as a result of the "student revolt" of the 'sixties. Originally published in 1971, when the results of the 'sixties research were available and yet before the current period of quiet on campus was evident, *Rebellion in the University* catches the flavor of the previous decade. Like much of Lipset's other recent work, this volume combines historical research concerning American student activism, secondary analysis of sociological data (mainly attitude surveys), and some comparative commentary dealing with student activism in other countries. The result of this combination of methodological approaches is a stimulating volume which provides a coherent discussion of some of the key issues relating to student political activism.

A number of themes emerge from *Rebellion in the University*. The fact that students should be in opposition to some of the major directions of contemporary societies is not surprising. Lipset points to data which stress the "oppositional" proclivities of students (and to some extent of faculty too). Some of these data are drawn from attitude surveys of American students, which Lipset argues indicate that students are considerably more liberal in their social and political attitudes than other segments of the American population. Historical data are cited to indicate that students have long been involved in activism in the United States, although there is no major tradition of student involvement in national politics in this country as there is in many other countries, particularly in the Third World. Lipset argues that the major theme of student activism has been liberal in orientation and often linked to foreign policy issues.

While there have been variations in the degree and orientation of student activism in American history, Lipset argues that there are common themes. Criticism of the academic environment, and locally oriented agitation have been recurring elements in student activism. Beginning in the period after the American Revolution, students have criticized and occasionally attacked parts of the educational



system. Concern with foreign policy questions was important not only during the 'sixties, when Vietnam was the overriding issue of the student movement, but also during the 'thirties, the other period of major student activism in American history. The overwhelmingly liberal subculture of the American university, Lipset argues, contributes to the leftist direction of student activism. In an interesting chapter on faculty involvement in student activism, Lipset points out that during the 'sixties faculty members were involved in a significant number of activist demonstrations and that the political orientations of the American professoriate are well to the left of the general population.

In a concluding chapter, Lipset comments that there are many common elements in student activism throughout the world, and that in general students have been a liberal or radical force in their societies. Students, and to some extent faculty members and other intellectuals, have traditionally been politically "unreliable" regardless of the orientation of the society. In the Communist countries, students have spearheaded antiregime activism, as they have done in right-wing dictatorships. Student activism is, furthermore, unpredictable and sometimes counterproductive from the liberal or radical viewpoint. Lipset quotes various pro-left spokesmen to the effect that some elements of the student movement of the 'sixties were damaging to the university as a free and autonomous institution and perhaps to the political goals of the student movement itself. He also points out that the occasional violent tactics of the student movement can create a backlash among conservative or moderate elements.

No major theories emerge from this volume. One has the impression that student activism, in the United States and elsewhere, is no passing phenomenon and that despite the current quiet on campus, student activism could easily become part of the political scene once more. The traditions of activism are strong, and the institutional and sociological prerequisites for activism exist in the universities. While the orientation of student activism is generally leftist, the exact issues or tactics which are used by the student movement are quite unpredictable. Lipset's discussion of the links between student activism and the institutional and intellectual environment of the universities are particularly interesting. Despite the many insights and the by and largely effective use of secondary data, the reader is left wondering what it all means. To some extent, this is not Professor Lipset's fault, as no coherent theory has emerged from the veritable flood of literature on students that poured forth during the

'sixties. *Rebellion in the University* at least provides a summary of much of this literature, asks some interesting questions, and posits a few answers. One may argue with some of these answers, but the book provides stimulating reading.

PHILIP G. ALTBACH

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**Who Makes the Laws?** By David Price. (Cambridge, Mass.: Schenkman Publishing Co., 1972. Pp. ix, 380. \$6.95.)

Twenty years ago Ralph Huitt demonstrated that a perceptive researcher could open up the inside of a Senate committee and explain how its members go about the business of law-making. Many students of the House of Representatives have followed Huitt's lead and exposed the internal operations of virtually every major house committee (and several minor ones). Now, with the publication of David Price's *Who Makes the Laws?*, students of the Senate and national policy making have a major work to place along side Huitt's work and the literature on the House. Professor Price's work on the Senate will, it is hoped, help stimulate more research on the Senate to bring our knowledge of that institution up to the level attained for the House.

The core of this book is a comparative analysis of 13 major bills handled by three Senate committees during the 89th Congress (1965-1966). But the book is not a plodding recitation of the legislative histories of these bills. Price treats his material within a broad theoretical framework that allows him to make suggestive generalizations about the role of the Senate and its committees in policy making. In the process, he demonstrates a nice feel for legislative politics, records numerous doubts about the tendency of political scientists to overgeneralize about the policy-making process, and develops an analytical framework that should prove useful to other students of national policy making. The book is marred only by occasional philosophical digressions about intentional behavior, phenomenology, and "human nature." Since these concerns are not fully developed nor central to the analysis, they strike the reader as rather jarring asides. For the most part, though, Price sticks close to his rich, unpretentious framework. The result is a rich, unpretentious book.

Instead of raising such global questions as which branch of government dominates legislation, Price develops a sixfold functional scheme of the policy-making process, and uses it to organize his materials. In order for pro-

posals to become law, they must be *instigated and publicized*, *formulated* into specific remedies, and studied (*information gathering*). *Interest aggregation* and *mobilization* of support (the distinction between them is not too sharp), and *modification* of proposals, demarcate three additional phases of policy making. There is, of course, some similarity between this list of functions and Lasswell's well-known categories (intelligence, recommendation, prescription, invocation, application, appraisal, termination), but Price makes a persuasive case that since the legislative process is a thoroughly political process, the rather bloodless Lasswellian categories will not do.

In the course of exploring policy making in terms of these six functions, Price teaches us a great deal about the Senate Committees on Finance, Commerce, and Labor and Public Welfare. The Senate legislative process emerges as a messy and fascinating struggle among a vast number of interested parties. The policy process varies from committee to committee and even within committees from bill to bill. In the end, however, some generalizations emerge that neither strain the analytical schema nor merely support the conventional wisdom about Washington politics.

One of the most interesting findings is that even during the 89th Congress the legislature played a vital role in the policy-making process. The executive tended to perform the information-gathering function, and the committees tended to dominate the modification function, but the most impressive feature of the process is the extent to which most of the functions are shared by the executive and legislative branches of government. This, of course, poses difficulties for Lowi's theory of policy making based on the distributive, redistributive, or regulatory content of policy. Shared functions defy neat typologies but perhaps come closer to the reality of the situation than any of the models currently in vogue.

Many of Price's findings, as he himself makes clear, are limited by the nature of the case studies examined. All 13 bills were relatively major items, but some were more major than others. The time span is limited to one Congress, and only three committees are examined. Still, there is much in this book that confirms Polsby's suggestive theory about the capacity of the Senate to play an innovative role in policy making, to act as a crucial nerve-end of the polity. Those interested in the struggle to understand the Senate and how national policy is made would do well to read Price's book in the context of Polsby's theory. If such ef-

forts are continued, the oft-heard lament about how little understood the Senate is compared to the House may soon be inaccurate.

JOHN F. MANLEY

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**Slavery in the Structure of American Politics, 1765-1820.** By Donald L. Robinson. (New York: Harcourt, Brace, Jovanovich, Inc., 1971. Pp. xii, 564. \$13.95.)

The political confrontation between North and South over slavery has traditionally been viewed as a conflict that began in the 1820s and 1830s. The founders of the American republic allegedly believed that the slave system of the South would stabilize and erode, and that it was therefore possible and politic to create a national government without coming to grips with the issue. But the unexpected vitality of slavery upset these calculations and set the stage for eventual conflict between the sections. The confrontation finally began when the application of Missouri for statehood threatened to disturb the balance of power in the national government. Most studies of the Missouri controversy stress the political calm that preceded the confrontation, and the eagerness of political leaders of both sections to keep slavery out of national politics.

Professor Donald L. Robinson, in *Slavery in the Structure of American Politics, 1765-1820*, rejects this familiar analysis. He shows that slavery was not a dying institution at the time the American republic was founded, and that the decision of the founders to avoid challenging the system was based, not on confidence that it would gradually disappear, but on the conviction that slavery and racial bias were beyond political solution. The founders persuaded themselves that unless slavery could be excluded from national politics, the new republic would soon be torn apart by sectional conflict. But many of them also knew that by masking the contradiction between their racial practices and their professed ideals they were bequeathing a corrosive legacy to posterity. By failing to face the issue squarely, they implanted irreconcilable conflict at the very heart of the nation's political system.

The attempt to keep slavery out of national politics failed completely. Robinson shows that the issue was never dormant, that it surfaced repeatedly prior to 1820, and that the bitterness displayed on both sides poisoned political processes from the beginning. He traces the slavery conflict to the very inception of national life during the Revolutionary War. The slave system weakened the South militarily and in-

spired resentment about the relative contribution of the sections to the war effort. When the scene of battle shifted southward in 1779, Southerners resented the failure of the Continental Army to leave its northern base and come to their assistance. Northerners, for their part, deplored the failure of the South to offer effective resistance to the British invasion.

Robinson stresses the continuity of the slavery issue in the years following the war. He shows how sectional differences over slavery helped to define sectional attitudes on other issues that confronted the nation. It affected foreign relations and domestic policy alike; it was the issue that cropped up continually and divisively as the new nation struggled to take hold. The author draws an excellent portrait of differences in temperament between Northern and Southern leaders—differences that could be ascribed to slavery, and that raised the level of acrimony by several decibels.

This is an immensely valuable book, not only in the wealth of detail it provides, but in its penetrating analysis of how national policy evolved in the period before 1820. The writing and organization are admirable. Every chapter, indeed every paragraph, is carefully conceived and lucidly presented. Robinson has been able to separate the substance from the rhetoric of political conflict. The result is a reliable, comprehensive account of how slavery affected major political developments in the United States from the Revolution to the Compromise of 1820.

Despite its many excellent qualities, the book is sometimes flawed by discursiveness and an occasional loss of topicality. Material is included that has only a tenuous bearing on the slavery question, such as the description of military campaigns during the Revolution, inter-sectional rivalries under the Articles of Confederation, and the details of debates in the Constitutional Convention. If handled more concisely, these digressions might have provided useful background; they are carried to such length, however, that the book sometimes goes beyond the stated purpose of the author. This is not to say that the surplus material is not interesting and ably presented, only that it occasionally shifts the emphasis of the book out of its proper focus.

But such criticism is only minor in view of Robinson's overall achievement. He has written a thoughtful work of meticulous scholarship that not only illuminates a dark chapter of the American past but also speaks cogently to the present. Filled with ideas and fresh interpretations, it adds significantly to our understand-

ing of the racial dilemma that has plagued this nation from the beginning. The book cannot be praised too highly.

EDGAR J. McMANUS

*Queens College, City University of New York*

**The Political Economy of Urban Poverty.** By Charles Sackrey. (New York: W. W. Norton & Co., 1973. Pp. xii, 172. \$6.95, cloth; \$1.95, paper.)

This is an interesting and engagingly written volume by an economist who has done his subject-matter research well, and has, in addition, suffused his analysis of the political economy of urban poverty with a considerable amount of soul searching. It is a peculiar blend of several approaches, themes and styles. The elements in the mix include: a criticism of the work of many of his academic fellows in economics and generally in the social sciences; a penetrating and lucid discussion of the explanations which economists and others have offered as the causes of urban poverty; a polemic on the state of America's social order dripping with moral indignation; and an introspective political essay on the dilemma of a would-be and sometime rebel against the existing political and social system who, finding unacceptable the immorality of the means which the revolutionaries seemingly have to employ, must, therefore, come to some kind of terms with the realities of engaging in "liberal reform."

The author himself describes his work as "... an introduction to the study of these urban poor and possible solutions to their most serious problems. It is a work in 'political economy' in the sense that it combines an analysis of the economic factors which cause poverty with a discussion of the implications for political activity which are inherent in all such analyses" (p. 1). "These poor" are those millions of Americans whose poverty Professor Sackrey considers one of the most offensive characteristics of the social order which proclaims to have found the true path to material welfare. The relationship between this condition and his political economy concerns are obvious.

Sackrey reviews the methods by which we have determined the number of poor in the country and whether or not those who fall into the poverty category have increased or declined during recent years. He disagrees with both the methods heretofore used and the findings which followed.

His disagreements with the measurements of poverty that are in vogue stem from their concentration on income as the principal index of

poorness, and the fact that what is being measured is absolute poverty rather than relative poverty. Beyond income, poverty must be measured in the disparities in services of various kinds and the character of the opportunity lacks which beset those who are poor and make their lives different in kind though the income differences may seem to be only matters of degree.

In order to make the poverty statistics tell you what reality is, Sackrey insists that they must be related to a meaningful explanation of the causes of the real poverty. In reviewing the causes from those clustering around the "unheavenly poor" theories to those stemming from the various econometric theories, he is biting critical because they explain in terms of variables without examination of the causes of the variables themselves. In short, they either paint a picture based on values which are accepted as given without examining the values themselves or explaining them against the backdrop of a social and economic system whose nature is simply accepted as a given. This sets the stage for his review and analysis of the causes of poverty as explained by the radical theories of poverty. These theories, he explains, are radical in that those economists offering them believe that "poverty cannot be eliminated in an advanced capitalist country" (n. 31, p. 30). In his elucidation of this theme, the author deftly and rather thoroughly introduces the reader to the body of radical literature which may be unfamiliar to many.

Since in Sackrey's judgment the black urban poor exemplify the worst evils of poverty in America, he devotes the bulk of his attention to them and the various explanations for the state in which that population grouping finds itself. The theories of black poverty he reviews fall into two main categories: the "liberal academic" explanations, which result in solutions to be pursued within the framework of the industrial capitalist system; and the "radical theorists" who see poverty as a consequence of the existing system, as wealth is the consequence of that same system. I find some weaknesses in his critique of the "academic liberals," and note that he misses some of the political implications of *those* explanations and the programs that flowed from them. The radical theories which were earlier touched upon are again elucidated forcefully and with discernment as he gives a detailed criticism of traditional economists' shortcomings. In a brilliant chapter on "Economics and Black Poverty" he pursues the explanation of his claim, "Show me your model and I'll tell you your politics."

In an unusual concluding chapter that serves

as his political testament, Sackrey introspectively explains his embrace of and then his rejection of the radical political behavior that followed from his acceptance of the methods and conclusions of the radical political economists. He emerges with a willingness to accept the style and the program essence of the liberal reformer. Space does not allow for a full explanation of his rationale, nor for a description of his modified "academic liberal" program. Its major elements are a black economic development program, a guaranteed annual income, and a strategy for a nonviolent antipoverty politics that builds on some of the things to be learned from the anti-war movement of the '60's, the women's movement, and the counterculture politics.

This is a book aimed principally at students and designed to be most useful in conjunction with classroom pursuits. I do not know Professor Sackrey, but I would bet that he is an interesting, stimulating, and respected teacher.

SEYMOUR Z. MANN

*Hunter College, City University of New York*

#### **Roots of Maryland Democracy, 1753-1776.**

By David Curtis Skaggs. (Westport, Conn.: Greenwood Publishing Co., 1973. Pp. 253. \$12.00.)

In recent years, numerous political scientists and historians have researched in or borrowed from each other's discipline. Studies by scholars such as Burnham, Converse, and Pomper reflect an interest in the American past, while many historians have used political science concepts and methodology to examine electoral and legislative behavior in earlier periods of United States history.

David Curtis Skaggs's book should be considered within this perspective. In focusing upon the question of whether colonial Maryland was or was not becoming a democracy in the years before the American Revolution, Professor Skaggs deals with aspects of electoral behavior and political leadership. Examining these topics in the first part of the book, he contends that many persons did not qualify to vote, because of a property qualification, and that political offices were controlled by the rich. From this evidence, he concludes that prerevolutionary Maryland was not democratic. The second part of the book, a narrative contingent upon the findings in the first section, traces the movement for democracy during the twenty-three years preceding the revolution.

Professor Skaggs's attempt to define democracy rigorously and his efforts to collect and analyze a vast array of data are commendable,

but his arguments are unconvincing. The discussion of suffrage reveals little awareness of recent findings on that subject. The examination of officeholding is far from complete since many offices are not included in the analysis (pp. 85–89). Frequently used terms, such as “radical,” “moderate,” and “conservative” are not defined (pp. 141–153, 176). Many calculations are based upon incorrect assumptions, e.g., that all Maryland counties had the same population breakdown as the colony overall (pp. 40–41). Statistical terms, including “average” and “correlation,” are misused (pp. 41, 89, 217–219). All of these shortcomings undermine his conclusions.

As an example, he contends that about half of Maryland's white adult males could not meet the property qualification for voting (pp. 44–46). He concedes that there are no voting data for the period to verify that assertion (p. 20) but attempts to support it indirectly. There are, however, available election returns for the 1790s, when the property qualification was still in effect. In this period, voter turnout in some counties exceeded 90 per cent of the white adult male population, suggesting that few were actually disfranchised. Turnout was markedly lower in other counties, but the differences seemed to be more related to the presence or absence of competing political organizations that actively sought popular support. Where such organizations contested, turnout was generally high. Where they did not, it was low. This pattern suggests that an examination of suffrage has little substance without also considering whether elections were contested by broadly based political organizations.

This, along with other difficulties in the book, introduces more fundamental questions. By focusing upon the subject of democracy in pre-revolutionary Maryland, Skaggs has attempted a subjective, difficult to resolve investigation. How do you determine precisely what is or what is not “democratic?” What is the basis for the comparison—that is, democratic or undemocratic relative to what? What is actually substantiated if a society is shown to be more or less “democratic?” A rigorous definition, by itself, cannot fully resolve such questions.

Professor Skaggs's emphasis on democracy was, then, unfortunate. In light of recent studies and the data he collected, there were, certainly, other analytical possibilities. By utilizing a variety of concepts and employing careful methodology, he could have dealt with other, more substantive issues. How relevant was the suffrage qualification? Who served in the numerous county level political offices not examined? What kinds of persons provided the

leadership in the various groups that contested for power prior to the Revolution? Were there voting blocs in the colony's legislature? Skaggs has not resolved the question of to what extent colonial Maryland could be considered democratic and, by focusing upon that issue, he has left other potentially interesting questions unanswered.

DAVID A. BOHMER

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**The Age of Giant Corporations: A Microeconomic History of American Business, 1914–1970.** By Robert Sobel. (Westport, Conn.: Greenwood Publishing Company, 1972. Pp. xiii, 257. \$9.00.)

This book is the seventh in the Greenwood Press series, *Contributions in Economics and Economic History*. It is readable and sticks to its point, which is to trace the main changes and general movements in the world of American business organization and experience from the early 20th century to the conglomerate era of the early 1970s. Had the book been written now it would no doubt contain a concise account of the international oil firms, their growth, and the organization of their cartel. As it stands, the obvious latest chapter in Mr. Sobel's book is missing.

The subject matter is the preserve of a very small group of academic specialists. Most economists and economic historians know little or nothing about the changing fine-structure of American business. So this little book fills a need. Possibly without being fully aware of it, Professor Sobel advances some striking theses and suggestions. For example, he maintains, supported by his selection of evidence, that the major oligopolies developed in the 1920s best withstood the erosion of the 1930s, and even experienced no little amount of growth in that period (pp. 126, 135, 136, 152). He is particularly good on the linkages between war-time government activities (in all four of our 20th century wars, and the Cold War as well) and subsequent developments of the specific connections between government and major business firms that resulted. Numerous well-chosen examples are given. Sobel's range of interest is shaped by the flow of new industries arising from changes in technology: organization, ownership, development linkages, entrepreneurship, these are the major foci of his interest. Furniture manufacturing will not be found here, but chain stores, automobiles, petrochemicals, motion pictures, TV, semiconductors, computery—these are covered. I found the book a fascinating “short course” on this subject. The book's

blemishes are few enough so that they are startling when they occur, as in the discussion of international finance after World War I: ". . . the United States had emerged as the strongest financial power in the world, while Great Britain, whose pound still financed a large number of international transactions, was in stagnation and decline" (p. 82). That sounds vaguely like the author was not really sure just what it was he wanted to say. But such lapses are few. Sobel places a greater emphasis upon the vagaries of central-bank policies as causal elements in business conditions than would be considered tasteful by some economists. On the other hand, he is clear-eyed about the tendency for concentration and oligopoly to be the harbingers of business success in the modern American economy without taking a moral stance on this subject, which is so repugnant to the true believer in academe. Sobel also sees the steady expansion of government participation by way of controls, as a natural political consequence of these developments. I would recommend this book to any reader who wants a fast and accurate introduction to modern business history.

JONATHAN HUGHES

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**Politics, Science, and Dread Disease: A Short History of United States Medical Research Policy.** By Stephen P. Strickland. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 329. \$9.95.)

The growing maturity of political science is shown in the increasing concern of our colleagues in policy formulation and questions of policy outcomes; the best of this work reveals a fusion of methodological and theoretical sophistication with detailed examination of present and approaching issues of national interests. An excellent example of this type of work is the present volume. In the execution of his "brief history," Mr. Strickland makes an important contribution to our knowledge of the policy formulation process, especially in revealing the three-way relationship which combines legislators, high-ranking federal bureaucrats, and advocates of powerful if little-known interest groups. But more significantly, Strickland provides us with useful insights into the forces and ideologies which shape national health policy. The book could not have been more welcome, for it comes at a time when a series of legislative initiatives are under way in the health-care field; and while the study does not deal with national health insurance, those who are concerned with providing a new direction and set of alternatives in the health-

care sector should read *Politics, Science, and Dread Disease* as a guide for their legislative efforts.

Strickland presents an account of the personalities and organizations which have shaped and directed federal cancer research policy. The arena is Congress, and the actors include subcommittee chairmen in both houses, top bureaucrats from the National Institutes of Health, and several representatives of private interest groups. The ubiquitous Mary Lasker is a dominant character, although Strickland makes it clear that other lobbyists were important and that none was able to control cancer research legislation. The key to control—and especially congressional appropriations—apparently is the medical researcher: physicians and scientists are brought before subcommittees by competing advocates, and they offer expert, recent, relevant advice—often incorporating information obtained from within the National Institutes of Health or one of its agencies but presented with apparent objectivity.

For those, and I confess to being one of them, who wonder about the criteria which congressional decision makers employ in arriving at their positions on highly technical items such as national cancer programs, Strickland's research, if we are willing to accept it at face value, is reassuring. He explains Congress's propensity for increasing medical research budgets above the figures requested by the Executive as reflecting substantive knowledge, certainty of goals, and sophistication regarding the technical aspects of reaching the proposed goals. Moreover, Congress would not have gone along with subcommittee increases, Strickland argues, had the subcommittees not vividly and comprehensively documented the need and adduced the proper scientific evidence.

Strickland's analysis emphasizes the formal actors and organizations; little mention is made of the broader forces of national politics, intra-party rivalry, or public opinion. Indeed, except for an occasional defeat of a key congressperson or an opinion poll, the public, whether ordinary citizens or members of a health related interest group such as the American Heart Association or the Cancer Society, seems to have played no role at all in national cancer policy. It may be that the author's concern with key congresspersons, bureaucrats, and lobbyists caused him to discount or ignore the "attentive public." But I suspect that few individuals beyond the handful of officials, politicians, and researchers discussed in the book were or are today even remotely involved in federal medi-

al policy formation. Nor is the public especially welcome. When lobbyists attempt to inform the public and draw it into the struggle, politicians apparently resent the effort. Strickland reports that when in 1971 the American Cancer Society placed a full-page advertisement in major newspapers urging its point of view on the electorate, the ad created sharp indignation among the members of the House subcommittee, leading to the threat that the Society might have jeopardized its right to tax-exempt status by spending funds to educate the public in order to attempt to influence the votes of its representatives. The ignorance and noninvolvement of the public and the "old boy" style of decision making are the most striking general revelations which emerge from this account. It is regrettable that the author did not directly confront these issues; but his work does offer directional signals for further research.

In sum, Strickland has produced an important, useful, and timely study of an important national issue. It is a model of the successful application of social science techniques and of literate, sensitive writing. The result is a volume that should be read and used as a guide by others.

JERRY L. WEAVER

*University of California, Los Angeles*

**To The Victor . . . : Political Patronage from the Clubhouse to the White House.** By Martin and Susan Tolchin. (New York: Random House, 1971. Pp. 370. \$7.95.)

Much of the recent interpretive writing on American politics has argued the view that the process by which public policy is formulated and executed is more sensitive to narrow considerations of political advantage than to long-run conceptions of the public good. Different writers have located the sources of this condition in quite different places. Mills, for example, located it in the relationships between economic, political, and military elites. Lowi, on the other hand, attributed it to the pulling and hauling of interest groups, a process which he says has been legitimated by academic theories of the political process. In the volume under review, Martin and Susan Tolchin, the former the City Hall Bureau Chief for the New York Times and the latter a member of the political science department at Brooklyn College, point to an old and much abused bogey—the patronage system.

The thesis of the book seems to be that political leaders are only able to maintain themselves in office by promising to deliver to potential supporters a range of material benefits that are at their disposal. In turn, citizens be-

come dependent upon such benefits so they have little choice but to support the politicians that deliver them. Written in the muckraking tradition, the book sets forth its argument through a series of anecdotes which purport to detail the adverse consequences of patronage at every governmental level. The authors offer numerous examples of how political appointments, contracts and preferments, and governmental expenditures are used to solidify the positions of incumbents. Though the authors note that "patronage can be used for good or evil" (p. 26), most of their comments are decidedly critical. The main problem with patronage in their view is that it leads to a system of unplanned and irrational policy making as leaders barter policy in exchange for political support.

As a remedy for this condition, the authors suggest a form of reverse patronage which would "harness government's enormous reward powers on behalf of rational planning, and on behalf of the public interest" (p. 311). Thus, they speak approvingly of Mayor Lindsay's proposal to use New York City's \$11 million worth of holdings in General Motors stock to pressure the company to limit the pollution created by its automobiles. In addition, they suggest that state and federal monies be withdrawn from companies and institutions that practice discriminatory hiring. The latter is, of course, already being done in some areas.

Most readers will find it difficult to understand why the burden of guilt and the responsibility for change must be laid at the doorstep of patronage, especially since most studies of the subject have indicated that patronage now plays a much diminished role in the incentive structures of party organizations. The source of the confusion is that the Tolchins adopt a rather expansive definition of patronage. The conventional notion is that patronage is some material benefit, such as jobs or contracts, that political officials can distribute at their discretion. The authors, however, seem to look upon patronage as any exchange of political goods, which means that all policy becomes a form of patronage. Thus, when Mr. Nixon attempted to ease the pressure for integration in the South, he was actually doling out patronage: "What the Southern strategy amounted to in concrete terms was a massive payment, on a kind of patronage installment plan, of what Nixon considered his major political debt: his election to the Presidency" (p. 282). The argument that patronage is responsible for fragmented policy making thus hinges upon a dubious notion of what patronage actually involves. If the authors are suggesting that lead-

ers exchange policy for political support, then there is not much to argue. But if they are suggesting that this is all a form of patronage politics, then the argument is difficult to accept.

The book is apparently not addressed to an academic market, for the authors do not attempt to deal seriously with the scholarly literature that has grown up around the issue, some of which actually defends patronage. For example, they do not consider the impacts upon party organizations that follow from the uses of material and nonmaterial incentives to recruit activists. The use of material incentives tends to give an organization greater flexibility in defining goals, selecting strategies, and formulating policy. If one believes that parties should be office-seeking organizations that respond to electoral pressures, then a case can be made for patronage in the conventional sense. The authors seem to believe that party leaders should respond primarily to some notion of the public interest without reference to electoral pressures, so they adopt a critical outlook. However, they do not appear to appreciate all the implications of this view.

The book is competently written, and it contains a number of interesting anecdotes about the abuses of patronage. It would not be a bad choice if one is interested in some supplementary reading on the subject, but if one is looking for some solid scholarship on the question of patronage, then one must look elsewhere.

JAMES E. PIERESON

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**Federal Evaluation Policy: Analyzing the Effects of Public Programs.** By Joseph S. Wholey et al. (Washington: The Urban Institute, 1970. Pp. 134. \$2.95, paper.)

Evaluation procedures and techniques are rapidly becoming of critical importance in the development of national social policies and in the administration of federal social programs. To insure that these policies and programs meet the demanding requirements of American society, it is imperative that agencies of government analyze their programs to determine the various consequences—in other words, to measure their successes and failures in achieving national objectives. It has long been evident to students of public administration that the federal government—and, for that matter, state and local government—has barely begun to develop the systems and methodologies necessary to produce viable assessments of this kind.

In this short volume, Wholey and four fel-

low policy analysts on the Urban Institute staff set forth their study of federal evaluation policies—concentrating on the organizational framework, the methodologies, and the amounts of national resources expended during the 1968–1969 period under investigation. Their research was funded by the Department of Housing and Urban Development and was carried out in close cooperation with the Executive Office of the President and the four agencies scrutinized: the Department of Housing and Urban Development; the Office of Economic Opportunity; the Department of Health, Education, and Welfare; and the Department of Labor.

The authors point out early in their study that “the most impressive finding about the evaluation of social programs in the federal government is that substantial work in the field has been almost nonexistent” (p. 15). They elaborate by declaring that those few previous evaluation studies were so poorly conceived and carried out with such a lack of uniformity of design and objective that the findings have not been related to or reflective of the problems confronting the makers of public policies. No comprehensive federal evaluation system exists. For example, the authors observe:

Even within agencies, orderly and integrated evaluation operations have not been established. Funding has been low. Staffing has been worse, forcing undue reliance on outside contractors by agencies that lack the in-house capacity to monitor contract work. The most clear-cut evidence of the primitive state of federal self-evaluation lies in the widespread failure of agencies even to spell out program objectives. Unless goals are precisely stated, there is no standard against which to measure whether the direction of a program or its rate of progress is satisfactory (p. 15).

With this indictment as prologue, the study moves on to a brief but thorough discussion of the chief kinds of evaluation (program impact, program strategy, etc.), alternatives to evaluation (field experiments, experimental, demonstration), and those activities that are evaluation-related (monitoring, reporting systems, cost analysis). Succeeding chapters examine the administration of an evaluation system; organizational relationships and responsibilities; the resources appropriate and available for purposes of evaluation; methodology (research design and method) for achieving sound techniques of evaluation. Each of these sections contains rather extensive lists of recommendations—related to the evaluative process—that the authors strongly believe should be widely adopted by federal agencies and departments.



The concluding chapter to this "cookbook-y" approach to evaluation contains an extensive review of the major recommendations for enhancing the ability of federal officials to analyze comprehensively the effects of their myriad public programs.

This administrative repair manual, while sometimes dull and plodding, has its moments of great insight, and the many remedial measures set forth are sound and capable of relatively rapid achievement within our bureaucracy. This work clearly belongs to the fast-growing list of volumes concerned with "inside public administration" and should be read (whether with enjoyment or not) by both students and practitioners of the craft, especially those administrators who want to achieve the objectives of various government social policies with the least amount of human or financial resources.

In fairness to our bureaucrats, it should be acknowledged that the authors relate that a significant measure of change and improvement has occurred in evaluation techniques in the agencies and departments investigated since this study was initiated. Presumably, this increased sophistication and improvement have been eagerly promoted up to the present time.

Within its limited terms of reference, this volume does a most creditable job of touching on the principal characteristics and problems associated with the evaluative process and of stimulating the serious reader to further works on this general topic by means of an excellent select bibliography.

JOEL R. DICKINSON

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**Latin American Legislatures: Their Role and Influence, Analyses for Nine Countries.**

Edited by Weston H. Agor. (New York: Praeger Publishers, 1971. Pp. 576. \$22.50.)

In the 1960s, when area studies were in bloom, irrigated by public and quasi-public money, "research agendas" began to appear in the land in great number. A recent one, calling for analysis of the "characters, roles and functions of national legislatures" in Latin America, led to this book. Eleven papers are collected here, all by young scholars (the oldest doctorate dates from 1963, near the zenith of the Ford Age). The collection aims at testing two propositions described by the editor as aspects of the conventional wisdom: (1) the view that Latin American legislatures are "rubber stamps, marginal to the decision-making process in the region," and (2) the view that the same legislatures are "obstacles to political and economic development" (p. xxvi).

It is hard to see how both these views can be held at the same time concerning the same legislature. Perhaps the editor meant to say that the assumed consensus view was an either/or proposition: Latin American legislatures, to the extent that they are not rubber stamps, are obstacles to development. So reformulated, the conventional wisdom emerges from these eleven studies not only unshaken but supported.

The nine legislatures studied are divided into three groups: the "modal cases," in which the legislature is weak in decisional influence (Venezuela, Colombia); the "deviant cases," in which the legislature is "relatively influential on a world scale" (Chile, Costa Rica, Uruguay); and the "transitional cases," in which the legislature either demonstrates "increasing decisional influence" or has been closed down since 1960 (Argentina, Brazil, Guatemala, Peru). Since the book's publication, of course, Chile and Uruguay presumably have found their way out of the "deviant" class and into the "transitional" class—although calling a legislature transitional when its doors have been closed by the generals is at least ironic.

It is the thesis of Terry McCoy's paper on Peru that the Congress has, indeed, played a significant role in Peruvian political life. That role, McCoy carefully shows, has been negative, impeding presidential action to the point where civilian government is paralyzed and military intervention unavoidable. Congressional power and independence, then, imply obstruction of development; Peruvian development proceeds when Congress is removed as an independent influence. Score one for the conventional wisdom. The cases of Argentina, Uruguay and Chile demonstrate that generals, too, are capable of failing the development test—but in none of those countries could it be argued seriously that the pre-coup legislature had been anything but an impediment to development.

Brazil's Congress was permitted to continue functioning in the immediate aftermath of the military coup of 1964. Robert Packenham's paper identifies a number of functions which the Congress performed during the period before 1966, when its role was reduced to near zero: (a) legitimation; (b) recruitment, socialization and training; and (c) decisional functions. The latter function, drastically reduced, was largely limited to a House-of-Lords style articulation of interests. But the legitimation function, as Packenham analyzes it, was an important one. In other words, the Brazilian Congress after 1964 made its most important mark on political life simply by continuing to meet, simulating an independence that helped

legitimize the generals. It bears emphasis that any such legitimization by a rubber-stamp Congress is necessarily a short-term affair. The legitimacy so conferred is a wasting asset, dependent on a popular illusion of congressional independence, an illusion that becomes progressively harder to maintain as genuine independence recedes into history.

If Peru and Brazil are "transitional" cases, that term is used not in an historical sense but to describe phenomena lying somewhere between what the editor calls the "modal" and "deviant" cases. The vocabulary is dictated not by history but by science. In fact, much of the book consists of efforts at quantification and scaling. Thus we learn that in Argentina the fathers of legislators who completed their university training tended to be professionals, whereas those fathers who attended, but failed to complete, the university and those with a secondary school education usually entered the sphere of commerce or business (p. 209).

And in Guatemala, rightists have more university training and foreign-travel experience than do leftists (pp. 308, 314). In Venezuela, one "cultural variable" influencing the output levels of the Senate is the tradition of "multiple career patterns" (pp. 485-86). But do Venezuelan Senators do little because they have other jobs, or do they keep their other jobs because the Senate is a trivial place, offering them little to do?

The McCoy and Pakenham papers, which I found the most useful, reject such sorties into science in favor of telling us what has happened in Peru and Brazil. They prompt me to suggest a formula as a guide to readers of this book. It is:  $U = 1/Q_v \cdot Q_s$  in which  $U$  = the utility of the paper,  $Q_v$  = the volume of quantification and  $Q_s$  the sophistication of quantitative method.

My other major complaint goes to the near-absence of any discussion of the substantive interests at stake in the political systems studied. McCoy does tell us why the various actors acted as they did, what interests they were defending, and the like. And Ernest Duff, in his paper on Colombia, does treat briefly two "case studies" of the issues of agrarian reform and constitutional amendment. But even here the assumption seems to be that, at least in the context of land reform, legislation and the political process are the same thing. Colombia, of course, is the archetypal model for the proposition that one of the most effective instruments for avoiding a land reform is a land reform law.

Perhaps it is an old spouses' tale that Latin

American legislatures are either ineffective or obstructionist. Nothing here convinces me that the tale is false.

KENNETH L. KARST

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**Aprismo: The Ideas and Doctrines of Víctor Raúl Haya de la Torre.** By Robert J. Alexander. (Kent, Ohio: The Kent State University Press, 1973. Pp. 367. \$12.00.)

Professor Alexander has produced an important addition to the literature in English that is helpful in explaining Latin American politics. Víctor Raúl Haya de la Torre, according to Alexander, "is the most important political philosopher and ideologist of the democratic left that Latin America has had in the present century" (p. 3). Rereading Haya in Alexander's translation after 25 years confirms this judgment. Haya wrote more than 16 books and hundreds of articles, and Professor Alexander has carefully selected and translated the heart of this corpus. He includes sections about Haya's theory of historic space-time, about the Aprista movement's program, about the kind of political parties Latin America needs, about what kind of political machinery Peru should have, about democracy and dictatorship, about why the Latin American republics ought to unite into a federated state, and about what the relations of the future united Latin America ought to be with the United States of America. For each section of the book, Professor Alexander has provided an introductory note which synthesizes the material, and in a 27-page introduction he reviews Haya's life and the history of the Aprista party which he founded and has led for fifty years.

Haya de la Torre's political philosophy blends ideas about the unique character of Latin America with the general ideas of Western Christianity and European Marxism to create what he calls Aprismo. Haya's great contribution has been to emphasize that Latin America is different from Europe and the United States and therefore must cease imitating their political and economic institutions. He argues that most of Latin America's political leaders have always sought their inspiration and their models in foreign lands and not in the reality of Latin America. Thus, political, religious, and economic systems that had developed in Europe and the United States were introduced into Latin America where they failed, producing turmoil and backwardness. Haya emphasized that the Latin American leader must begin to base his programs on the true character of Latin America and develop political and other institutions which fit the

customs of the people and were not copies of United States Constitutionalism, Soviet Communism, Adam Smith's free enterprise, Maoism, European Christian Democracy, or any other foreign model.

By 1974 most of the intelligent leaders of Latin America have accepted this concept first publicized by Haya. In Peru, a military dictatorship is introducing practically all of the specific planks of the Aprista program except democracy.

Haya de la Torre came to his ideas because he never seemed to be able to fit Peru into the European scheme of dividing world history into ancient, medieval, and modern periods. Haya was born in Trujillo, a small city on the Pacific coast of Peru located near the ruins of the pre-Colombian city of Chan-Chan. Where, he kept wondering, does Chan-Chan fit into the European scheme of world history? When the Peruvian dictatorship exiled Haya in 1923 he went looking for the answer to this question. In Russia he discussed Communism with the leaders of the Bolshevik Party; in Germany he observed the Nazi Party; in Italy he looked at Fascism. He spent long years studying in England, but he never found the answer to his question of where does one fit the indigenous people of America into the European scheme of world history. Whenever he looked at Peru he saw that half of the population continued to speak Quechua and Aymara, to wear its traditional clothes, and to follow its age-old customs. Haya kept wondering why this should be until he decided that there were two cultural groups in Peru which had to be blended into a new synthesis which would be truly Peruvian and neither traditional Indian or imported European.

It was from his speculations about the place of the Indian that Haya developed his philosophical concept of historical space-time. According to Haya, there is in each country a certain rhythm or spirit or collective consciousness which its people develop as a result of all the influences to which they have been exposed: historical, geographical, climatic, etc. This "X" quality which Haya named historical space-time motivates a people so that they develop in harmony with their historical space-time despite the introduction of social institutions from another area. The conclusion for Haya is clear: Latin Americans must cease copying the world and develop in harmony with their true character.

The only serious criticism of the book is that it lacks selections from Haya's writings about the connection between the means used to come to power and the ends achieved. Haya

has always insisted that political leaders must not seek power with the gun, that only elections are valid means of creating governments, and that political leaders must learn to be responsible in opposition before they can serve successfully and responsibly in power.

Professor Alexander identifies many of the obscure references in Haya's text in footnotes and adds a very short bibliographic note. Unfortunately there is no index. This is a valuable book for understanding Latin America.

HARRY KANTOR

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**Realignment of World Power: The Russo-Chinese Schism Under the Impact of Mao Tse-tung's Last Revolution, Vol. I and Vol. II.** By Oton Ambroz. (New York: Robert Speller & Sons, Publishers, Inc., 1972. Vol. I. Pp. 338, Vol. II. Pp. 406. \$12.50 each.)

Dr. Oton Ambroz, "a veteran journalist and political analyst on communist affairs," (quotation from jacket flap) has combed through and made use of a wealth of contemporary source materials to write a timely two-volume work. Sometimes his ready use of quotes tend to stifle the flow of his major ideas and concepts, but nonetheless, it does seem to bolster his main conclusions.

Dr. Ambroz believes that ideology is far from dead in our world. He makes the point, correctly in my opinion, that neither the Soviet Union nor the People's Republic of China believes ideology to be a receding element in either domestic or international affairs. These two Communist states are vying with one another for leadership among those countries now governed by Communist authoritarian governments and among those developing countries whose people may be susceptible to Communist promises. Dr. Ambroz assumes that this battle for ideological supremacy will continue and intensify in the future. He sees no compromise.

Ideological differences, however, form only part of the schism between the two largest Communist countries. Their "national-imperial" interests also meet head on, and in the view of Dr. Ambroz, this conflict is "much more important than . . . ideological squabbles" (II, 291). His quotations from Soviet sources on this are most compelling.

What all of this amounts to is that the author throughout his work is building up the case that the differences between the Soviet Union and the People's Republic of China are well-nigh irreconcilable. He cites quotation after quotation from Chinese and Soviet leaders, magazines and newspapers which back up

this contention. The rhetoric from both sides does seem overwhelming and definitive.

I do not dispute the fact that tensions between the USSR and the People's Republic of China are at a fever pitch or that the existence of such tensions will continue. A word of caution, however, may be in order. We have seen seemingly irreconcilable differences between states diminish almost overnight and make possible a *détente* if not an actual *entente*. This is particularly true for countries with similar political and social systems, as in the Soviet-German agreements of August, 1939. After all, as Dr. Ambroz makes clear in the preface of his work, quoting Professors Brzezinski and Friedrich, "fascist and communist totalitarian dictatorships are basically alike" (I, xiii): While the Soviet-Nazi arrangement was not long lasting, it did work for a time, during which the map of Europe was redrawn, Nazi power furthered, and Japan's decision to move southward in Asia toward confrontation with the United States brought closer.

A number of scenarios for the future of Sino-Soviet relations can be written ranging from military conflict, as one extreme, to *de facto* alliance as well as the present *de jure* alliance, as the other. Publications in Communist China today are carrying articles which indicate that differences exist within Chinese governing circles in part at least over China's international affairs. How deep and far-reaching these differences are, and to what extent they may presage a change in policy with a change in leadership is unknown to the outsider. It might be well for us to remember, however, that nothing is permanent in this world, even differences that appear to be irreconcilable.

Having made the above point, I should add that I support Dr. Ambroz's view that the Russo-Chinese schism is the essential element in what he terms the "realignment of world power." Without this schism, the United States could surely not have moved with any degree of success, as it has, to open relations with Peking and to better relations with Moscow. The United States has skillfully taken advantage of the Moscow-Peking confrontation to reduce tensions by adopting a negotiating attitude toward the Soviet Union and the People's Republic of China. These negotiations, however, can only be fruitful if the United States maintains her military strength and alliance structure. Negotiating from strength, after all, is essential for the United States and its allies during this period of changing relationships between and with Communist states. Those concerned with international affairs and America's

role in world politics should read Dr. Ambroz' well-documented work on the Sino-Soviet dispute and the effect of this dispute on the global power structure.

GASTON J. SIGUR

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**The Matrix of Policy in the Philippines.** By Harvey A. Averch, John E. Koehler, and Frank H. Denton. (Princeton, N.J.: Princeton University Press, 1971. Pp. ix, 234 \$8.00.)

This is a sophisticated exercise in quantification. The book is devoted to the discussion of the perceptions of the political system, electoral politics, the "lurching" economy, crime, and dissidence. Thanks to their sponsorship by the AID and RAND, the authors (economists with the NSF, Rand and A.I.D.) had access to all sorts of official and semi-official data. They also relied on a stratified random sample survey especially carried out for them in 1969.

Rather triumphantly, the authors found the Philippines to be quite the opposite of what most observers, foreign and local, had pictured. The political system was "stable—hard to change and unlikely to disintegrate" (p. 26). The public, politicians and bureaucrats "all know what to expect from one another"; hence the political system "is responsive to the needs of the people . . ." (p. 45). As for the economy, its growth was "respectable" and "[spreading] broadly across the country" (p. 69). Unemployment was largely illusory as it was "concentrated in quite specific groups" (p. 75), affecting mostly individuals who had never been employed before. In short, "unemployment does not appear to be a widespread and mounting economic problem" (p. 78). Although the level of violence was "*relatively high*" (p. 120, italics in original), there was no crime wave and crime had actually been decreasing. Violent crime was largely related to politics and was "determined principally by ethnic factors and proximity to Manila" (p. 126). Finally, dissidence, primarily that of the Huks, had become only a "nuisance" to the government. The success of the HMB "rests in large measure on what they do rather than on the condition of . . . society . . ." (p. 150).

With these rather startling findings, the authors make a very strong claim for the virtues of rigorous empirical methods—in this case principally factor and regression analyses. Indeed, they seem to be saying that not much was actually upsetting in the Philippines (and other LDC's as well?) other than the inadequacy of data and analysis. There is of course a great deal to be said for the greater use of

quantitative analysis and the dearth of reliable information is almost inherent in underdevelopment. The trouble is that the authors seem to have been too obsessed with their initial contractual commitment to the promise of quantification.

For one thing, there is too much ingenuity in the analysis. The authors consider the finding that 82 per cent of respondents perceive at least some politicians to be corrupt (Table I, p. 32) paradoxical in terms of the revelation that 90 per cent value honesty most in their candidates (p. 36). At any rate, the latter, together with another finding that 73 per cent of the respondents acknowledge the influence of government on their daily life and that 73 per cent of these also believe that their life would be worse without government activities (p. 28) is supposed to indicate political contentment. The authors simply dismiss an unemployment rate of about 38 per cent by saying that many of those considered jobless are simply without well-defined jobs and "are, by Philippine standards, far from destitute . . ." (p. 77). They also dismiss the less-than-bright national accounts as at best ambiguous. Economic growth is equated with the growth of the noncraft "modern labor force" in "regular, visible establishments" which happened to increase by about 60 per cent between 1960 and 1968 (Figure 8, p. 87). Most interestingly, the authors support their optimistic conclusion about dissidence with the fact that the respondents in the affected areas were not openly less respectful toward the government than other groups (pp. 136-8).

The book is also clever in a more fundamental way. Why did the authors choose to concentrate on those issues presented in the book? Why did they elect to analyze these particular aspects of those problems? Why, for example, should the size of the modern labor force be more indicative of economic development than, say, the standard of public health? No serious justification is offered. It is intriguing how the authors touch and then push aside such questions as the distribution of income and wealth, and social and economic disparities among the provinces. In the face of new and more active dissident movements in various parts of the country, the concentration on the Huks is, to say the least, puzzling.

Judging from their torrential condemnation of almost all aspects of the old society, one has to conclude that the leaders of the New Society do not share the authors' view about the contentment of the populace. Of course their rhetoric might have been merely tactical. But they could not have been simply the victims

of the alleged general delusion, for they had at least been familiar with the authors' findings some time before September 1972. Indeed, Marcos himself had used the earlier RAND report version as the basis of the central chapter of his 1971 manifesto, *Today's Revolution: Democracy*.

The inherent shortcoming of a book like this is its innocence. We surely need good data, but one set of data may be more relevant or significant than another, equally rigorous set. Different analysts may not look at the same things or the same aspects of the same things. Exactly the same data may also be interpreted and evaluated differently. Certainly many students of development do not put much value on the size of the modern labor force as an indication of development. Marcos clearly knew that the highly personalistic politics was relatively stable but he obviously did not think it too hard to change.

One fashionable ideological stand is shared by the authors and the Marcos revolutionaries. Whatever it is, development is supposedly best achieved technocratically through the "politics of trying to be above politics." Such an approach is doomed to failure.

MOHD. A. NAWAWI

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**Rich Nations and Poor in Peace and War.** By Henry Barbera. (Lexington, Mass.: Lexington Books, 1973. Pp. xiii, 213. \$13.00.)

Some years ago, Theodore Caplow argued most persuasively that because of its reliability, accessibility, and congruence with alternative measures, telephones per capita should be adopted as the best single index of socioeconomic development. Professor Barbera, who is evidently one of Caplow's former students, has utilized the same measure as his sole dependent variable in an inquiry as to "whether or not war affects development and, if so, to what extent" (p. 2). His conclusion, supportive of what he terms the "irrelevant hypothesis," is that "the two total wars of this century have neither helped nor hindered development or noticeably affected the inequalities between rich nations and poor" (p. 121). The main thrust of this review will be to consider whether shortcomings in the author's research design and execution have yielded results that some readers will view as somewhat surprising.

Barbera examines four main characteristics of nations in the developmental hierarchy over the 40-year period, 1913-1952: *order* ("the relative rank stability of nations"); *integration* ("the diminishing of relative spread of nations"); *progress* ("the relative speed of ma-

terial improvement"); and *satisfaction* ("the reduction of relative stress") [p. 28]. Each characteristic is evaluated by means of a single statistical test: order by product-moment correlation of the arrays at different points in time (1913/20, 1913/25, 1938/47, and 1938/52); integration by comparing coefficients of variation for each of the six years; progress by calculating regression (slope) coefficients for different groups of nations through time; and satisfaction by comparing ratios of "ranks involved in change" to "the number of nations that have changed ranks" (p. 109). The latter procedure, as might be expected, yields rather inconclusive results since the author himself confesses that he intends to "leap from an objective fact, rank changes, to a subjective state, stress" on the assumption that "an analysis of rank change will provide clues as to what a dominant subjective state might be" (p. 109). Furthermore, he evidently assumes that the *perception* of rank change by a nation's citizens will occur simultaneously with the *fact* of such change, thus ruling out the more than likely possibility of a time-lag relationship between the two phenomena. Collaterally, in evaluating rank stability, he appears oblivious to the commonly recognized problem of autocorrelation in calculating *r*-coefficients for time-series data; while in evaluating material progress, he does not consider the possibility that beta coefficients may be noncomparable because of scalar differences in arrays that are apparently unstandardized.

There are other problems that worry me. For example, Barbera's sample consists of 70 territorial units, including some two dozen that have only recently become independent and one (Mozambique) that, as of this writing, is still dependent. For many purposes, of course, the precise composition of the sample may be irrelevant if the latter can be defended as being reasonably representative of a larger universe. Crucial to Barbera's analysis, however, is his categorization of nations by "degree of war participation," which is operationally based on "years of involvement." This leads, however, to some distressing anomalies, such as the fact that India's degree of participation in World War I is coded the same as that of Germany, and that Ceylon's participation in World War II is construed as being equivalent to that of Belgium.

In addition to the matter of validity, Barbera also has some problems of comparability, as evidenced by his somewhat astonishing assertion that "it is quite legitimate to compare West Germany in 1952 with the Third Reich in 1938" (p. 26). In this case, it would have been a simple matter, for the one variable involved,

to have analytically "reunited" the two contemporary Germanies in order to approximate comparability with the interwar state.

It is also difficult to see why Barbera felt constrained, in a time-series analysis, to limit himself to the year immediately before and the second and fifth years after World Wars I and II. Given the nature of his dependent variable relevant *annual* data were certainly available data which the author merely tells us he chose not to use "in the interest of parsimony and personal judgement" (p. 47). The decision may have been unfortunate, since my own tests (based on annual data) of a portion of Barbera's analysis yields overall congruence with his findings for the first 12 years after each war, but a slightly widening "gap" between the rich nations and the poor thereafter. The latter results (which are admittedly inconclusive, at this point) would suggest that Barbera's rejection of the "negative hypothesis" (that war is developmentally dysfunctional) should be carefully re-examined.

Finally, while both Caplow and Barbera are probably right in identifying telephones per capita as the best *single* measure of development, it is obvious that it is not the *only* measure that might be so employed. Indeed, as I have indicated elsewhere ("Industrialization and Development," *Economic Development and Cultural Change*, 22 [January 1974]), a variety of quite different longitudinal trend lines, exhibiting markedly different patterns of "gap" between rich nations and poor, are possible, depending on the indices being employed. Most importantly, the "gap" (for the modern period) fluctuates *least* when communications indices are utilized.

Single measures are easier to work with and are intuitively more appealing than are composite measures, but they can rarely be construed as valid operational indicators of complex empirical phenomena, and when so employed, they have a tendency to yield either inconclusive or erroneous research results. Professor Barbera has produced an interesting (and remarkably well-written) book, but a more sophisticated analysis is required if his conclusion is to be substantiated.

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**Red Guard: The Political Biography of Dai Hsiao-ai.** By Gordon A. Bennett and Ronald N. Montaperto. (New York: Doubleday & Company, Inc., 1972. Pp. 280. \$1.95, paper.)

When China's Great Proletarian Cultural Revolution began in the spring of 1966, "Dai Hsiao-ai" (a pseudonym) was a student leader

at an elite upper-middle school in a suburb of Canton. By summer, participation in the Cultural Revolution occupied most of the students' time. Red Guard units were organized, and students patrolled the streets of Canton, confiscating or destroying items in people's homes which struck them as representing traditional or Western influences. Soon Dai and his comrades participated in "revolutionary tourism" in Peking, Tientsin, Shanghai and elsewhere, combining sightseeing and adolescent hi-jinks with revolutionary activity.

Despite his enjoyment of these experiences, Dai became disillusioned with much of his political environment. Dai and his student comrades, perhaps like their counterparts in the United States, were struck by the relative conservatism of workers, peasants, and soldiers, who, according to Maoist doctrine, should be the most revolutionary of classes. The People's Liberation Army (PLA), which Dai and his fellow students had been taught to idolize, became the leading political authority at Kao-chung middle school, and far from behaving like the spearhead of the revolution, suppressed the more radical of the student factions, of which Dai was the leader.

In January and February [Dai wrote] we had risked our lives. Several of my fellows had been injured . . . and now we were penned up in the school while the army, our former heroes and models, consorted with conservative bastards. We had virtually succeeded in seizing power, in making a true revolution. Now the bastards had thrown it all away. . . . I was actually being attacked and suppressed by the very authorities to whom I had dedicated my life. It seemed they had used me and then cast me aside when I ceased to be of value to them. My bitterness knew no bounds (pp. 206-208).

In anger and disappointment, Dai returned home to his peasant family and discovered that the revolutionary activities which had dominated his life and thoughts for the past year and more had had little impact on his home village. The villagers complained constantly about their hard life, and about the scarcity of food. "To them Mao meant only compulsory grain deliveries, long hours of work for little reward, and movements which wasted the nation's time and money" (p. 214). Dai's own arguments in favor of the revolution were dismissed by the peasants. Exhausted, angry, thoroughly disillusioned, he grasped at the chance to escape to Hong Kong, where some relatives lived. Yet Dai did not become simply an anti-Communist exile, Bennett and Montaperto tell us. Still a close follower of events in China, he did not give up his commitment to the goals of Communism and the Cultural Revolution, though he continued to question

the institutions and personalities expressing those goals in China today. Indeed he often also questioned his own decision to leave his country and his people in the midst of their struggle to build a new and better way of life.

Professors Gordon Bennett and Ronald Montaperto have performed an important service for everyone interested in contemporary China by arranging for Dai to write down his account of the events in which he participated, by translating it, and by providing informed background material on the course of the Cultural Revolution nationally as a backdrop to Dai's own experiences. The value of *Red Guard* lies not in providing new information about the Cultural Revolution but in recreating the ambience of that movement as seen and felt by a single active young citizen.

Since *Red Guard*'s most absorbing and useful sections are the long quotations from Dai's own accounts of his experiences and his reactions to them, Bennett and Montaperto's basic decision to present his account as it stands rather than to "correct his biases" seems a wise one. Yet their introductory comments suggest that even so they may have intruded on Dai's account more than would have been desirable: "When we felt that his views on a given matter were too one-sided or that they differed markedly from other sources, we asked him to reconsider his position. . . . In the two or three cases where Dai was intransigent, we have tried to explain why this might have been so" (p. xiv). A straightforward presentation of Dai's one-sided views, with Bennett and Montaperto's comments appended as notes, might have been preferable, distinguishing more clearly between Dai's recollections and impressions and the authors' own reconstruction of events.

In sum, *Red Guard* offers a unique perspective on the Cultural Revolution, and it is well worth the attention of those interested in understanding the meaning and impact of this movement on the lives and minds of China's new "generation of revolutionary successors."

SHELDON APPLETON

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**Politics in New Guinea.** Edited by R. M. Berndt and P. Lawrence. (Seattle: University of Washington Press, 1973. Pp. 430. \$15.00.)

This collection of papers is long overdue in publication and as a result suffers in content and presentation. The idea for the book was conceived in 1955, given multiple and lengthy birth in *Anthropological Forum* from 1965-70, and finally appeared in toto in the Australian edition in 1971 and the American edition of

1973. Thus the book's main title, *Politics in New Guinea* has become less appropriate than it would have been in 1955: with the expansion in the numbers of local government councils and the formation of the House of Assembly, "politics" in this book takes on a more specialized and restricted sense. It is a book "for the professional anthropologist" and "for those who are vitally concerned with Papuan-New Guinea political administration and government" (p. xviii). Although it is recognized that the papers "do not lend themselves easily to the analysis of contact problems," it is claimed that the book is relevant because New Guinea cultures "have an influential role in contemporary politics" and "shape the attitudes of New Guineans toward the Western political institutions" and thus the book "provides an insight into New Guinea political values and processes in the emerging protonational situation" (p. 24). How valid is this claim?

The book could be useful for political scientists in two ways: it could provide information about particular people and particular areas, or it could provide a way of interpreting present reactions to political change. It is not completely successful in either role. It suffers as an informational resource from an inadequate index. Proper names are not always included, and while it could be argued that some minor characters should be excluded, others of district or national renown should have been included (e.g., Siune, p. 218; Jim Taylor, pp. 211, 258). Other groups are also excluded from the index (e.g., police, pp. 107, 243; Dani of West Irian, p. 248) as are topics such as education (pp. 216, 279), cash cropping/employment (p. 288) and ideology or norms (pp. 292, 298, 281) to name but a few. On a similar technical point, it would have been more convenient for readers if references to the other papers now collected here had been to the book itself rather than to their original references in *Anthropological Forum* (e.g., pp. 313, 319, 396).

The book is also not completely successful in interpreting political change. The research for some papers was carried out before the Second World War (e.g. Kaberry, Oliver), and while some of the papers were originally written in the mid-1950s (e.g. Kaberry, Oliver, Burridge, Lawrence, Berndt), the others were written in the mid-1960s. The varying Administration penetration pattern meant that some authors began their research when an area had been exposed to introduced institutions for some time (e.g., Oliver, Burridge, Chowning

and Goodenough), while others first investigated the situation after an area had had only a few years of first-hand European contact (e.g., Berndt, Lowman-Vayda, Reay). Thus the degree to which the "traditional" societies had been affected by missions or by the Administration and its police and its appointments of officials vary, and the papers themselves vary in the time period they deal with. Watson's paper deals with a leader who died before the establishment of an Australian administrative post in the area and thus is more of historical interest. Burridge's paper, on the other hand shows the way in which the mission and the Administration have affected village life and that Administration-sponsored activities are generally only successful if the role of New Guinean official is combined with that of "traditional" manager (pp. 109-10). Unlike these papers however, Reay's paper stands out in making little concession to the uninitiated reader, and while making useful points about clan land as a resource in political competition, possibly requires the reader to have more background knowledge of the situation in the Wahgi Valley.

Generally the relevance of these papers to an understanding of present-day political processes in the "emerging protonational situation" in Papua New Guinea is tenuous and implicit rather than explicit. They outline the varying leadership patterns in their areas but are concerned with local, parochial issues, not district-wide ones. However some points have immediate application. In an area which recently has seen an increase in clan warfare and the burning down of coffee trees, it is noted (p. 306) that in the 1930s invaders of a village did not consider they had properly destroyed it until the trees (hand planted, individually owned) had been ring-barked. The 1973 Papua versus New Guinea football match which, like an earlier occasion, led to brawling and interregional fighting, could be interestingly compared with the use of football matches as a form of retaliatory action among the Garia of the Madang District (p. 88). Such points of immediate relevance, however, are few and far between.

This collection of papers thus has less relevance for political scientists than would have been the case if it had appeared when first mooted. It is to be hoped that the companion volume being planned on "contemporary political development" in Papua New Guinea (p. xvii) does not have such a long gestation.

PETA COLEBATCH

Waigani, Papua New Guinea



**Comparative Legislatures.** By Jean Blondel. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973. Pp. 173. \$7.95, cloth; \$2.95, paper.)

The field of comparative legislatures is virgin territory, devoid of extensive theoretical work or empirical analysis. On a world map, imaginary dots would litter the landscape with studies of the political parties, bureaucracies, socialization processes, or cultural attributes of disparate nations. But few indeed would be the imaginary dots symbolizing non-American studies of nations whose legislatures have received systematic study as institutions. Fewer still are the crossnational studies seeking to compare and contrast legislatures. Against this backdrop, Jean Blondel has published *Comparative Legislatures*. The book is both a contribution to the discipline and a disappointment.

The first contribution Blondel makes is contained in the data he has collected—covering more than 100 countries in all regions of the world. For example, the appendix includes the following:

1. Constitutional Characteristics of Legislatures: size of lower chamber, voting restrictions in selection of legislators, nature of the party system, form of government, procedural independence of the legislature, emergency power of legislature, power and basis for selecting second chamber, existence of dissolution, power to remove executive, field of legislative jurisdiction.
2. Activities of the Lower House: number of days of sittings, number of bills passed, number of committees, percentage of members who participate on the floor.
3. Social Background and Turnover of Legislators: age, sex, profession, socioeconomic status, turnover.

Given the number of countries involved and the data collection problems that exist, Blondel has achieved a significant feat. It is the most extensive collection of data specific to legislatures of which I am aware. Based on these data, Blondel compares the general characteristics of legislatures across the 100 nations. These comparisons provide useful information establishing the parameters of legislative tendencies in the world and aiding the area specialist by placing particular legislatures in a broader perspective.

The second contribution Blondel makes is likewise impressive. He cuts through much of the haze on legislatures and their supposed impotence to emphasize their resiliency and to argue for a more realistic evaluation of their contributions to modern government. One of the main reasons legislatures have not received

significant study (outside of the United States Congress and a few western parliaments) is because modern political analysts have become disheartened by the inability of legislatures to legislate—that is, to initiate, formulate, and pass public policy. The ensuing disenchantment has led comparative analysts to forsake legislatures as relics of the past. Blondel takes issue with this disenchantment, arguing that the “yardstick” by which analysts should determine the significance of legislatures should not be “whether the legislature ‘really’ passes all the statutes or even most of the rules of the country, and whether it is in a ‘real’ position to make and unmake governments.” Blondel maintains that these are simply not the real “functions” of legislatures. “The function of the legislature is to provide a means of ensuring that there are channels of communication between the people and the executive, as a result of which it is possible for demands to be injected into the decision-making machinery whenever they exist and for the executive decisions to be checked if they raise difficulties, problems, and injustices” (p. 135). In his emphasis on the “constraining” and “informing” functions of legislatures, Blondel renews a perspective as old as Bagehot and Woodrow Wilson. And while he may oversimplify the ability of legislatures to inform or constrain executives when the legislatures lack real law-making powers, still Blondel directs us more clearly toward the real empirical question in regard to legislatures—not whether they fulfill expectations but whether they have systemic significance.

Unfortunately, serious shortcomings detract from these very important contributions. The basic problem is that Blondel’s focus is so broad and comprehensive as to be misleading. Legislatures in dictatorial systems are included along with those in electoral systems. Basic comparisons are made along quantitative scales that may have totally different qualitative meaning in the different types of systems. Blondel recognizes these problems in his chapter on “The Methodological Difficulties of the Study of Legislatures,” but the recognition does not seem to penetrate all of the study. And while focusing on the broad comparisons, Blondel avoids a systematic examination of legislatures in those electoral systems where they are generally more significant and where greater knowledge about them exists. For example, there is no full scale treatment of the nature of legislatures in parliamentary versus presidential systems; there is only cursory discussion of the role that committees play in legis-

latures, with little attempt to characterize and compare committee systems; the central role that party organizations and party systems often play in legislatures receives causal reference rather than rigorous investigation; and no feel is generated for the internal workings of legislatures or their varying decision-making processes. A student unfamiliar with such matters would scarcely find enlightenment in *Comparative Legislatures*.

Thus my evaluation of this book is mixed. Blondel chose to range widely across the untended field. That choice is understandable at this stage in comparative study of legislatures. Certainly "clearing away the underbrush" is a necessary task that Blondel performs well in places. But in the end Blondel has left us only a fallow field. Significant advancement in our understanding of legislatures now requires comparative attention to their inner workings. More limited comparative studies are necessary delineating the different types of organizational structure that exist, the influence of structure on internal behavior and external role, the relation of internal behavior to external authority. *Comparative Legislatures* is a useful reference in these tasks but it makes the work no less pressing.

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**From Ottomanism to Arabism: Essays on the Origins of Arab Nationalism.** By C. Ernest Dawn. (Urbana: University of Illinois Press, 1973. Pp. 212. \$8.95.)

When George Antonius, with a skillful sense of the scope which personal interpretation allows the historian, wrote dramatically of the last weeks of 1918 in the Middle East, "The War was won, and for the first time in its history, the Arab national movement stood abreast of its destiny" (*The Arab Awakening*, New York: Capricorn, 1965, p. 276), he referred to a movement which may have been more amorphous than his objectives led him to disclose. In these collected essays, Professor Dawn examines critically the political origins, ideological aspirations, leadership, and membership of the Arab Revolt and the Arab nationalist movement. His argumentation and his conclusions often differ from those of Antonius.

Three main themes form the core of Dawn's investigations: first, that defense of an injured Eastern self-view against the West served as the catalyst for an examination of the causes of Eastern weaknesses; that defense of this injured self-view embodied, for Ottomanists and Arabists alike, a defense of Islam as a civilization; and that intra-elite conflict rather than class

pressure or class change animated the adoption of Arabism by certain Arab leaders.

These themes are given support in Dawn's detailed examination of more specific items. In his analysis of the leadership of the Arab Revolt, the author raises the issue of the relationship of Arab nationalism to the Revolt and finds an indirect rather than an organic connection. Husayn ibn Ali, Sharif of Mecca, is convincingly portrayed as an artful dynastic opportunist for whom Arabism and a British alliance were seen to serve the same ends as Ottomanism previously had—the preservation of his freedom of action in the Hijaz at the expense of his Arab rivals. In Dawn's opinion, the Hashimite leadership did not develop ideological support for these actions through the use of Arabism as a consciously revolutionary ideology. Rather, both Husayn and his son Abdullah, on whose memoirs Dawn relies heavily, justified Arab rebellion out of a concern for Islamic reform and revival.

Outside the Arab peninsula, the development of an ideology of Arabism among more sophisticated theorists took a similar course. Dawn argues that a sense of ethnic nationalism emerged as a result of efforts to overcome the gap between East and West through the revitalization of the Ottoman Empire and the Islamic civilization of which it was the guardian. Paradoxically, however, thinkers like Abduh and al-Kawakibi, in seeking the original dynamic of Islam, could not but emphasize the Arab element to the exclusion of others. As a result, "the theory of Arab nationalism thus grew out of the modernist diagnosis of Moslem decline and prescription for Moslem revival" (p. 140).

If the Hashimite leadership was motivated by dynastic considerations while Islamic concerns dominated the thoughts of Arab Ottomanist reformers, who were the Arab nationalists before 1914? In a densely packed essay, "The Rise of Arabism in Syria," Dawn explores this question of participants. He posits social attributes as indices of Arab separatist versus pro-Ottoman behavior in the years before 1914. Through a comparative, and heavily qualified, use of biographical data, the author eliminates as determinative the variables of age, education, and class. Instead, he finds that the holding of government office was the crucial factor determining political orientation—those Syrian Arabs with state positions tended to be pro-Ottoman while those without them, even if wealthy individuals, favored Arab nationalism. Dawn feels that what was essentially a traditional intra-elite conflict thus took on an added dimension when defined in terms of the new ideology of Arabism.

Given what the author says about the origins of the Arab nationalist movement, it is not surprising to find him asserting, in a challenging concluding essay, that Arab economic and social structures have experienced fewer disruptions in the years following World War I than other scholars have maintained. In particular, the behavioral scientists are chided for their claims to have discovered emerging classes. Dawn's interpretation provides a needed corrective to the historical periodization which tends to see all Ottoman influences among the Arabs terminated by the collapse of the Ottoman state and the creation of separate Arab polities. He shows clearly that the postwar Arab political leaders were mainly individuals who had supported, because they served, the Ottoman Empire during the War. Implicit in this analysis is a recognition of the lack of revolutionary legitimacy of the new Arab states. Nevertheless, one would have liked to have seen more supportive evidence for certain of the concluding generalizations, especially those on the limited impact of urbanization and the upper-class origins of most Arab army officers.

These essays, the richness of which can only be suggested here, constitute a truly significant contribution to the study of Arab nationalism. While the bulk of them has appeared previously, they retain their freshness and their collection in book form is most welcome. In his skillful use of sources and his carefully constructed argumentation, Dawn has produced essays which are not only important studies on the origins of Arabism, but also models of historical analysis.

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**Serfs, Peasants and Socialists: A Former Serf Village in the Republic of Guinea.** By William Derman. (Berkeley: University of California Press, 1973. Pp. 304. \$12.00.)

Life for the average citizen of the Republic of Guinea has undergone profound—and even traumatic—changes since Guinea's historic "No" vote in the referendum of September 28, 1958. That vote made Guinea the first of the former French territories in Black Africa to reject membership in the nascent French Community. It made Sékou Touré, Guinea's volatile young president, a hero to much of the Third World and its sympathizers, and led to the collapse of General de Gaulle's grand design for a new relationship between metropolitan France and its overseas territories.

At the time Guinea embarked on its lonely and courageous adventure as an independent state, hopes were high in many quarters of the

world for the success of the new nation. Virtually everyone in the American academic community then writing about this small country in West Africa readily acknowledged that Guinea's importance as a political entity far exceeded its relatively modest size (97,000 square miles; 3 million population in 1959). Its principal leaders at the time—Sékou Touré, head of the *Parti Démocratique de Guinée* (PDG), and his two principal opponents, Barry Ibrahima (called Barry III), former head of the Guinean branch of the *Mouvement Socialiste Africain* (MSA), and Barry Diawadou, leader of the *Bloc Africain de Guinée* (BAG)—appeared willing to subordinate their differences to the common good and to work through Touré's PDG which had won a triumphant victory at the polls in the territorial elections of 1957.

Guinea, at the time it became independent, excited the imagination, for it held forth the promise of being a place where bold and innovative programs would be tested for the social and economic advancement not only of its own citizens but also of Africans everywhere. This new order in Guinea proclaimed itself proudly and defiantly as a society that would be both more humane and more just than any other—precolonial or colonial—which until then had held sway in the country.

Unfortunately—and for the people of Guinea, tragically—that promise was never fulfilled. Today, seventeen years after it gained its independence amid such hope and fanfare, Guinea remains what it has progressively become under Sékou Touré's ruthless dictatorship: a land of economic stagnation, political turmoil, and personal terror—a kind of West African Haiti.

It would be unfair to evaluate William Derman's book *Serfs, Peasants and Socialists: A Former Serf Village in the Republic of Guinea*, by the criteria used by political scientists, for Mr. Derman is an anthropologist. His book, based upon a doctoral dissertation prepared for the University of Michigan, is pre-eminently an anthropological study of the changes that have affected the social organization, economic life, and life style of a Fulbe serf village in the Fouta Djallon region of Guinea. Insofar as Mr. Derman deals with anthropological data, the book provides a wealth of carefully researched and well-documented detail concerning such topics as the social and economic relations under the impact of French colonial rule and, subsequently, of the new national government.

The book moves along smoothly for the first two-thirds, but begins to falter when it reaches the section dealing with ideology. Judging from

the word "Socialist" in its title, one anticipates a detailed discussion of the nature and impact of the PDG's socialist ideology on the Fulbe and on their attitude toward their government, not merely an anthropological survey of such phenomena as life-cycle ceremonies, sorcery, and the nature of Islamic tradition in Fulbe society.

What we do not get from Mr. Derman's book is a true sense of the direction that social, economic, and political changes have taken in Guinea under the policies (ambitious but often ill-conceived) formulated and put into effect by Sékou Touré and the PDG. From a work that aspires to provide insights into the effects of socialist ideology on the serf and peasant classes of Guinea, the reader has a right to expect a discussion of some of the more critical developments that have occurred in Guinea during the period under examination: for example, the result of the intensive politicization that has taken place under Touré's leadership and the control of the country by the PDG. One would have liked to get some idea of what the problems have been between Touré and the Soussou-Malinké constituency (which he originally represented) and the Fulbe who were their traditional enemies and, significantly, the largest ethnic group in Guinea. Mr. Derman, regrettably, provides us with very little information on any of these topics. In brief, what the book lacks is a sustained attempt to relate the social and political implications of the microcosm of a serf village to the macrocosm that is the rest of the country.

One senses, instead, that in these pages there is a conscious effort to avoid at all costs broaching any but the most innocuous subjects: those that will not risk giving offense to the authorities back in Conakry. One cannot but be understanding of the author's timidity, for Guinea is one of the most difficult countries in which to do research. Government officials, particularly President Sékou Touré, resentful of criticism of their régime, have for years placed extraordinary obstacles in the way of anyone wishing to conduct research in their country, especially Westerners. If a researcher does not abstain totally from making any value judgments not in keeping with the accepted views of the party leadership, he runs the risks of being denied access to essential sources of information, and even of being expelled from the country.

Unless there is a fundamental change in the present xenophobia on the part of Guinea's leaders (a development unlikely to occur until there is a change in régime), the best that the academic community can probably expect is additional methodical but nonprovocative stud-

ies like the volume under review. The really important things that are happening in Guinea today, the tumultuous events that have wrought dramatic and far-reaching changes of profound significance to that society and to the large polity which is Africa, are part of a story that still remains to be told.

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**The Pattern of the Chinese Past.** By Mark Elvin. (London: Eyre Methuen; and Stanford: Stanford University Press, 1973. Pp 346. \$12.50.)

Historical science may progress in two ways which are complementary: by deeply probing monographs or by broad syntheses which provide a general interpretation for long-term developments. Mark Elvin has daringly chosen the second path and has succeeded in following it with flawless logic, clarity and brilliance, which win the reader's admiration, if not always his conviction. Professor Elvin's ambition is to offer a general view of the evolution of Chinese economy and society from the third century B.C. through the nineteenth century A.D., a view "into which studies on particular aspects of Chinese history can be fitted, and from the partial modification of which their findings should gain their deepest significance" (p. 319). The changing pattern of the economics of technology is made the central point of reference for reasons of "practical convenience" (p. 317), because it emphasizes the interrelationship between politics, warfare, economics, institutions and ideas, and helps the solution of some basic historical problems.

The book gives equal consideration to three fundamental questions, connected both logically and chronologically. First, the explanation for the size and survival of a united Chinese Empire is found in the ability of the Chinese to keep ahead of their neighbors in the main technical skills—military, economic and organizational. Second, the change of technological capacity itself—what Elvin calls the "medieval economic revolution"—resulted from the combination of progress in farming productivity, organization of water transport, money and credit, market structure and large-scale urbanization, science and technology. Third, if on the basis of the prosperity and high level of economic organization attained by the early fourteenth century, China failed to develop a socioeconomic system like that of European capitalism, capable of producing a more rapid, sustained growth rate of per-capita product such as Western nations achieved after the eighteenth century, the blame, contrary to

the common view, is not to be placed on the shortage of capital, the smallness of the market, or political, bureaucratic, or cultural obstacles. In fact, from 1500 to 1800 China did experience a renewal of vigorous economic growth, but almost no technological invention. The reasons for this qualitative standstill are complex. They include a paradoxical "high-level equilibrium trap," whereby traditional means of farming, water transport, and market organization were so perfected as to constitute an obstacle to the expansion of per-capita demand and thus discourage investment of time and money in technological improvements.

The bulk of the evidence brought forward by Elvin to sustain his argument relies primarily on studies by Japanese scholars. It is mostly from their works that the quotations from Chinese documents which enrich the book are borrowed. Unfortunately, the author often forgets to mention the date and original location of those quotations, as well as the sources from which he compiled his otherwise useful maps, and to provide a systematic list of the bibliography referred to in his notes.

However perceptive and challenging, such a bold survey of a two-thousand-year history is bound to have its limitations and shortcomings. One could point out a few hasty statements or generalizations, such as taking as proof of the reappearance of Chinese creativity "in the interior" (p. 315) the development in the 1870s of the Kwangtung silk industry, which was in fact located in the districts around Canton on the Pearl River delta. Of greater concern are some incomplete, disputable analyses, such as the case Elvin makes, with special emphasis (pp. 235-255 and 318), for the widespread survival of manor-like estates and serf-like landlord-tenant relationships until the late sixteenth century, the disappearance of serfdom being thus held as decisive in the demographic upsurge, the increased agricultural productivity, and the new type of power structure characteristic of the eighteenth century. But even this type of hypothesis may help to foster further research. Indeed, the whole book shows the need for more monographic studies before definite judgment can be made. If it is therefore likely to elicit heated controversy among sinologists, its great interest to the political scientist is that it breaks through a number of prejudices and false truths, raises major issues, and provides a framework not only for the understanding of China's past, but also for her present problems of economic development, and for comparative analysis with European history. Finally, it is rather the very nature of this framework which may call for the most serious

reservations. The striking absence of reference to Chinese historiography and of direct discussion of Marxist interpretations (though a number of them are in fact dispelled), as well as the insistence on the beneficial effects of China's opening to the world market as a consequence of the Opium War leave one with the impression that it is not merely for "practical convenience" that the author focussed on the economics of technology, but that this approach is also borne out by definite assumptions about historical causation.

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**Radical Politics in West Bengal.** By Marcus F. Franda. (Cambridge, Mass.: MIT Press, 1971. Pp. xiv, 287. \$12.00.)

This book has been in search of an author for a long time. It analyzes the complex and rich culture of communism in West Bengal and attempts to answer the following questions (pp. 3-4): What are the sources of communism in West Bengal? Why have Bengalis been so active in the Communist movement while other Indians have for the most part sought alternative modes of political expression and participation? What are the Bengali Communists trying to accomplish, and how do they go about it? What has been the impact of the Bengali Communist movement on state and national politics, and on the Communists themselves?

Professor Franda's answers are difficult to summarize, but the following are his main points: First, Bengalis have adopted Marxism and communism to express their search for a regional identity and to have access to regional power. In doing so, they have drawn upon the traditions of their long struggle for autonomy from the central authorities at Delhi during the pre-British and British periods. Second, the Indian federal structure has stoked the psychology of discrimination, and the Communists have exploited the sense of defeat of the Bengali gentlemanly classes, the *bhadraloks*, who feel that they have lost to the Hindi-speaking Gangetic heartland of Hinduism. Third, Marxism came to Bengal mainly as a package of ideas, and naturally its appeal is primarily to the educated, upper caste gentry, who dominate the Communist movement. Indeed, a logical extension of this elitist radicalism is the slogan of the Bengali Maoists that mass work and organizational effort lead to revisionism and economism. Fourth, communism has not been creative in West Bengal and is subservient to the Communist movements outside India and their slightly jaded chairmen. This weakness is

mainly due to the Bengali Communist's acute sense of incompetence vis-à-vis the Indian environment, so that he uses as an escape the unending debates and ideological battles within the international structure of communism. However, he *has* sometimes successfully exploited the dissensions within international communism for his own factional purposes. Fifth, in the affairs of West Bengal, the Communists have shown some initiative, but they are fettered by their inability to spill over the boundaries of West Bengal into the nearby states and by their internal factionalism.

These are important points, and the author supports them with a wealth of facts which, in spite of occasional slips, have been carefully marshaled and systematized. Particularly good are the discussions of Communist factionalism and the sociopolitical differences among the pro-Soviet Communist Party of India (CPI), the middle-of-the-road CPI (Marxist) and the pro-China CPI (Marxist-Leninist). These strengths make the book a valuable monograph on Indian radicalism.

Unfortunately the author has been badly let down by time (the book was written before the Bangladesh war in 1971) and the history of American scholarship (the book tends to over-correct the earlier anti-Communist interpretations of Communist movements in India). Thus, he often overestimates the Bengali radicals, particularly the Maoists (e.g., pp. 176–181, 266–267). Given the small geographical spread and support base of the CPI(ML) even in its heyday, the speed with which it virtually vanished from the face of the state within a period of eighteen months, and the manner in which a large number of its activists joined the Congress youth groups in 1971–72 (the Maoist slogans were often left unaltered, and only the name of the redoubtable chairman was replaced by India's own materfamilias), one would have liked to see some analysis of the psychology of party allegiance in the *lumpen* elements who constituted the Maoist street gangs of greater Calcutta. On the other hand Mr. Franda, writing before the 1971 war, could not imagine that the Maoists would make a mess of the Bengalis' traditional struggle for autonomy from external authorities and support the West Pakistani regime against the Bangladeshis, that the CPI(M) would equate the West Pakistan government with the New Delhi government, or that the flat-footed Congress government at the Centre would suddenly swing into action to help create a Bengali nation next door (risking confrontation with China and inculcating in the *bhadraloks* a new sense of national

pride and a new hostility toward the pro-China elements in India). Similarly, the author's analysis of the different possible coalitions in Parliament seems to assume that the ruling party would naturally move toward a coalition with the rightists whereas the Communist parties would ultimately work with each other and the socialists (pp. 255–258). Yet, already India's major Communist party, the CPI, has become an ornamental adjunct of the ruling party; the CPI(M) has been cornered as a political force, and the CPI(ML) has not only been forced to recognize India's "highly institutionalized military, police, judicial and bureaucratic network" (p. 257), but also the Bengali allegiance to regime norms which, some surveys show, is among the strongest in India. Evidently, in spite of its creaking wheels, the bullock cart called the Indian political system has once again shown its capacity to negotiate dissent and to falter toward a coalition of the hopes and aspirations of diverse sections of the Indian people, including West Bengal.

Actually, Mr. Franda has missed three enduring elements in the *bhadralok* identity which give Bengali communism much of its distinctiveness, though each of these elements is reflected in his own data. First, there is the deepseated *bhadralok* ambivalence toward all authority. Historically this has taken the community through cycles of total opposition and total submission. Bengal has always been peripheral to the greater Sanskrit culture; yet it is the place where often a new search for India's traditional self is mounted and where learning has the deepest Brahminic connotations. Similarly, during the British period, the *bhadraloks* were sometimes the most earnest supporters of the *raj*; at other times its most dedicated opponents. In post-Independence India too, no other group endows the concept of Indianness with such importance (after all, it was in nineteenth-century Bengal that the political concept of Indianness was first forged) and attaches such sanctity to systemic norms. Yet no other state has been a greater headache to the central authorities.

Second, there is the search for Brahminic purity by a community which, Mr. Franda admits, has always been somewhat peripheral to the heartland of Brahminic Hinduism. Thus, pure radicalism remains the ultimate goal of the various factions within factions, and the cruelest blood-feuds take place among those who are ideologically closest and threaten each other's purity. Of the persons killed by the CPI(ML), the largest number belonged not to the police or to the Congress party but to the

CPI(M); the number of military personnel and big businessmen killed in the "revolution" was exactly zero.

Third, Marxism being essentially an intellectual movement in Bengal, its utopian elements have been seized upon by the Bengali *bhadraloks*, so that all active intervention in the real world of persons and events, all micro-level organizational and mobilizational work are seen as detracting from the so-called "fundamental structural changes." It is this apocalyptic romance of a distant revolution which never comes, combined with a total lack of contact with those plebeian elements in whose name unfortunately revolutions have to be fought, that has powered the Marxism of the westernized *bhadraloks* and comprador literati of Bengal.

All this may sound dangerously psychologicistic. But if Mr. Franda had scanned his data more carefully, he would have seen the clay feet of Bengali communism and foreseen its present plight. It neither has the strength of the Communist movements in some other parts of India nor has it contributed an iota to Marxian theory in spite of having within its ranks some of the finest minds of India. Thus, Mr. Franda could have written this book as a minor tragedy. He has preferred to write it as a long footnote. Perhaps that is what makes it a scientific study of politics.

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**Lumpenbourgeoisie and Lumpendevelopment: Dependency, Class and Politics in Latin America.** By Andre Gunder Frank. (New York: Monthly Review Press, 1972. Pp. 151. \$6.00.)

In this volume Andre Gunder Frank attempts to answer some of the criticisms leveled at his influential *Capitalism and Underdevelopment in Latin America* by Latin scholars. While praising Frank for his important contributions to a critical re-examination of Latin American development, and agreeing with him that underdevelopment is in part due to a long history of exploitation by the centers of world capitalism, his detractors felt that Frank overemphasized the importance of center-periphery ties. They argued that his focus on the drain of economic surplus from satellite countries to the metropolis was excessively static and should have included a more sophisticated analysis of the changing class structure of individual countries.

Frank argues that his earlier work was misunderstood, and that he does subscribe to the

view that underdevelopment is as much due to internal patterns of class conflict as it is to patterns of dependence. His thesis is that Latin American underdevelopment is "the product of a bourgeois policy formulated in response to class interest and class structure, which are in turn determined by the dependence of the Latin American satellite on the colonialist, imperialist metropolis" (p. 1). He notes that as patterns of dependence changed over the centuries, transformations took place in the culture, economy and class structure of Latin American societies. These shifts, however, did not lead to a weakening of the dominant classes, who simply responded to change by creating new policies designed to aggravate Latin America's dependence and underdevelopment. Frank proposes to trace this phenomenon by analyzing economic dependence, class structure, and the resulting policy of underdevelopment in each of a series of chapters dealing with the Colonial Structure, the Agrarian Structure, Independence, the Civil Wars, Nationalism vs. Free Trade, the Liberal Reform, Imperialism, Bourgeois Nationalism, and Contemporary Neo-imperialism.

I agree with Frank and others that an analysis of Latin American underdevelopment focusing, at least in part, on relations of dependence is more viable than explanations which attribute underdevelopment to "feudal structures" or to simplistic cultural and psychological variables. *Lumpenbourgeoisie and Lumpendevelopment*, however, is much too sketchy to buttress both empirically and conceptually a theory of Latin American underdevelopment. Several chapters are so short that they should not be major subdivisions of a book. (The chapter on Independence has only six pages, and the chapter on Liberal Reform has four.) Thus, documentation leaves much to be desired, and the author aggravates the problem by relying too heavily on extensive but highly selective quotes for his main arguments. Often these quotations raise more questions than answers.

As a result, Frank's analysis is not fully convincing. For example, it is not clear why changes in relations of dependence necessarily lead to policy changes which contribute to further dependence and underdevelopment. Frank argues categorically against the possibility that a nationalist group can emerge and cut off dependent ties. Yet, his model does not provide an adequate explanation for why nationalist groups emerged in the first place or why they were defeated. He provides no developed analysis of intra-elite clashes and little

evidence on the sources of power of different groups. To say that the 19th-century bourgeoisie tied to the export sector won because it had greater strength in colonial times is begging the question. In refuting the contention that a nationalist elite is a possibility today, Frank does not use a theoretical argument, but simply refers to the failures of the military junta in Peru as sufficient evidence to buttress his thesis. Equally problematic is Frank's statement, without proof, that the only solution to underdevelopment is a violent revolution and socialism. It is never clear whether socialism is bound to emerge out of the contradictions in Latin American society—or whether it is just a conviction which the author shares.

The principal difficulty with Frank's analysis is that he has not fully overcome the static nature of his earlier efforts. He continues merely to *combine* an analysis of colonial relations with a class analysis lacking a dynamic or dialectical component that would be capable of sustaining theoretically his argument for the inevitability of the phenomena he describes.

In part this difficulty is due to a continued lack of definitional precision. Dependence means underdevelopment, and yet neither term is adequately explained. Thus, Frank does not distinguish conceptually between different levels or types of development and dependence. At one point he argues that the "Canadian Model" represents an advanced form of integration with imperialism which accelerates further underdevelopment (p. 122). Does this mean Canada is more underdeveloped than Nicaragua? Since he defined underdevelopment or lumpenddevelopment as "wretched backwardness" (p. 5), his analysis would benefit by clarifying what "wretched backwardness" means. In conceptual terms, Frank is close to falling into a tautological trap by saying, on the one hand, that dependence causes underdevelopment; and, on the other, that underdevelopment is identified by the presence of dependence. In this reviewer's estimation, scholars such as Fernando Enrique Cardozo, Theotonio Dos Santos and Osvaldo Sunkel have more successfully overcome these difficulties.

ARTURO VALENZUELA

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**Bureaucracy and Development: A Mexican Case Study.** By Martin Harry Greenberg. (Lexington, Mass.: Heath Lexington Books, 1970. Pp. 158. \$12.50.)

This tightly written volume is the first empirical study in English of a major department of the Mexican government. It is a skillful blend of comparative administration theory and

administrative practice, providing a detailed examination of the Ministry of Hydraulic Resources (SRH), the agency responsible for development and utilization of Mexican water resources.

The Ministry is of interest because of its peculiar blend of modernism and traditionalism. It is modern because it utilizes modern technologies (civil engineering and economics) in its decision making and operational processes, and has developed for this purpose what the author calls a "quasi-merit system." But it is traditional because jobs and advancement are secured primarily through personal, party, and family relationships. Moreover, the really important decisions (i.e., policy or non-technical) are made on the basis of partisan-political considerations. The organization is shot through with graft and corruption.

To elaborate, such questions as how to build a dam, how to construct an irrigation district, what their structural characteristics will be, what are the reasonable costs and benefits (in the technical sense): these are technical questions and are left to the engineers and economists. *Where*, the dam, irrigation district, or sewer system is to be located, *who* is to be benefited, *how much* graft is allowed, *who* gets it: these questions are of a "policy" or a "political" nature. They involve the highest Ministry officials, party representatives, and members of the Office of the Ministry of the President.

Two parallel structures exist in the ministry, each dealing separately with the technical and the policy realms. Where the scheme gets interesting, and where the peculiar characteristics of the transitional regime emerge, is in the interaction of party, ministry, and technical officials. The patterns of organizational behaviors and ideologies they adopt are designed to prevent undue self-aggrandizement of the more modern sector at the expense of the more traditional.

The party is ever-present in bureaucratic operations. The PRI, Mexico's mass party in what is essentially a one-party system, represents all the most important interest groups and carries forth the "revolutionary tradition." There is no preferment in Mexican political or bureaucratic life not sanctioned by the party. The party provides not only strong political leadership but also the active coordination of bureaucratic entities. This is an element often found lacking in other transitional polities.

Professor Greenberg warns that the very strength of the PRI makes it difficult for the analyst to distinguish political from bureau-



ratic functions, as well as the political from the economic. For example, bureaucrats are required to take part in partisan political activities. Politicians make important bureaucratic decisions. Not only that; there is a growing class of "bureaucratic capitalists." They use their government positions as a "base" and take advantage of "technical consultantships" widely available to bureaucratic elites in business enterprise. A whole new class of "bureaucratic capitalists" has grown up, causing conflicts of interests and further confusion of roles. Greenberg attributes this blurring of institutional roles to "intense overlapping"—utilizing to good effect a Riggsian term, with which the book is liberally sprinkled.

The country has been saved from an out-and-out authoritarian regime by the maneuverability, flexibility, and responsiveness of the PRI. Here Greenberg refers to the vaunted capability of the party to co-opt almost every dissident group within society. This co-option capability is an important factor in keeping the potentially explosive Mexican society together. The memory of the violence of the "revolution" is still alive. It is a factor in the party's favor. But it is based on a system of interlocking interest groups and numerous veto powers, exacting a high price in forward movement. It rewards political power with economic benefits. It leaves the politically unorganized and the economically weak almost out of the system altogether.

In the Ministry, potential for conflict between the technical branches and the policy-oriented branches is serious. It only occasionally breaks out in the open. Greenberg alludes to a continuing battle between those who favored the establishment of large irrigation projects, and those who demanded a higher priority for small irrigation (small farmers). The proponents of the latter consistently lost. "Sound economic reasons favored the greater emphasis on large irrigation, but another factor was the need for large scale projects for political purposes to enable the PRI to offer visible proof of its accomplishments" (p. 80). Also, one can surmise, large-scale projects offer more opportunities for patronage and corruption.

Another revealing comment illustrates how the rates of water users are set. The Ministry is supposed to take into account the ability to pay. This principle is unevenly applied, depending upon the user's political influence. Granted that this is not fair "in the western sense of the word" Greenberg explains that "in the Revolutionary context, some 'political' considerations often mean watching out for the interests of groups which (if neglected) could

topple the fragile pattern of accord and compromise." The system, he says, benefits everyone in the society, even if some pay more than their "fair share" (p. 81).

In the name of revolutionary justice, Mexico gets institutionalized selfishness. This tendency has resulted, many believe, in a very highly skewed income distribution in Mexico. The rich grow richer. One wonders whether a more highly competitive political system (i.e., two or three competitive parties) might benefit larger segments of the Mexican people.

CLARENCE E. THURBER

*University of Oregon*

**Quisling: The Career and Political Ideas of Vidkun Quisling, 1887–1945.** By Paul M. Hayes. (Bloomington: Indiana University Press, 1972. \$12.95.)

To write the biography of *Vidkun Quisling* is a fascinating task. The biographer encounters, however, serious problems connected with choice of methods and analytic focus.

Dr. Hayes employs a traditional historical approach. He presents a broad description of Quisling's life, his childhood and humanitarian activities, his turning to politics and a cabinet position, the launching of a party, contact with Germany before and during the occupation of Norway, his trial, sentence, and execution. Having delved diligently into a rich material, the author presents an interesting exposition of an extraordinary politician.

Still, the book remains unsatisfactory for several reasons. The author seems to be unaware of the general problems of political biography. (See for instance L. J. Edinger, *Kurt Schumacher* [Stanford: Stanford University Press, 1965] Chap. I, and Fred Greenstein, *Personality and Politics* [Chicago: Markham, 1969]). As a minimum, a conscious balance must be maintained among the choice of focus, of data, and of analytic method. Not only must the biographer employ a fair distribution of emphasis between "the man" and "the time"; he must also be aware of the complex borderlines between the "hero's" behavior, his inner motives and outer justifications; furthermore, his intended influence and actual role must be kept strictly apart. Problems of this kind are implicit in biographical analysis, but the author does not come to grips with them explicitly. Instead of offering a methodological clarification and arguments for his own line of approach, he limits himself "to allow Quisling's actions and words to speak for themselves" (p. 10).

In spite of—or rather because of—a detailed presentation of both "action and words," there

emerges no clearly marked profile of Quisling, the man and the politician. Only a few pages are devoted to his ideas, thus forfeiting the promise given in the book's title. Ideological aspects are neither integrated with the historical narrative nor used to illuminate behavior. Quisling's lack of political realism and his unbounded faith in his own ability are, of course, mentioned (chap. 9), but not used as a key to an understanding of essential phases in his career.

The historical description is too broad, thus sometimes making Quisling too important, sometimes letting him almost disappear among the several actors mentioned. This lack of a clear theme is essential. From this point of view a series of factual blunders—arising from a rather uncritical interpretation of basic sources—remain less important. In spite of such weaknesses the author might have provided a well-balanced picture of Quisling had he attacked the problems more systematically.

It is to the author's credit that he took upon himself the task of giving us a scholarly biography of Quisling, long overdue. It is regrettable, however, that he has not responded fully to the challenge, although he has presented a very broad narrative and many keen observations.

The really absorbing problem presented by Quisling has more to do with treason as such than with historical details. In a century of conflicting loyalties the legal aspect of treason is to this reviewer less interesting than its moral and political implications, encompassing the human dilemma of an era.

Quisling was decidedly "more muddled than corrupted" (p. 317), and I have no argument with Dr. Hayes when he finds it clearly unfair to let Quisling's name remain a symbol of the notorious traitor. There were elements in him of the "true believer," an attitude which flagrantly contradicts the basic democratic demand for loyalty to a majority. His career leads us to ask under what circumstances, social and individual, does a political conviction turn into open disloyalty, to what and to whom?

THOMAS CHR. WYLLER

*University of Oslo, Norway*

**Social Change in Angola.** Edited by Franz-Wilhelm Heimer. (Munich: Arnold-Bergstraesser-Institut, Weltforum Verlag, 1973. Pp. 284.

This collection represents the first serious empirical studies of Angola. Heimer's volume includes sophisticated studies by Portuguese and non-Portuguese who through official sup-

port or toleration of their efforts were able to undertake field investigation.

The methods of the contributors vary. Joseph C. Miller's study of 19th-century slavery in Kasanje, and Douglas Wheeler and Diane Christensen's account of the 1902 Bailundo War are descriptively and analytically historical in approach. An article by Hermann Pössinger examines the historical, social, and economic evolution of the Ovimbundu peoples and elaborates on a thesis that Portuguese commercial and administrative penetration into the central highlands has resulted in a general decline of African society. Other studies are behavioral, relying strongly on survey technique. These include the editor's own contribution on change in the Cuíma region, as well as the reports by Elisete Marques da Silva and Ramiro Ladeiro Monteiro, who respectively focus on school children in suburban Luanda and family life in the *mucueques* of the capital city. Finally, there are studies which incorporate a technical-scientific perspective. For example, Jorge Vieira da Silva and Júlio Artur de Moraes make use of general ecosystems theory in a study of the Ovimbundu; while Vieira da Silva and Eduardo Cruz de Carvalho use a similar approach in presenting a mosaic of data and charts on the Cunene region. Gerald Bender systematically examines the records of the Angolan Provincial Settlement Board from 1961 to 1968 to give empirical support to his finding that the Portuguese rural settlement scheme in Angola never achieved its objectives.

Some of the essays deal with rural Angola. Miller offers a detailed analysis of how the slave trade was carried on within Angola a century ago. He traces the transformation of Imbangala society from a relatively large and cohesive lineage to a dispersal of power throughout the Kasanje state, the consequence of internal response to outside pressures for more slaves. Such an internal-external relationship was also evident among the Ovimbundu. The discussion by Wheeler and Christensen is a good synthesis of generally available (though not necessarily in English) material and information. They look at the Bailundo kingdom, the largest of some 22 kingdoms in the central highlands, and note the early trading patterns, the internal organization of African trade, kinship, and policy with neighboring African tribes. In a postscript, they focus on Portuguese interference with the African control of the caravan trade from central Angola to the coast.

Their essay serves as background material and therefore complements Pössinger's analysis which examines the early 20th-century decline of the Ovimbundu. He notes the creation of a

capitalist economy, the role of women and men, the shift to a cash crop agriculture and the need for new land, the changes in the local hierarchy through the weakening of the power of the local chiefs, the Portuguese advance into the highlands and the cooptation of new and old chiefs with the colonial administrators, the relinquishing of judicial power to colonial authority, the influence of the Christian missionary schools, and the modifications in family structure.

The essay by Vieira da Silva and Morais goes another step further in this analysis. They turn to the decay of Ovimbundu political economy in the Huambo district. They show that changing economic patterns in the area relate closely to developments elsewhere, especially in the cities. They affirm a capitalist development of underdevelopment, such as suggested by Walter Rodney for Africa and André Gunder Frank and others for Latin America.

Heimer also directs attention to rural Angola as does Bender, and both studies relate to European influence in the colony. Elsewhere Heimer has revealed the massive data from his remarkable survey of education and society in rural Angola. The present essay is drawn from these data. His survey was undertaken in 20 villages of Cuima among 114 heads of families. Let us hope that in some future essay, Heimer can carry his discussion to an in-depth analysis which probes the implications of his findings and hypothesis. In the present study his description of methods and results does not reveal the significance of his findings.

Bender's synthesis of planned rural settlements in Angola subtly but devastatingly reveals the failure of Portuguese objectives: to secure Portuguese sovereignty and civilization in the rural areas. Instead of ensuring the Portuguese hold upon the rural areas, most settlers moved to the cities after an initial stay in the countryside. The Portuguese goals to raise agricultural production and the cultural and economic level of Africans failed. Land conceded to Europeans in recent years has increased while that to the Africans has decreased, the impact being "catastrophic" on Africans. Over \$100 million was spent on the planned rural settlements during the past two decades, yet only 840 European settlers remain in the rural areas, this representing about one percent of the entire European population in Angola!

The essay by Cruz de Carvalho and Vieira da Silva also looks at European influence with attention to southwestern Angola where European ranching has intruded into the traditional agro-pastoral structure of the region. They

show how government plans in the area will lead to a reduction of pastoral use, and they express doubts that any significant agricultural production will take place. Their very technical and detailed study demonstrates the impact of outside private and public capital upon the potential underdevelopment of a region, thereby lending credence to a thesis which seems to emanate from other essays mentioned above.

Finally, the essays by Elisete Marques da Silva and Ramiro Ladeiro Monteiro focus on urban Angola. The former offers a "preliminary test of some hypotheses" and attempts to assess the impact of education upon African families in three *muceques* (slums) and two *bairros populares* (popular districts) of Luanda. The author's data are drawn from the Heimer survey and consist of a series of tables and cross-tabulations as well as some hypotheses which either are not confirmed or only weakly so. Monteiro also looks at *muceque* life of Luanda and his data, from a survey in 1971, are accompanied by description and analysis. His is the most readable and clearly stated of the behavioral essays in this volume as he looks into the traditional matrilinear system, paternal lines, the extended and nuclear family. One comes away with the sense that these two essays may well serve the Portuguese cause in Angola. Both probe into African behavior and life style and constitute an empirical basis for continued Portuguese manipulation of the African populations.

RONALD H. CHILCOTE

*University of California, Riverside*

**India's Revolution: Gandhi and the Quit India Movement.** By Francis G. Hutchins. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 326. \$14.00.)

The subject of this short but expensive volume should interest political scientists, especially those concerned with typologies of revolution. Dr. Hutchins has drawn from untapped sources, showing the strategy of the Indian revolutionary movement as well as of the British government in India. The total picture that emerges is coherent if controversial, and is highly relevant to general theories of revolution. About one-fifth of the book portrays Gandhi as a revolutionary leader, but its chief contribution is the suggestive comparison it makes between the American and the Indian revolutions.

Chapters two and three illustrate the many facets of the imperialist dilemma, mainly from the standpoint of political sociology. Imperialism is depicted as a social and political system as well as an ideology, supported by vested

interests and supporting in turn institutions related to its perpetuation. Even benevolent intentions had pernicious consequences. The attempt to introduce a system of representation based on classes and interests merely resulted in the development of a radical political consciousness on factional lines. The infrastructure created by the imperialists became a continuing obstacle to postrevolutionary development.

Chapter four presents the nationalist dilemma in the context of cultural and social history. This chapter is a feat of compression, raising more questions than it can answer. Just as the French Revolution had a complex intellectual lineage in the different strands of the French Enlightenment, the Indian Revolution had its roots in an intellectual soil enriched by a wide-ranging interchange of ideas and converging cultural movements over an extended period. The author perceives three phases—the manipulative stage, involving religious or cultural conversion and collaboration with the regime; the phase of cultural assimilation, dependent upon foreign sponsorship; and the phase of rebellion, in which traditionalists seek to transform their cultural reaction into a political rebellion.

In Chapter five, we have an explicit recognition that revolutions are "intellectual events," involving perceptions, decisions, and acts of will. Predominantly plan-fulfilling revolutions are then contrasted with those that are essentially self-fulfilling. While it is meaningful to claim that a self-fulfilling revolution accomplishes more than it intends, the emphasis upon "accidental" factors could be questioned by Marxists as well as by Hegelians or Platonists. The author contends that India's leaders, like America's Founding Fathers, failed to perceive the extent of the transformation which the revolution had wrought. According to Francis Hutchins, every revolution is a "revolution of saints" (p. 85), and the line separating the reactionary from the revolutionary is thin. This thesis is applied to Gandhi. "As Gandhi's political tactics were drawn from those of political reactionaries—the extremists—so were his philosophical concepts modified versions of ideas first elaborated by philosophical reactionaries" (p. 91). Is the author committing the genetic fallacy both in regard to historical events and political ideas?

Chapter six deals with Gandhi as a revolutionary leader, the key to whose accomplishment lay in his rational modifications of tradition. He is portrayed not only as a charismatic leader "rooted in tradition and motivated by rationality" (p. 117), but also as an entrepre-

neur, amassing reserves, winning followers, and then creatively applying his resources to new purposes. His style and tactics are stressed, and the author endorses Erikson's claim that Gandhi's nonviolence had much in common with Freudian psychotherapy, i.e., that "Gandhi developed for the relations between social groups, many of the same insights Freud applied to private relationships (p. 127). When the book ventures into psychological analysis, it is less plausible than when it is content to invoke current classifications of social structures.

The next three chapters embody the fruits of much factual research. There is a convincing account of the logic and the limitations of the policies of the Viceroy, and of his disagreements with the Secretary of State. In elaborating the strategy behind the Quit India campaign, the author infers from Gandhi's dialectically confusing statements that he was conniving at the violence that accompanied his nonviolent struggle. The central thesis concerning "Spontaneous Revolution" is supported by references to the roles and views of underground militants. The final chapter comments on the decline in revolutionary fervor after independence, and concludes that Gandhi's disillusionment meant that he was working out a destiny which he had known was inevitable.

Francis Hutchins is happy to invoke King Lear repeatedly, in reference to the British Government in India as well as to Mahatma Gandhi in the last phase. This thought-provoking book is itself more suggestive of *Hamlet* than of *King Lear*, illustrating the insight that "the readiness is all" rather than the realization that "Ripeness is all."

RAGHAVAN IYER

University of California, Santa Barbara

**Palestine Papers, 1917-1922: Seeds of Conflict.** Compiled and annotated by Doreen Ingrams. (New York: George Braziller, 1973. Pp. 198, \$6.95.)

Few contemporary historical problems are more shrouded in myth than the question of Palestine. The origins of this tragic dispute remain a favorite hunting-ground for both historians and propagandists; and the flood of literature about it has now reached gargantuan proportions. Much of it, of course, is highly tendentious, and can be easily dismissed. But with the relaxation of the "fifty-year rule" in 1968, the British records for the period became available for scholars. This is a rich treasure-house indeed, and many recent books about Palestine have benefited enormously from

dipping into it. Yet documents, like statistics, have to be treated with extreme caution. They are not an end in themselves; and however revealing, almost by definition they cannot tell the complete story.

The book under review, according to the dust-jacket, purports to relate "For the first time the story of Palestine from the Balfour Declaration to the British Mandate" as "presented by the official documents, memorandum and letters of those in power both in Whitehall and Palestine." This is a highly laudable aim. Unfortunately, it is not borne out by the contents of this volume. The documents here presented have been chosen on an extremely selective basis; it could hardly have been otherwise in a volume of 198 pages. Moreover, they have also been edited, at times most severely. Bridge-notes have been inserted in an attempt to link the various episodes, but they are scarcely adequate to give a comprehensible picture. The picture which does emerge is, inevitably, a distorted one. The overwhelming majority of these documents lend credence to the by now discredited view that Britain's Palestine policy was the result of some mysterious conspiracy foisted upon an unsuspecting government by a determined band of Zionists and their supporters. Well-known Zionists such as Leo Amery or Col. R. Meinertzhagen or Lord Balfour speak out. In particular, Balfour's celebrated and oft-quoted remark that "The Four Great Powers are committed to Zionism. And Zionism, be it right or wrong, good or bad, is rooted in age-long traditions, in present needs, in future hopes, of far profounder import than the desires and prejudices of the 700,000 Arabs who now inhabit that ancient land" (pp 73).

But their observations are generally lost among the often bitter comments of the officials and the men-on-the-spot in the Palestine administration.

Why then, in the face of so much criticism, did Britain persevere in this policy? There is no clear, unequivocal answer to this intriguing question. Obstinacy? A sense of obligation? Ostrich-like blindness? The furtherance of British interests? Obviously all these elements played their role. Sadly, they make little impact in this book. Still, we have to be thankful for what we are given. Any collection of documents is valuable, provided one avoids the beckoning pitfalls.

One last word. Many of these documents first saw the light of day some years ago. Those familiar with the bibliography of the subject will know that many of them were made available in the important, official series, *Documents*

*on British Foreign Policy*. And on the whole they were set in a more comprehensive and historical context.

NORMAN ROSE

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**Israel and Nuclear Weapons: Present Options and Future Strategies.** By Fuad Jabber. Published for the International Institute For Strategic Studies. (London: Chatto and Windus, 1971. Pp. 164. £2.50.)

*Israel and Nuclear Weapons* consists of three parts. The first reviews the institutional framework and its development since Israel made the first steps in its nuclear development; the second discusses requirements for nuclear weapons production as they relate to the country's scientific and economic potential, and the third reviews the ends and means of the Israeli nuclear strategy.

Israeli nuclear development started as early as 1949, a year after the state was founded. The first steps were the production of uranium as a byproduct of the phosphate industry and the development of what was regarded as a cheap way of producing heavy water. Various motives lay behind Israel's nuclear development. One is the use of nuclear power as a source for fuel due to the absence of local fuel sources. Another is the use of atomic energy for water resource development through desalination. The author stresses, however, the military and political implications of this endeavor. The key problem he is concerned with is the possible development of nuclear warheads and the strategic implications of this possibility. In developing nuclear weapons three stages seem necessary: the production of the fissionable core, weapons design, and assembly and testing. Whereas weapons design and assembly do not pose a serious obstacle, Jabber tells us, conditions for testing of an Israeli bomb seem unfavorable because of the possible reactions of the superpowers and the Arab world. Testing is not however regarded as an absolute necessity. The major problem is the production of the fissionable core and here the author assumes on the basis of available information that Israel has not reached the last stage which involves operating a chemical separation plant for plutonium production, though he does not rule out the possibility that Israel has entered this stage clandestinely.

Israeli nuclear policy has been described by the author as aiming at deterrence through uncertainty and ambiguity. Israel did not sign the non-proliferation treaty of 1968 but the officially declared government policy was that

Israel will not be the first country to introduce nuclear arms in the Middle East.

The author points to the fact that Israeli nuclear capacity served as a bargaining lever in the past. In 1966 Israel froze her operations at the Dimona nuclear center in return for an arms supply from the United States. The nuclear option seem to provide Israel with an alternative, in case the conventional power balance takes an unfavorable turn. The author suggests that nuclear deterrence could help in preserving the post June 1967 boundaries. He also considers the eventuality of a nuclear race between Israel and Egypt.

Since the book was written (in 1970) some developments have cast a new light on the nuclear problem in the Middle East. The October 1973 war illustrated that the nuclear deterrent did not figure directly, though Israel's nuclear capacity has probably not been ignored. The war has indirectly increased the probability of a Middle-Eastern nuclear race. It grew with president Nixon's visit to the Middle East in June 1974 and his pledge to support Egypt's nuclear development.

The possible results of this race either as a stabilizing factor due to mutual balance of horror or as an additional source for aggravating the conflict cannot be predicted. The problem could however be viewed in a broader context pointing to an inevitable proliferation of nuclear capacity and nuclear weapons in middle and small size countries.

Jabber's book is a well documented work that is probably the most authoritative source to date on the history of Israeli nuclear development. There seems, however, a reason to doubt the author's conception of Israel's strategy as well of its political culture.

JACOB REUVENY

*Bar Ilan University, Israel*

**Soviet Economists of the Twenties: Names to be Remembered.** By Naum Jasny. (New York: Cambridge University Press, 1972. Pp. ix, 218. Index. \$12.50.)

The title of this book carries an implicit negative emphasis. This book is not about the economy or economics. The work offers a bird's-eye view of happenings in the Soviet economy over the period from the trebly escalated disaster (of World War I, the Civil War, and finally the War Communism which brought it to a virtual paralysis) through the recovery under the NEP (New Economic Policy) up to the crystalization of the mechanism for a highly centralist command economy and of a strategy for economic advance. It carries also a critical excursus into their sequel—the process

of a "heroic" drive which was ultimately to bring the primitive economy to its present status in the world. (I use the word "heroic" in a neutral fashion, indicating supreme intensity of effort, not to convey any qualitative or ethical valuation.) All that the book gives the reader about these happenings is a sketch of the background: describing the environment in which the tragedy of the Soviet economy occurred.

Further, the book also offers broad sketches of some of the great issues of economic policy which confronted the Soviet generation of the time. Quite clearly it is Jasny's aim to give here a full analysis of these issues or of the conceptions underlying them. What is offered is no more than what is needed to make the roles of the actors of the tragedy intelligible; the actors are economists of the period. In a word the book is essentially *not* about the economy; it is *not* about economics; it is about economists, indeed *some* economists—as will be clear from what follows immediately.

In this presentation, the focus is on the opposition to the regime. As the author points out, while the opposition outside the country was mainly interested in politics, the opposition inside the country operated mainly in the economic field. The author, one of those who for a time had a large part in it, chooses the economists he discusses by a *political* criterion: participation in the oppositional movement, essentially the *mensheviks* and *neo-narodniks* (adherents of the "minoritarian" and "neopopulist" opposition). The specific area of conflict with official orthodoxy is here that of planning, its mechanism and principles.

Thus the central figure of the tragedy, Groman, appears as the author of a 1925 study on certain regularities empirically discovered in the economy, seen as the milestone of Soviet ideas on planning (true, his theoretical stand—the insistence on the need to pursue equilibrium in economic strategy—is mentioned). Yet another principal figure, Bazarov, is shown as a proponent of a "general"—very long-term—plan. Ginzburg is here identified with the draft of the pioneering first Five-Year plan (1927/28–1931/32); Kondratyev is depicted as the head of the Business-Cycle ("conjunctures") Institute (although some mention is made of his concept of the long cycle, known in literature as the "Kondratyev cycle," which was condemned by Soviet critics as granting new lease on life to capitalism).

The way the book was conceived and the criteria adopted account for the scant treatment given some of the most interesting figures among Soviet economists of the time. A most

telling example: ever since Professor Domar discovered his remarkable work, Feldman has been considered by students interested in the formal modeling of economic growth as one of the most important pioneers in the field. Indeed, in the longer perspective it seems legitimate to see him as *the* greatest Soviet economist of the 1920s. Yet in this book, devoted, as the title announces, to "the Soviet economists of the Twenties" all that can be found about Feldman is a sentence describing him as an "arch-planner" and a footnote giving reference to his 1929 papers (now deservedly celebrated in Western literature)—papers which Jasny singles out as those from the pen of the best known exponent of what he calls "bachchanalian" planning (p. 136, p. 135 fn). I believe that one of the virtues of Feldman's mathematical construct is that it demonstrates how different assumptions and postulates (which we would say, are in the last instance extra-economic, evading valuation in economic terms) do lead to an *alternative* solution of the problem of the pace of growth. (Was this the cause of Feldman's doom at the time when the *uniqueness, no-alternative* type of solution adopted in economic policy problems had the status of an axiom? The question is not even touched upon by the author.)

The book is essentially Jasny's personal tribute to a group of economists and an attempt to rescue them from the oblivion into which their names have fallen. (Of the economists whose profiles are drawn—ten specifically classified as mensheviks, and six as neonarodniks—only one, Vainshtein, who reappeared not long before his death, deservedly attracted attention in the world of economics as an eminent mathematical economist.)

I have spoken of the book as depicting the actors in a tragedy. For it is supremely tragic that the forum in which the economy's historically crucial issues were discussed—and the fate of economists decided—was a criminal court.

If nothing else the reader of Jasny's book will experience this sense of tragedy. In this lies its principal merit.

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**Urban Government for the Prefecture of Casablanca.** By Katherine Marshall Johnson. (New York: Praeger Publishers, 1970. Pp. 251. \$5.75.)

The quality of the work of documentation, research, and synthesis done by Mrs. Johnson is worth praising. The author has been able to

gather the documents, question the responsible persons, and analyze the web of services that ensure the working of this complex administrative setup. This work is certainly worthy of being used by Moroccan students of administration, giving as it does a precise idea of the juridical attributions and the theoretical functions of each of the city's organs. Such readers will become aware of the subtlety of the tutely rapport that the Ministries, especially the Ministry of the Interior and the Ministry of Finance, have with the local services of the municipality. The author does not conceal the sense of an evolution which ends by stripping all real authority from the Town Council, the elected assembly for administering, in theory, the city's affairs.

Even if we stick strictly to the author's analysis, we must keep in mind that the administration of a big city like Casablanca requires vast juridical, technical, and financial machinery, extending, in present-day Morocco, structures that had been set up essentially by the French Protectorate. We can certainly perceive that the present demands are not those of colonial times, and that the rhythm of development conceived by foreigners cannot be imposed on the biggest metropolis of the country. But the impression remains that the Moroccan authorities manage the French bureaucratic inheritance competently and endeavor to maintain its spirit. In large part, this impression corresponds to reality, but the description given by the author sometimes conveys the same feeling of inaccuracy and unreality as that conveyed generally by the French Law manuals dealing with the same subjects.

In fact, it seems impossible to have a precise view of the mechanisms of an urban administration without integrating their political functions in the analysis. The juridical system so well observed by Mrs. Johnson often aims to dissuade the persons under its administration from having any recourse to the normal proceedings, and to make them consent to enter a system of patronage which will permit adapting the law to particular cases in exchange for some financial advantages and prestige for the persons in charge of it. Moreover, simple bribery makes possible a kind of participation by subjects in the decisions of the administration, and this is not given by legal procedures or democratic elections or the existence of a party's cells controlling the administration.

At a higher level, the juridical character of the procedures has a yet more important political role. In fact, it enables the Governor of the Prefecture, who has often been in the past a political man of national importance, to en-

sure a certain form of arbitration of the demands of the social forces represented by the human milieu of the economic metropolitan system. For if the governor's regulatory power is limited, the personal decisions he has to make are numerous and involve large interests. First of all, owing to a custom inherited from the French political administration, he may encourage the fussy and trivial juridical attitude of the Prefecture's officials so as to be in a better position to manage the application of laws and regulations. The regulations on building matters, price control, or industrial plants offer a wide range of possible derogations for a supporter one wants to protect or troublesome interventions for an opponent one wants to force to yield. The alteration of urban plans, the inclusion or exclusion of certain zones in the areas to be built offer important means of pressure. The attribution of public expenditures gives similar advantages.

In these conditions, the Governor seems to appear as the referee between rival groups (Fassi or Souassa trademen, foreign contractors, trade-unions leaders). Troublesome juridical rules and a cumbersome administrative system give him the opportunity to interfere in many affairs, to assert his power based on a flexible and personalized law, rather than over the action of a rational bureaucracy.

The accurate knowledge of the Moroccan administration shown by the author leads me to suppose that she is aware of its real operating mechanisms. It is indeed a subject on which a social inquiry, difficult to carry out, must convey the juridical analysis, if one does not want to give a partial view of the real situation. Evidence is difficult to find, and the subject is politically a ticklish one. But avoiding a statement on the problem is not a solution.

REMY LEVEAU

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**The Politics of Chinese Communism: Kiangsi under the Soviets.** By Ilpyong J. Kim. (Berkeley: University of California Press, 1974. Pp. 240. \$12.50.)

After the débacle of the "first united front" with the Kuomintang in 1927, remnants of the shattered Chinese Communist Party (CCP) retreated to the mountainous borders of Kiangsi province in south central China, and under the leadership of Mao Tse-tung reoriented the revolutionary movement toward a rural strategy. While the formal leadership of the CCP continued to operate fitfully underground in Shanghai until about 1931 and to hatch urban insurrection, the Communist movement existed as a reality only in the "soviet areas"—inkspots

of continually changing size and shape on the borders between Kiangsi, Fukien, Hunan, Anhwei, Hupei, and Szechwan provinces—which were formally organized into a "Chinese Soviet Republic" in 1931. The Republic survived only briefly, before the victorious Kuomintang armies of General Ch'en Ch'eng swept the Communists out of Kiangsi and onto the "Long March" to Shensi province in north-west China in 1934 and 1935. In the course of his mop-up of the CCP-held regions, General Ch'en collected numerous Communist Party and Soviet Government documents, newspapers, and magazines which constitute rich source materials for research on the CCP in the early 1930s. Professor Kim's book is a study of how the soviet areas were governed and is based on the Ch'en Ch'eng collection (which is now largely available on microfilm in several American libraries).

Among students of the Chinese Communist movement, consideration of the Kiangsi period has tended to focus on a number of key questions: the rise of Mao Tse-tung and his relations with the CCP Central Committee, factionalism in the CCP leadership, the nature of the economic and social program of the CCP in the Soviet areas, and conflict among the CCP leaders over military strategy in response to the Kuomintang's "extermination campaigns." Professor Kim provides useful information and commentary on several of these matters: for example, he is convincing in his conclusion (in opposition to the views of Hsiao Tso-liang) that no single faction dominated either the Party or Government during the Kiangsi period; although his views on military strategy were not followed, Mao Tse-tung was never without influence in nonmilitary matters. Kim is similarly credible in his account of the basic opportunism of the economic policies of Mao as chairman of the Soviet Republic. But none of these matters are Kim's central concern.

Professor Kim's primary theme is the assertion that the CCP in Kiangsi under Mao first developed its distinctive techniques for popular political participation (i.e., the "mass line"), which were further elaborated at Yen-an during the war years and remain central to the political style of the People's Republic of China. This is an interesting and plausible hypothesis, but in the body of his work it is less analyzed and proved than advanced by exhortation and repetition. Although he may believe that he is illustrating his hypothesis—and perhaps the Ch'en Ch'eng materials would uphold it—nowhere does Kim present a sustained and documented example of this new kind of politics in action. Instead we are treated to a meander-



ing discussion of Mao's "theories" of administration and organization, the individual sentences and paragraphs of which it would be embarrassing to parse in search of meaning; a confusing description of the various levels of government in the Communist-held areas (in which not even the map of the Soviet areas on p. 33 is keyed to the text); a useful presentation (already referred to) of the leadership structure; an account of mass mobilizational policies and organizational techniques which is schematically interesting but lacks verisimilitude in almost totally ignoring the objective constraints (the Kuomintang attacks and the shifting boundaries of the areas and population under CCP control) on carrying out *any* policy by *any* method; again a schematic account of local government which leaves one wondering how it really did work; and a scanty analysis of cadre training in mass line politics.

Occasionally Professor Kim's own doubts are evident: "The rural population remained passive, if not openly hostile, toward the mobilizational efforts of the central soviet government" (p. 149). "Apparently the CCP often had difficulty in extending its primary organizations at the grass-roots level throughout the soviet area" (p. 170). "No accurate data are available, however, to measure the extent of mass support during the Kiangsi soviet period" (p. 189). It would not be surprising if the origins of Mao's political style were traceable to the Kiangsi soviets, but its later effectiveness depended at least as much upon a favorable domestic and international context in the 1940s—in contrast to the fragility of the CCP's hold on the Soviet areas in the early 1930s—as on the mass line itself. What we need at this point is a careful, comprehensive, and analytical history of the Kiangsi period, which is not primarily concerned with finding the roots of the present in the past.

ALBERT FEUERWERKER

*The University of Michigan*

**Leader and Vanguard in Mass Society: A Study of Peronist Argentina.** By Jeanne Kirkpatrick. (Cambridge, Mass.: The MIT Press, 1971. Pp. 262. \$12.50.)

Professor Jeanne Kirkpatrick has written an interesting, useful, important and somewhat controversial book. Using survey research data obtained by Research Associates in 1965, she has explored a number of interesting hypotheses. Making a distinction between core Peronists (those expressing personal support for Perón) and pro-Peronists (those expressing support for the movement or Peronist objectives) Professor Kirkpatrick has made useful

observations on the differences among the Peronist following, on the general strength of the Peronist movement among the electorate, and on the social heterogeneity of the pro-Peronists as compared to the core Peronists during the middle 1960s.

The author's interpretation of the survey results as indicating that "the image of the nation as ruled by the traditional 'oligarchy' for their own interests is dead" (pp. 128–129) is important since Perón set out to accomplish this objective in his first era of rule (1946–1955). It was not generally known that many people believed the old oligarchy was gone and a new one in place outside of guerrilla and guerrilla-supporting circles. Also noteworthy is Professor Kirkpatrick's analysis of Peronist demands: particularly interesting was their strong support for price controls in 1965, a demand Perón responded to in his Social Pact. Other highlights of the study are the good description of where the political center is if one uses a single Left-Right continuum, the necessary relating of Peronism to traditional Argentine politics, the exploration of the problem of identifying an heir to Perón, the evaluation of Peronism as not a profoundly revolutionary movement, and the generally accurate description of recent Argentine political history.

Nonetheless, a number of Professor Kirkpatrick's findings or observations and conclusions are controversial. One of the key objectives of the study was to test the "two cultures" hypothesis regarding Argentina's political culture. Professor Kirkpatrick finds "the absence of major, socially based cultural discontinuities in Argentina" (p. 198). Her conclusion is that it is a "stereotype" to believe there is any geographically based division to the nation's political cultures. The evidence of the March and September 1973 elections confirms, contrary to the author's contention, the continued existence of two cultures, both with Right-Left-Center positions. It is true that the "liberal" tradition is the smaller of the two cultures, having accounted for slightly more than 20 per cent of the electorate in March and for only 12 per cent in September. This reviewer believes that attitudes originally geopolitically and historically based have been abstracted to the level of ideology, have now come to cut across class and regional lines, and have subsequently become dominant (nationalist-populist culture) and subordinate (liberal culture) value systems. Probably Professor Kirkpatrick's failure to identify the two political cultures may be traced to the questionnaire. The questionnaire was not constructed to probe deeply the values associated with the two-culture thesis. For ex-

ample, *Argentinidad* in the text is associated with anti-foreign and anti-cosmopolitan attitudes (p. 189) but *Argentinidad* is also associated with the values of the liberal tradition as in the writings of José Ingenieros, e.g., *Las Direcciones Filosóficas de la Cultura Argentina*. It is necessary to explore more fully than was done in the book to disprove the two-culture thesis, since both are "nationalistic" (*Argentinidad*) traditions.

A number of observations are questionable: Perón did not represent a significant change from ascription to achievement (p. 45); indeed his rule was noted for its nepotism and cronyism. Moreover, there was far more conflict between sectors of the middle and working classes than the survey seems to indicate; and there is considerable evidence that Marxism may become the doctrine of a portion of the middle class hostile to the workers. In the treatment of "influentials" no attention was paid to the managers of the state-owned enterprises, and the findings on the newspapers were in part distorted by not including *La Nación*.

The findings of the study might have been more persuasive if placed in the context of the political situation of the Illia regime. The existence of the Radical Party in power may explain some of the peculiarities of the survey. Other issues raised by the study probably requiring modification are the author's interpretation of Peronist distrust of government and the author's interpretation of earlier Peronism in terms of later Peronism.

Despite my criticisms I consider the Kirkpatrick study provocative and insightful. But it remains, nonetheless, an incomplete investigation and needs the depth and interpretation that only sustained study of the Peronist movement and Argentina can bring.

DAVID C. JORDAN

*University of Virginia*

**Pluralism in Africa.** Edited by Leo Kuper and M. G. Smith. (Berkeley and Los Angeles: University of California Press, 1969. Pp. 546. \$12.75.)

Tension along ethnic and racial lines continues to plague African states. Therefore the need to ameliorate the resultant difficulties for national order persists. This book seeks to increase our conceptual and empirical understanding of the problem. Despite their individual nuances, the contributors share Furnivall's model of the plural society as well as his inverted emphasis on the effects of its heterogeneous nature on its socioeconomic organization. Their intention, however, is to clarify and generalize his model (p. 448). They de-empha-

size the economic factors and perceive the significant explanatory variables to be biological, cultural, and institutional differences; the absence of value consensus; and the rigidity and clarity of group definition. Societal factors are limited to the relative and absolute numbers of population identified by Pierre Van den Berghe and M. G. Smith, and the analyses of variations in the number of plural sections by Van den Berghe and Hilda Kuper.

Among their various solutions to the adverse effects of pluralism is faith in the evolutionary process by which increased functional differentiation would provide new and cross-cutting bases for association. This viewpoint is most strongly expressed in Van den Berghe's advocacy of nonviolence in South Africa. All Mazrui envisages a dialectical process which creates a cumulative experience in intergroup conflict resolution; and Smith recommends a uniform mode of incorporation of disparate groups into national life, equal opportunity, and public enforcement of fundamental freedoms. Leo Kuper, however, recognizes the reversible nature of depluralization, which he argues arises from the readiness of politicians to exploit ethnic and racial differences.

Thus the book largely represents the orthodox Western analysis of pluralism, replete with its use of social science jargon. But the products of this perspective which continue to be peddled in the African states have failed significantly to improve the situation. Smith rationalizes this failure by arguing that "to anticipate the smooth development of such societies into nations, or even any rapid, continuous process of modernization within them, is anti-historical and antiempirical in the extreme. We need merely remember Latin America, Belgium, Spain, Portugal and Germany, all of which have known independence far longer and under far more favorable conditions" (p. 448). To accept this rationalization is to accept the theory of the inevitability of political instability in Africa. It reflects the failure of the contributors to diagnose the problem adequately.

Their analyses are deficient for two major reasons: (1) they neglect the nature of the socioeconomic organization of society, and (2) they are totally preoccupied with intergroup variables. By focusing on biocultural factors they treat the significant units of social action as though they are innate; as if the socioeconomic organization of society is static, and the economic structure of society is an indifferent condition of sociopolitical action. Since the context for the interaction of the plural sections is excluded from analysis, his-

torical changes in it are ignored. Similarities are identified across the precolonial, colonial, and postcolonial societies of Africa irrespective of the differences in their degree of social differentiation, the level of growth of their productive forces, their prevailing relations of production, and the degree of class consciousness of their populations.

While they correctly identify the origin of pluralism in group interaction, the authors fail to investigate the cause and nature of such contact. For example, the relevant significant arena of interaction in contemporary African societies is the urban area. Therefore, factors which define their urban situations are the most crucial for analysis. These are the colonial heritage of the African states, particularly their status as dependent and peripheral appendages of the advanced capitalist nations; their inferior role in the international division of labor; scarcity of goods and services; socioeconomic competition of the kind underlined by Furnivall; inequality in socioeconomic life; social atomization of the urban population; the pervasive socioeconomic insecurity of the individual; and the low level of class consciousness of the population. By failing to integrate these factors into their analyses, the contributors mystify, rather than clarify Furnivall's pioneering work.

Also, although the authors view processes at the individual level as relevant, they de-emphasize Furnivall's correct observation that capitalist market forces in tropical colonies created conditions of social atomization. Furnivall incorrectly attributes to market relations and colonial domination the success in counteracting the resultant anomie of the individual. Rather, it was ethnic and racial group solidarity that provided the necessary solace. The destructive competition for scarce socioeconomic resources in urban Africa impels the individual to seek allies in order to attain his goals. The low level of class consciousness ensures that these informal alliances follow ethnic and racial lines. Heightened socioeconomic competition is a crucial motivational complex leading to pluralism. Thus the group factors which the authors view as central to analysis are today symptoms rather than causes of pluralism.

A focus on the individual is important for resolving ethnic and racial difficulties in the African states. Given the dominance of capitalist structures in these states, the psychological paradox reflected in Sumner's concept of antagonistic cooperation characterizes group cohesion. Members are still hostile to each other; they have only renounced a part of this hostility because of utilitarian motives and

sentiments of actual and putative kinship ties. Cohesion is permanently countered, however, by disruptive forces of antagonism. Egotistic interests remain the most powerful motives and are only somewhat mitigated by group solidarity. Therefore, the individual's identification with ethnic and racial groups can be decreased by increasing his socioeconomic security.

As the experiences of Belgium, Canada, and the United States indicate, this security cannot be achieved by the elimination of scarcity of resources alone. Socioeconomic insecurity is also related to the inequitable distribution of wealth in society. A higher degree of inequality diminishes the certainty of a reasonable share. Fear of being confined to the bottom of the receiving ladder impels the individual to seek the security of ethnic and racial group solidarity. Private ownership together with the profit motive of economic enterprises in capitalist societies encourage inequality. Thus the requisite solution to the adverse effects of pluralism in Africa is a socioeconomic organization which limits the scope and intensity of competition and encourages a high degree of egalitarianism. The strength of the book lies in the breadth and rigor of its analyses; but its explanatory capacity is weak. It is a useful starting point for further examinations of pluralism in Africa.

OKWUDIBA NNOLI

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**White-Collar Unionism in Britain.** By Roger Lumley. (London: Methuen, 1973. Pp. 160. \$7.50, cloth; \$3.25, paper.)

We actually know very little yet about white-collar trade unionism. This at least is clear from this survey of the present state of—and state of knowledge about—white collar unionism in Britain. What we do know is pulled together here in a useful and concise report that will be an invaluable sourcebook for students of British industrial relations, politics, and social structure. Research students will find here up-to-date information on white-collar trade-union growth, structure and patterns of behavior; and will find, in the heavily footnoted chapters, a useful guide to the limited amount of academic work so far completed on this major growth area of British trade unionism.

Yet in his attempt to summarize both existing trade union practice and academic research at the same time, Mr. Lumley performs neither task adequately. As a brief description of white-collar union practice, his report is superficial, and as a result at times misleading. This is particularly true of his account of militancy by teachers' unions, where he reduces complex

patterns of organizational motivation to a single preoccupation with the declining status of teaching as a profession (p. 100). Throughout indeed, the chapter on the characteristics of white-collar unionism reads like a collection of union broadsheets, so that in the end it offers little more than a formal catalogue of official bargaining procedures. Serious scholars of white-collar unionism will not find this enough and will be forced to examine the detailed monographs on which this book is based.

Nor is Mr. Lumley sufficiently critical of the academic literature that he marshals and surveys. He makes no attempt to guide the reader through the competing schools of thought on union growth and character, or to tackle systematically the difficult problems of definition and problem-specification that must precede the future progress in white-collar trade-union studies that he seeks. In particular, he uncritically adopts the well-worn argument that it is the particular social position of white-collar workers that best explains the growth and character of the unions that they join. But—as is so often the case in the literature on white-collar unionism—the book does not offer the parallel analysis of manual workers and their unions which alone could validate such an argument; it merely implies (p. 34) that manual workers have different attitudes about their unions—attitudes that are in some unspecified way connected to their different social position. Yet recent work by myself and others (G. S. Bain, D. Coates and V. Ellis, *Social Stratification and Trade Unionism* [London: Heinemann, 1973]) has argued that this easy equation of social position and trade unionism does not hold, and that students of white-collar union growth and character should abandon previous preoccupations with questions of social position. If we are right, Mr. Lumley's report—though specifically produced to guide future research (p. 14)—is more likely simply to reinforce misconceptions that have already dulled the impact of research on various aspects of white-collar unionism; and accordingly American readers who make use of the book's excellent statistical data should treat its nonquantitative sections with a degree of caution.

DAVID COATES

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**The Seizure of Power: Fascism in Italy, 1919–1929.** By Adrian Lyttelton. (New York: Charles Scribner's Sons, 1973. Pp. 544. \$17.50.)

Fascists have often been accused of pursuing power for its own sake and Lyttelton comes

perilously close to this demonic view of Fascism when he describes it as "the creed of a group devoted essentially to the pure aim of seizing power by violence" (p. 42). Fortunately for the book and for the reader, he does not dwell long on such putative motivations. Instead—and this is perhaps the most noteworthy feature of the book—he leads the reader through the complicated and shifting rivalries, alliances, confrontations, and compromises that were the hallmark of Fascist government in Italy. He does so with a wealth of detail and comes up with the most thorough "group analysis" of Fascism available in English. The research is extensive both in the published primary and secondary sources up to about 1970 and in archival collections located in Italy and England.

The range of the author's definition of power is indicated by the chapter headings, which include: Mussolini and His Allies, The Party and the State, The Fascist Economy, Ideology and Culture, and Propaganda and Education. Power in the encompassing sense suggested here must be built up very gradually. It took the Fascists nearly seven years after they seized the government to concentrate such pervasive power in their own hands. The fact that their power was never as absolute as some of them had anticipated cannot obscure the fact that by 1929 a new type of government had emerged in Italy which people throughout the world either admired or detested.

What, according to the author, were the distinctive traits of this new type of government? Lyttelton apparently sees the uniqueness of Fascism in the way it manipulated power, particularly in the manner in which Mussolini imposed and maintained his supremacy, the techniques whereby he won the support of vested interests like the Church, the bureaucracy, and big business, and the adroitness with which he prevented his own followers from challenging his personal rule. The result was a compromise with every vested interest strong enough to resist the more radical aspirations of Fascism but willing to come to terms with what might be called the Mussolinian center, a compromise that was also achieved mainly at the expense of workers, small peasants, and politically expendable Fascists. None of this will strike those who are familiar with recent writings on Fascism as being particularly new. The originality of the work lies rather in the way these themes are developed, in the kind of supporting information uncovered by the author, and in his concern for what was happening at the "intermediate levels of power" (p. 429).

The author also attempts to introduce a com-

parative perspective, but at this point one begins to have some reservations. The comparative perspective is not as pervasive as the author seems to promise when he writes that the Italian movement "cannot be viewed in correct perspective if the European dimension of Fascism is not kept in mind" (p. 1). The promised European dimension turns out to be mostly Nazi Germany, and the only clear message that comes across is the familiar one that Italian Fascism was the lax and inefficient precursor of German Nazism.

More serious problems affect those chapters that deal with economic policy, ideology, and propaganda. The discussion of economic policy begins with the flat statement that "Fascism had no consistent theory about the economic system" (p. 333). Further on, however, we come across the observation that under government prodding, "organized capitalism replaced market capitalism as the Fascist model of the economy" (p. 360). How are we to reconcile the denial of a Fascist economic theory with the alleged existence of a Fascist economic model? A similar question arises in connection with the author's blanket dismissal of the role of ideology in the exercise of power. "The novelty of Fascism," he writes, "was that it was a political movement which deliberately set out to exploit irrational instincts" (pp. 364-65). What is needed here is an explicit distinction between theory and ideology, recognizing that ideology can appeal to both rational and irrational faculties. Fascist ideology certainly had a rational component when it emphasized the importance of social harmony within the nation. Rightly or wrongly, social harmony was perceived very rationally as a prerequisite for modernization and a stronger national posture. While one may certainly criticize the terms on which the Fascists achieved social reconciliation (and it was certainly not the workers who benefited from it), it seems nevertheless clear that those special interests who came to terms with Fascism did so on the basis of a rational calculation of anticipated gains, not because they were taken in by the Fascist appeal to irrational urges.

ROLAND SARTI

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### **The Untouchables in Contemporary India.**

Edited by J. Michael Mahar. (Tucson: The University of Arizona Press, 1972. Pp. 496. \$6.95.)

Let it be said at the outset that while this is primarily a book for South Asia area specialists, it contains descriptive matter and analysis of interest to political scientists concerned

with the politics of social stratification and social change.

There can be "dark" corners in an otherwise open society, and untouchability has been and in some ways continues to be one of them in India. So it was a good initiative which brought together under the sponsorship of the Association for Asian Studies in 1967 a group of scholars who had been especially devoting energies to probing various aspects of "the black sin." The present volume is the outcome, though the editor tells us that to the conference papers "other pertinent writings have been added." (Surely it is not pedantic to be irritated by such vagueness? The coyness of authors can usually be understood and sometimes forgiven, that of editors is often needlessly baffling and thus inexcusable.)

A general merit of the volume is its exploitation of several disciplines in order to close in on the problem from different approaches. There is a brief but telling essay by Walter Neale on the untouchable as "marginal laborer," a piece which shows the rigor and elegance of economic analysis at its best; its conclusion is that the bottom of the rural heap of Indian society can only be spared even grimmer effects of its marginality if investment increases fast enough to enhance the demand for labor and/or if the outcastes can exploit better than they have so far done the potential political advantages of their sheer numbers. Contributions from sociologists and anthropologists are too numerous for discussion here; it deserves to be mentioned, however, that Owen Lynch has interesting suggestions to offer on the components into which charismatic leadership (such as that of Ambedkar) can be analyzed; that Adele Fiske had brought to light much inaccessible information on the organizational outcomes, seemingly highly fragmented and minimally politicized, of the untouchables' escape into Buddhism; and that in general the reports from the villages speak of change which is patchy, variable, limited in scale and painful both to the oppressors who have to adjust and to those of the oppressed who see the gains as too tiny.

Fortunately, the two contributions which are of special interest to the political scientist are among the best in the book; they are the most substantial, more than 60 pages each, and they are—not surprisingly, in view of their authors' previous work of distinction—contributions of quality. Lelah Dushkin examines the political operation since independence of the system of "protective discrimination" in favor of the untouchables. That system, as Eleanor Zelliot shows in her historical essay, was in part a

legacy of imperial rule, in part the outcome of the contrasted political philosophies and pressures of Gandhi and Ambedkar, the former seeking to arouse the moral conscience of the caste Hindus by preaching their duties, and the latter striving to awaken the political consciousness of the untouchables by teaching their rights. (There seems to be an inconsistency in Gandhi which Zelliot fails to emphasize and Gandhians generally ignore. Gandhi tackled the Indo-British conflict from both ends: he sought to sap the morale and weaken the will of the rulers while building up the self-reliance and courage of the ruled to the end that the unequal relation could simply not continue. In relation to the Harijan problem he acted differently by using philanthropy rather than the whole of *satyagraha*: he worked mainly on the conscience of the elites, hardly at all on giving moral muscle to the oppressed—using half a weapon only, perhaps because in this case he espoused only half the goal.) Central to the system, which, however, includes job reservations in government service, is the special arrangement of political representation involving the reservation of legislative seats for untouchables. Dushkin provides the first overview analysis of the working of this system, drawing attention to the causes and consequences of the shift from double-member constituencies in 1952 and 1957 to single-member constituencies subsequently, to the fact that normally it is caste Hindu voters who determine the winners of the reserved seats, and that, for this and other reasons, Congress rather than the specifically untouchables' Republican Party has been the main beneficiary. The few radical leaders of the untouchables tend to see the whole operation as a clever fraud, a means of social control over the deprived: job reservations fall far short of targets and in any case constitute political castration by depriving the outcaste community of leaders; the reserved seats are comfortable places for puppets of the ruling party. Dushkin without endorsing such a view shows how far there are grounds for bitterness. She considers that plainly protective discrimination, while it provides leading Congress untouchables with bargaining power at times, defuses a nasty social problem without solving it and creates fresh divisions among the untouchables themselves. It is a defect in Dushkin's essay that its organization is untidy; it is no defect but a real merit that at a number of points she identifies specific problems calling for research, and it is to be hoped that she, as one of the best qualified, will press on with such research.

The lawyer's contribution by Marc Galanter, beautiful in its precision and order, is concerned with the other side of state action in relation to untouchability: the attempt to remove by law those very disabilities whose existence in Hindu society constitutes the framework of untouchability. A key section of his chapter assesses the effectiveness of the Untouchability (Offences) Act and kindred measures; it makes sorry reading. The state-wise distribution of prosecutions varies bafflingly from state to state. There has also been a decline in prosecutions while the percentage of convictions has fallen and that of acquittals has increased: "there is no doubt that there are more offenses against the Act in a good-sized village in a week than there are prosecutions in all of India in an entire year" (p. 270). The social context conditions the operations of the law to a greater extent than the law can change the social context. Galanter suggests the need for a new kind of administrative agency consisting of both an investigative branch and a tribunal.

It is a commonplace that the Indian political system has done a remarkable job of accommodating demands and containing tensions. This book amply shows just how much there is to do.

W. H. MORRIS-JONES

*University of London*

**Handbook of Soviet Social Science Data.**

Edited by Ellen Mickiewicz. (New York: The Free Press, 1973. Pp. 225. \$14.95.)

The last five years have witnessed an increase in the use of aggregate data in research on the Soviet Union. There are several apparent reasons for this, some having to do with such trends in the field of comparative politics generally, others rooted more specifically in the Soviet situation, especially the growing availability of reasonably reliable social, economic, and politically relevant statistics, coupled with the continuing paucity of other types of information, e.g., survey or event data. Still, the growth in the use of aggregate data on the Soviet Union has not been as dramatic as it might have been, presumably in part because these data are not as readily available as they could and should be to Western scholars.

Consequently, it is easy to agree with Karl Deutsch's statement, in his Foreword to the Mickiewicz handbook, that the book represents an "essential first step"—a "crucial beginning" for a "long journey" (p. xxvii)—toward increased use of aggregate data, both in research focused on the Soviet Union itself, and in cross-

national studies including the Soviet Union. Professor Mickiewicz and her collaborators have made an important contribution by demonstrating that a considerable variety of statistical material on the Soviet Union can be easily accumulated.

The book consists of an introduction by Professor Mickiewicz, in which she illustrates how quantitative data can be used to investigate several important issues related to the emergence and management of social cleavages in an industrialized society, followed by nine "chapters" contributed by specialists on various aspects of Soviet society. The chapters deal with demography, agriculture, economic production, health, housing, education, elite recruitment and mobilization, communications, and international interactions. Each consists of a brief—usually two pages—statement characterizing in very general terms the nature of available statistical data on that sector of Soviet society, followed by a series of tables (the average is about 22 tables per chapter) each carefully footnoted to its source. The vast majority of the data are from Soviet sources, although some secondary Western sources have been used. The contributors of these chapters are all respected scholars who have made important research contributions in Soviet studies.

Because the book is the first such handbook on the Soviet Union to be published in English, and because its catalytic and socialization effects should be substantial, we should not be too quick to form unfavorable judgments of it. Further, there is reason to restrain criticism based on what is left out of a collection of data, rather than what is included. But so much more could profitably have been included that it is difficult not to be disappointed with this book.

Perhaps the most fundamental—and puzzling—difficulty is the lack of systematic attention to subnational units. Data for the majority of variables are reported only at the national (all-Union) level. There are exceptions: Henry W. Morton's chapter on "Housing" reports republic-level data more often than not, as does the chapter on "Education." In her introduction, Professor Mickiewicz remarks that "... in every section but the one on international interactions, data are available for the union republic level" (p. 4). True; but Stanley H. Cohn's chapter on "Production" includes republic data in one table (of 17), and Warren W. Eason ("Demography") has subnational data in two tables (of 12). Overall, data on subnational units are included for considerably fewer than half of the variables. Yet data at

the union-republic level are available for most of these variables, often in the same sources as were used in gathering the all-Union data.

There are at least four reasons why the concentration on the all-Union level, and the accompanying neglect of subnational data, constitute a serious problem. In the first place, there is the simple fact that the Soviet Union is complex and heterogeneous; consequently, an appreciation of the character of that society cannot be gained merely from national-level data, which wash out significant interregional differences. Second, and relatedly, many of the interesting and important questions raised by Professors Deutsch (in the Foreword) and Mickiewicz (in the Introduction) involve within-system variation, and demand within-system data. Third, the presentation of only national-level data for many variables invites problems of ecological inference, difficulties which become less pronounced as one uses data at lower administrative levels, involving a lesser degree of aggregation. Finally, meaningful cross-national research—as well as inquiry focusing on the Soviet Union alone—requires investigation of *between-system differences in within-system relationships* (as suggested by A. Przeworski and H. Teune in *The Logic of Comparative Social Inquiry* [New York: Wiley, 1970]). System-level variables should be used to explain variance in a dependent variable only when other within-system variables leave substantial portions of variance unaccounted for.

The point is not that all-Union data on the Soviet Union are useless, but rather that such data must be used in conjunction with subnational data—at the union-republic level and below—in order to conduct meaningful research, either single-country or cross-national studies. And these subnational data *are* available, commonly at the union-republic level, and sometimes even at the *oblast* level, as can be seen simply by looking at Library of Congress holdings.

A second basic limitation of this handbook is the omission altogether of important elements of social—especially political—activity. Some of these omissions are especially puzzling, since relevant data could have been culled from Western, as well as Soviet, sources. For example, Professor Mickiewicz, in her chapter on "Elite Recruitment and Mobilization," includes virtually no data on the composition of important Party bodies (e.g., the Politburo or the Central Committee). Despite her recognition (p. 159) that "only a small proportion of the Party's membership" serves in centrally-import-

tant political roles, she reports only the composition of the CPSU as a whole. (Educational data on lower-level Party secretaries are the exception.) And non-Party (e.g., governmental) bodies are wholly excluded; especially disappointing is the absence of data on the composition of subnational legislative bodies (available, for example, in *Itogi vyborov i sostav deputatov*, 2 vols., Moscow, 1967). The chapters on production and demography also omit a substantial number of seemingly interesting variables for which both all-Union and republic-level data might have been included, as a glance at the various editions of *Narodnoe khoziaistvo SSSR*, *Vestnik statistiki*, and *Itogi vsesoiuznoi perepisi naseleniia* would suggest. A related difficulty is the absence throughout of measures dealing with consumption patterns. And, given the increasing interest in comparative public policy and expenditure analysis, the neglect of subnational budgetary data is unfortunate (see *Mestnye biudzhety SSSR*, Moscow, 1970).

A reasonable response to these first two objections might be that one cannot possibly include all of the potentially relevant statistics now available on the Soviet Union in such a book. Indeed, this consideration raises a third fundamental doubt about the book, especially insofar as it is primarily a collection of aggregate data: Is a bound book really the appropriate outlet for this kind of undertaking? Would not the interests of the relevant community have been better served if the data coverage had been significantly expanded, both vertically and horizontally, and the resulting data collection made available as a magnetic computer tape? For persons who do not have access to data processing facilities, hard-copy versions of selected data could be generated quickly and inexpensively from the tape; the reverse process, incidentally, is neither automatic nor inexpensive.

If the data themselves—in significantly expanded form—could have been made available on magnetic tape, a companion volume might have been published—one of smaller size and greater direct usefulness—in which Professor Mickiewicz and her collaborators could have turned their attention to two critical subjects which are given superficial treatment in the present volume. First, there remains a need for a much more detailed treatment of primary Soviet sources, indicating the substantive range of available data, the societal levels at which given data are reported, and the problems involved in using these data. (The chapters by Roy D. Laird, on agriculture, and Roger E. Kanet, on international interactions, are the only ones to treat such problems in any detail.)

Second, relatedly, there is a need for a guide to those Soviet publications that describe and discuss the nature of Soviet statistics-generating and gathering operations. One has the feeling, for example, that a careful reading of the instructions given to census-takers concerning the native language (*rodnoi iazyk*) question might have led Brian Silver ("Social Mobilization and the Russification of Soviet Nationalities," *APSR*, 68 [March, 1974], 45–66) to question the validity of his measure of "national identity." (See *Vsesoiuznoe soveshchanie statistikov*, 1968: *stenograficheskii otchet* [Moscow, 1969], p. 601.) A number of important sources dealing with the generation and handling of Soviet statistics need to be reviewed for the Western researcher—e.g., *Istoriia sovetskoi gosudarstvennoi statistiki* (Moscow, 1969); A. I. Yezhov, *Organizatsiia statistiki v SSSR* (Moscow, 1968); and *Sovetskaiia statistika za polveka (1917–1967 gg.)* (Moscow, 1970). In short, there is a need for a handbook on the availability and use of Soviet sources of social data.

There are two things about this book which seem to warrant positive response. First, the book demonstrates that a variety of social statistics on the Soviet Union can be easily assembled. The lament that "poor data" preclude systematic, quantitative research on Communist systems, and that work in this area must therefore be judged by less demanding criteria, has become a dubious rationalization. Perhaps the Mickiewicz handbook will speed the disintegration of this already crumbling façade.

Professor Mickiewicz's introduction is the book's other strength. In it, she shows that aggregate data can be used to address important questions attending processes of social change in the Soviet Union; e.g., the impact of changing elements of economic and technological context on the roles of women in Soviet society. At the same time, some of the most interesting parts of the introduction illustrate indirectly the major shortcomings of the data-reporting "chapters" that follow. That is, Professor Mickiewicz understandably uses subnational data in generating explanatory statements about the effects of social and economic context, for interregional differences in rates of social change are usually a good deal more interesting than are overall national (all-Union) trends. Similarly, Professor Mickiewicz makes use of some sample survey data in her introduction, but such data appear nowhere else in the book.

In short, the basic idea behind this volume is laudable. Unfortunately, the nature and depth of the data reported leave a great deal



to be desired. Nor is it clear that book form is the appropriate outlet for such data.

(NB: I am grateful to Professors Charles D. Cary and Barclay Ward for bibliographic assistance in the preparation of this review.)

WILLIAM A. WELSH

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**The Growth of Parliamentary Scrutiny by Committee: A Symposium.** By Alfred Morris. (New York: Pergamon Press Ltd., 1970. Pp. 141. \$8.00, cloth; \$5.50, paper.)

Analysis of British political institutions has directed considerable attention to the relative strength of the cabinet, parliament, and civil service. During the early 1960s critics who feared the decline of parliament advocated, in part, the development of a system of specialist committees as one means of improving the effectiveness of parliament. For various reasons, some select committees were established as an experiment beginning in 1966, and by 1970 many analysts believed that the experience with select committees warranted evaluation.

The symposium edited by Alfred Morris was one of the efforts made to assess the value of select committees. The book contains an introduction and conclusion by Mr. Morris, who is himself a Member of Parliament, and seven essays by Members of Parliament who have been Chairmen or members of select committees. The introduction outlines the background leading to the establishment of select committees and sets forth the major objections raised against them. The Foreword indicates that there are essays both for and against select committees, but this is somewhat misleading as there are six essays which on balance view select committees favorably and one which broadly views them unfavorably in comparison with the functions of the House of Commons as a whole and the relationship of both specialist committees and the House to the government.

The six essays dealing with particular specialist committees review the experience of the select committees on Science and Technology, Agriculture, Overseas Aid, Nationalized Industries, Britain's "Ombudsman," and Race Relations and Immigration. These essays vary in quality. Analysis of the select committees on Agriculture, Science and Technology, the Ombudsman, and Nationalized Industries are the most candid and give the most intricate analysis of the relationships among parliament, the committees, and the civil service. The weakest essays deal with the committees on Overseas Aid and Race Relations. As the author of

the essay on the Overseas Aid committee suggested, analysis of this committee is premature considering that it did not make a formal report before the dissolution of parliament and the doctrine of parliamentary privilege prevents the author from discussing outside the Committee either its deliberations or unpublished memoranda. The essay on the Race Relations Committee is an elementary statement of the activities of the committee without much analysis of the intricacies of committee operations.

In general the authors discuss the substantive problems and policies of their respective committees as well as basic institutional problems such as the role of select committees in a political system that does not entail separation of powers between executive and legislature. More specifically, the Members of Parliament discussed problems their committees experienced with terms of reference, procedural obstacles, limited research time, use of subcommittees, staffing, degree of cooperation with the civil service and Ministers, debate of reports in the House, and tenure of the committees themselves. Although many of the Morris symposium authors mention these as problems experienced by their committees, other reports in the literature on this subject may lead the reader to conclude that the Members of Parliament writing for the Morris symposium exhibit a tendency to emphasize the success and minimize the problems and failures of select committees. This tendency is perhaps regrettable considering that the objective of the symposium was to assess the experience of the committees. The responsibility for providing a balanced view of committee experience lies, however, less with the individual authors than with the symposium director. Rather than providing the reader with one assessment of each of six committees, a more insightful and effective symposium might have been organized around a format which would have given the reader several assessments of two or three widely known committees such as Agriculture or Science and Technology. Analysis by both academicians and Members of Parliament would have provided assessments based on first-hand experience as well as academic comparative analysis placing committee experience in perspective.

This symposium is a useful contribution to the evaluation of specialist committees and their place in the relationship between the House of Commons and the cabinet, but the reader would best insure obtaining a balanced view by consulting other analyses recently published in journals, as well as John Mackintosh's brief but insightful paper "Reform of the

House of Commons: The Case for Specialization," an Edinburgh University Press Occasional Paper reprinted in Gerhard Loewenberg's *Modern Parliaments: Change or Decline?* (Chicago: Aldine-Atherton, Inc., 1971).

LYNDELLE D. FAIRLIE

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**Entnazifizierung in Bayern: Säuberung und Rehabilitation unter amerikanischer Besatzung.** By Lutz Niethammer. (Frankfurt A.M.: S. Fischer Verlag, 1972. Pp. 710. DM 68.)

In the landscape of recent history, the American occupation of Germany is already thoroughly traveled terrain. But where the earlier theses concerning the inconsistencies and failures of American denazification policies have been demonstrated ad nauseam, the present work brings new material and fresh perspectives to bear. Lutz Niethammer's diverse German training in history and political science, combined with extended research in Britain and the United States, is evident in this example of meticulous scholarship.

At the cost of some injustice to his predecessors (e.g., John Gimbel and Gabriel Kolko), Dr. Niethammer attributes four basic deficiencies to previous studies: (1) the assumption that U.S. policies failed, without a thorough investigation of the social and political context in which they were formed; (2) the limited perspective of researchers who based their findings on a biased selection of evidence; (3) the false assumption of a "zero hour" at which the occupation began, which ignores the gradual extension of U.S. control and depreciates the constraints imposed by diverse conditions in German society; and (4) the misplaced emphasis on statistical aggregates showing social structural continuity (e.g., Edinger), rather than on the denazification processes themselves.

Despite its considerable length, this study excludes several related issues, such as the social structure of fascism, the efforts toward positive re-education, and the problems of securing sufficient economic and administrative personnel. The primary focus is on the interaction of social forces and individual personalities in the U.S. and Bavaria, boldly characterized as "a microcosm of German politics." Bavaria was chosen specifically because it was a self-conscious unit, less subject to inter-regional influences, with a more clearly delineated political infrastructure.

The first three chapters comprise a painstaking, sometimes tedious recounting of the diverse goals of American decision makers, the interim purges, the obstructive alliance of the

"non-partisan" Schäffer administration with the Patton command, the rise of the Bavarian parties, and the negotiations leading to the *Befreiungsgesetz*, the statutory basis for the extended phase of denazification. Although his conclusions do not diverge markedly from those of other recent studies, Niethammer's greater access to U.S. military documents permits a clearer assessment of the reciprocal influence of American and Bavarian officials. In discussing the origins of American policy, it is difficult to see where Niethammer has improved on the work of Gimbel, for example. But his ability to integrate German and American perspectives, particularly in the creation of the *Befreiungsgesetz*, is certainly impressive. Nevertheless, this first half of the book is unnecessarily protracted and marred by tiresome refutations of interpretations which, in my view, have already been discredited.

A more valuable contribution appears in the fourth chapter, with its emphasis upon the conflicting expectations of the U.S. command and the Bavarian government over the functions of the denazification tribunals. Because the enabling law included many loopholes and a basic obfuscation of the separate purge and rehabilitation functions, its early application depended heavily on the initiatives of the *Land* "liberation ministers." Niethammer recounts how numerous coalitions were built and shattered, as the Bavarian parties maneuvered to alter the impact of the proceedings, while avoiding the liabilities of "collaboration."

Particular attention is given the Communist attempt to accelerate the exoneration of certain groups to gain electoral advantages. This emphasis had the effect of delaying the cases of more significant Nazi officials, until the tribunals had degenerated into "white-washing" mechanisms. A conservative reaction followed the Communist failure, and attempts to reach a middle ground were preempted by renewed American control. The abrupt termination of U.S. concern is explained in the familiar terms of German economic needs and the pressure of the domestic American coalition forming along the battle lines of the imminent Cold War.

In general, Niethammer attributes the tribunals' abortive experience to: (1) the excessive magnitude of their task, given the shortage of competent German personnel; (2) the basic ambiguities of the *Befreiungsgesetz*; (3) the inconsistent orientations and recruitment criteria employed by successive Bavarian ministers; and (4) the persistent divisions between the U.S. command and the military agencies responsible for implementing policy.

In the final chapter the treatment of individual cases is analyzed by means of a sample taken from records of the Munich District. Detailed reporting of numerous cases is ably employed to illustrate how tribunal proceedings were misused by participants, as well as to support observed patterns of "typically abnormal" mitigating circumstances, which led to reduced charges and nominal penalties in the overwhelming majority of cases. The sample is disaggregated to reveal how tribunal verdicts discriminated among cases according to the level and duration of membership in Nazi organizations, a variety of personal and occupational attributes, and differences in the timing and procedures of adjudication. It is in this aspect that Niethammer's work is most innovative and productive.

Specialists in this question will surely want to consult the methods and findings of these last two chapters. In particular, Niethammer's thorough use of evidence deserves emulation. This book, however, cannot be said to have general significance, because of the author's disinclination to explore the implications of his findings beyond the unique circumstances prevailing in Bavaria in 1945-1949.

C. BRADLEY SCHARF

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**Area Handbook for Algeria.** By Richard F. Nyrop, John Duke Anthony, Beryl Lieff Benderly, William W. Cover, Newton B. Parker, and Suzanne Teleki. (Washington, D.C.: U.S. Government Printing Office, 1972. Pp. 401. \$5.00.)

This handbook, written primarily for U.S. military personnel, but probably with no mischief intended, offers a competent and useful if unoriginal survey of economic, social, and political trends in Algeria up to March, 1972. It marks a considerable revision and rewriting of its predecessor published in 1965. The chapters on education, economic structure, agriculture, and industry (respectively chapters 8, 13-15) are of special interest to students of political economy, in that they describe some of the regime's most significant choices. Thus, in the field of education, progress toward Arabizing the French system has been limited by the fact that "the existing trained Algerians merely continued the traditional dominance of French in all aspects of the modern sector" (pp. 120-21). Yet to satisfy those segments of public opinion that aspire for national "authenticity" and the resurrection of an Algerian, Arab-Islamic heritage, the Minister of Traditional Education and Religious Affairs has recently accelerated the construction of Islamic

secondary schools. In short, "the country seemed to be producing a French-speaking elite and an Arabic-speaking mass" (p. 121).

Despite the regime's commitment to Arab-Islamic culture, it is the French-trained technocratic elite that President Boumedienne has enlisted, together with foreign advisors including some 300 American consultants, to carry out his government's principal priority, rapid industrialization. Forty-five per cent of the six billion dollars to be invested from 1970 to 1973 was earmarked for petroleum, other mining, and the industrial sector (p. 260). Since this handbook was written, the dramatic rise in petroleum revenues may facilitate even more ambitious investment in heavy industry without risking excessive dependence on foreign credits. It remains to be seen whether the necessary skilled workers and cadres can be trained to run and manage the new industries efficiently. It is unlikely, however, that the "rapid and sweeping economic modernization" envisaged by Boumedienne and his advisors depends, as the handbook has it, "on westernization at the expense of Arabization" (p. 218).

Furthermore, the regime embarked in 1970 upon an "agrarian revolution" that is designed not only to remedy glaring inequalities (greater than those of Tunisia or Morocco) in the distribution of land but also to bridge the technological gap between the modern (ex-French, but also some Algerian) and traditional sectors. Unfortunately the handbook omits any discussion of the political implications of this new, long-term commitment. Personal networks linking the Algerian landowners with high officials are not discussed. Rather, the "technocrat" officials are supposed to have "brought their growing influence to bear upon the government" to reform the traditional sector so as to expand the country's market for industrial products (p. 294).

Usually the writers of the handbook are less naive, but, making "no attempt to express any particular point of view," (p. iii) they tend to have political judgments that are bland and bureaucratic, based on the conventional wisdom of various "observers" without citing names or sources. There are no footnotes. Clichés like "one million" killed in the war of independence, Algerians "overcome with deep alienation" (p. 177), and "the old institutions and values" crumbling (p. 151) abound. There is a tantalizing reference on p. 223 to the "active and decisive" role of army officers in local politics in the early 1970s, but no evidence is offered. And what is the political scientist to make of the following remark that by then "the civil servants had been superseded by the techno-

crats, a more diffuse and less precisely defined group, but nonetheless a politically potent one . . ." (pp. 223-4)? Obviously the technocrats, however they are defined, are important in Algeria, but their influence has not yet been seriously analyzed.

In an introductory but careful way this handbook covers everything from Algeria's political history to contemporary literary trends, Arabic as well as French, and even family planning, about which there is little to say (pp. 88-90). Weaknesses in political interpretations reflect more upon the poverty of scholarship on contemporary Algeria than upon the handbook's authors or any official constraints upon them. It is to be regretted, however, that the huge bibliography is simply a compilation without critical summaries or comments. Jeanne Favret's brilliant article, reprinted in E. Gellner and C. Micaud, *Arabs and Berbers* (London: Weidenfeld and Nicholson, 1972), is not cited; nor is the bi-monthly, *Maghreb-Machrek* (Paris: Documentation française), which, together with *Annuaire de l'Afrique du Nord* (Paris: Centre National de la Recherche Scientifique), only one year of which is cited, is the most useful periodical reference to contemporary Algeria.

CLEMENT HENRY MOORE

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**West Malaysia and Singapore: A Selected Bibliography.** By Karl J. Pelzer. (New Haven, Conn.: Human Relations Area File Press, 1971. Pp. 394. \$15.00.)

According to the compiler's preface this volume "is intended to serve as [a] reference and reading list for students in the Behavioral Sciences . . . , as well as History and the Natural Sciences. . . ." It undoubtedly has served students and practitioners in these disciplines well in the three years since publication, and, while the half-life of a bibliography is usually relatively short, it no doubt will continue to be of assistance in the endless search for pertinent source material. While I have found this bibliography helpful on numerous occasions, using it has also brought out some of its deficiencies. Of course, the criticisms that follow must be considered in perspective. No bibliographical tool is likely to satisfy completely the needs of a particular researcher unless he compiles it himself, and when the reviewer and the compiler are trained in different disciplines, the problem is compounded. Moreover, compiling a bibliography is a tedious and often thankless task, and it is with some hesitation that I venture to criticize any useful research tool. Nevertheless, for the benefit of

potential users in the social sciences some deficiencies must be noted.

The bibliography makes no claim to completeness, and almost no published bibliography on a subject as broad as this could be comprehensive, but what were the criteria for selection? Why was Eastern Malaysia (Borneo) excluded? Why was Singapore not covered as thoroughly as Western Malaysia? Why are some (to my mind) marginal University of Singapore M.A. theses included and some significant Ph.D. dissertations from the University of London omitted? If the volume is not to be confined to material in English, why are there so few non-English entries in the entire volume? The compiler plunged into much greater depth in his own discipline (geography), but in other areas there are fewer entries and the categories employed are much broader and vaguer. Is this a legitimate criterion for selection in a bibliography for social scientists? Mention must also be made of the bibliography's selectivity in the information provided. Some entries are very complete, but some are sketchy ("Kuala Lumpur, 1911" does not give the serious researcher much to go on if he wants to obtain a copy of "The Geology and Mining Industries of Ulu Pahang"). Lastly, HRAF should have devised a more usable indexing system for this volume. Unless one has memorized the organization of the book it is necessary to move from the author index to the desired citation by way of the table of contents, and for authors with multiple entries this can be a time-consuming and sometimes frustrating undertaking.

ROBERT O. TILMAN

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**Decentralisation in Yugoslavia and India.** By Nageshwar Prasad. (Varanasi, India: Navchetan Prakashan, 1972. Pp. 330. \$6.00.)

This book is an attempt at evaluating the experiments in decentralization of authority in two different societies, Yugoslavia and India. It confines itself, by and large, to the institutional aspects of such experiments and, therefore, tells us only half the story. Being deeply involved in the decentralization movement in India, Nageshwar Prasad, a Sarvodaya (a social philosophy based on Gandhism) theoretician and political worker, was in a position to tell us much more about the effects of such experiments than in fact he has done. Nevertheless, so far as the structural aspects of these experiments are concerned, the author has presented them in great detail and clarity.

Prasad analyzes in depth the different circumstances which stimulated the decentraliza-

ion movements in Yugoslavia and India. Consequently, the goals which these two societies sought to realize through decentralization remained materially different.

After the rift between Belgrade and Moscow, Prasad tells us, the Yugoslavs went back to the corpus of Marxism-Leninism in search of a political alternative. They looked for a political system which would reject the Russian type of coercive state, secret service, and bureaucracy. Soon they discovered that neither Marx nor Engels nor, indeed, Lenin thought of "the dictatorship of the proletariat" as a permanent phenomenon. Moreover, each of them had something to say about "the withering away of the state" along with the disappearance of the class structure. None, however, was clear about the form of authority which would emerge in a classless society. Marx in particular, Prasad points out, held the short-lived Paris Commune in high esteem and even treated it as a possible model for the reorganization of society. Consequently, the Yugoslavs, in search of an alternate political system, paid great attention to the principles of communal organization implicit in the Paris Commune. Subsequently, Kardelj and Tito viewed "decentralization" as an integral part of "the withering away" process. As Tito put it, "the decentralization . . . is not only profoundly democratic but has inherent in it the seeds of withering away not only of centralism, but of the state in general, as a machine of force" (p. 20). From such a theoretical position the Yugoslavs went on to create a series of institutions by which participation in decision making in industry as well as agriculture became possible.

Prasad points out that the Indian experiment with decentralization, on the other hand, was born out of a difficulty in reaching the targets set forth by India's five-year plans. The committee appointed to report on the problem recommended the creation of popular institutions at different levels of district administration so as to involve people in what was being undertaken by way of development targets. The author notes that in its recommendation the committee took a much wider view of development, equating it with human development through democratic process. Subsequently, the different states of India passed different legislations to create participatory institutions.

According to Prasad, another strand in Indian thinking on decentralization came from Gandhi and his followers. Gandhi had a horror of the coercive modern state. He also intensely disliked the exploitation of rural areas by urban centers. What made them possible, in his

opinion, was the over-concentration of technology and the consequent bureaucracy. The only antidote to them, in his opinion, was the diffusion of technology itself so as to create "ever widening, never ascending circles" (p. 195) of self-governing and self-sufficient communities.

Further, Prasad points out that Gandhi's followers, particularly, Jayaprakash Narayan (JP), identified yet another dimension, namely, the party warfare which is implicit in western democracies. JP maintained that democracies which are run with the help of party organizations neither attend to the outstanding problems of the community nor do they refrain from subverting the democratic process itself. JP therefore became, as well as a proponent of decentralization, an advocate of a communitarian partyless democracy.

The weakest portions of this book are those which are devoted to the exploitation of "conflicts" by political parties. Political parties, as a rule, manipulate cleavages wherever they are. But not all cleavages which result in conflicts are deleterious to the community. Quite often conflict is the only idiom through which a community can regenerate itself. In a traditional society such as the Indian the search for human dignity and social justice often expresses itself through what is generally regarded as conflict.

Despite its disproportionate emphasis on the structural aspects of the decentralization movement in Yugoslavia and India, the book has made a significant contribution to the field of comparative government and politics.

A. H. SOMJEE

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**Elite Accommodation in Canadian Politics.** By Robert Presthus. (New York: Cambridge University Press, 1973. Pp. 372. \$16.50.)

To reinforce his point that Canadian political scientists have, in the past, neglected the role of interest groups in Canadian politics, Presthus remarks that even the most authoritative text on Canadian government includes neither a section on interest groups nor index entries for "pressure groups" and "lobbyist." Presthus clearly sees his book as redressing the imbalance. Arguing for an end "to any artificial separation between 'political' leaders in the formal apparatus of government and 'private' elites in the institutional sectors of industry, labour, agriculture, and the rest" (p. 10), Presthus relies upon two major theories in this study of the political behaviour of interest groups and their place in Canadian society.

Interaction theory, resting upon economic as

well as psychological propositions, and elite accommodation theory applied to a society allegedly "consociational," combine to provide the framework within which Presthus explains his findings. The data for the study were gathered between 1967 and 1971 from an interview sample of more than 1,100 legislators, bureaucrats, and interest-group directors in Ottawa, British Columbia, Ontario, and Quebec. The presence of no fewer than 103 tables attests both to the volume and to the variety of the data gathered by the author.

In many ways the most useful and informative sections of the book are to be found in chapters 4 and 7—chapters primarily devoted to a description of the aims, functions, organizations, and financing of Canadian interest groups, and to an assessment of the groups' political effectiveness. The author concludes that the personal contact among the legislators, bureaucrats, and directors, as well as the shared normative values and the homogeneous socioeconomic background of the three elites, combine in such a way as to enable the elites to interact productively across a fragmented political culture and to maintain "the solvency of the Canadian nation." Noting the strength of the bureaucracy and the critical role it plays in policy formulation, Presthus's work lends support to Samuel Beer's maxim that "where the power is, there the pressure will be applied."

Apart from its modest contribution to an unexplored area of Canadian politics, the book is a disappointment. For one thing, it need not have been so long: judicious editing would have helped. For another, a careful check of the small, incidental facts would have given the reader more confidence in the work's total reliability. Accommodation is institutionalized, we are told (p. 9), through "the 'rule of proportionality' in appointments to the civil service." *Rule?* In the sense of a clearly defined and strictly adhered to principle? Scarcely. In supporting his claim of proportionality in the civil service, Presthus, in a footnote, cites a quotation from *The Vertical Mosaic* by John Porter (whose version of Canadian culture and society Presthus readily accepts). Yet Porter himself includes that very quotation as part of a larger section on the *under-representation* of French Canadians in the federal civil service.

The reader is told of a Quebec election in 1971 (when it was held, in fact, in 1970) in which the separatist *Parti Quebecois* received support from "some one-third of the electorate" (rather than the 23 per cent of the vote it obtained). He is given the wrong dates for Lester Pearson's government. He is informed that

Canada has had sixteen prime ministers, "precisely four" of whom have been French Canadian, when the numbers should be fifteen and three respectively. He winces at the description of British Columbia's Social Credit Government as *socialist*. He twice sees the CMA described as the Canadian Association of Manufacturers. He learns of the "virtually dictatorial power of the Cabinet which can, presumably, in Jennings's [sic] oft-quoted hyperbole 'do anything but turn a man into a woman'" (p. 148). The truth is that the quotation, or at least a reasonable variant of it, was de Lolme's, not Jennings's, and it was directed not at the powers of Cabinet but at the sovereignty of Parliament. Jennings, in fact, took the trouble to point out that de Lolme was in error, for if Parliament declared that men were women, *they would be*—so far as the law was concerned.

Trends are suggested and assertions made without the necessary supporting evidence. Is it true that "regional and subcultural criteria of representation are the *central* premises of appointment" (p. 9), to the Cabinet? That "the Canadian higher civil service is patterned rather closely after the British administrative class" (p. 34)? That Senators perceive their representational role in functional and occupational terms? That Canadian agriculture is an "underorganized" section of society (p. 111)? That "as government's net has spread more widely into new and highly technical areas, its reliance upon [agency-created] liaisons has inevitably increased" (p. 79)? That there are "increasing class tensions" (p. 255), in Quebec? That the property qualification for appointment to the Senate (\$4,000) is correctly described as a "substantial" one (p. 27)? Before claims can be made and arguments presented, further information is needed to answer such questions satisfactorily.

Presthus's definition of interest groups as "collectivities organized around an explicit value on behalf of which essentially political demands are made *vis-à-vis* government, other groups, and the general public" (p. 99), makes it difficult to conceive of political activities that could not be cited as "interest group" activities involving elite accommodation. Indeed, at one point we are told that even "parties themselves may be defined as interest groups or congeries of interest groups" (p. 60), although for the most part, a distinction is maintained between parties and interest groups on the grounds that having assumed a formal obligation to run the official apparatus, parties have a legitimacy and power unmatched by private interest groups. If, as the author states, the theory of

lite accommodation "relates only peripherally to party government and its ideological dimensions" (p. 60), then doubts may well be expressed about the appropriateness of elite accommodation theory to a study of a developed political culture with a highly institutionalized party system. Indeed, if, as seems to be suggested, party is simply a vehicle for control of office and recruitment of politicians, and if party policy is so indeterminate, and if interest groups are so essential to the operation of the political system, then what possible difference can it make which party is in office?

Elite accommodation in Canadian politics would be better understood provided: (a) more were known of the nature of the interest groups' contacts with other elites (the "case study" approach which Presthus uses in studying bureaucrats and directors ought to be no less appropriate in studying legislators, yet for some unstated reason legislators were not given an opportunity to record a case history during the interview as were the other two groups); and (b) the reader had been presented with somewhat more refined data on the 296 legislators in Presthus's sample. Was the legislator a newly elected member or a veteran? Was he from a safe or a competitive seat? These variables may have some bearing on the matter.

Of the non-words currently making the rounds in certain academic circles at least two are to be found in Presthus's work: "channelized" and "prestigious," the latter appearing no fewer than nine times. Finally, although both are discussed in the book, neither "lobbyist" nor "pressure groups" appears in the index.

JOHN C. COURTNEY

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**Front-Bench Opposition: The Role of the Leader of the Opposition, the Shadow Cabinet and the Shadow Government in British Politics.** By R. M. Punnett. (New York: St. Martin's Press, 1973. Pp. 500. \$17.95.)

Although the idea of a "loyal opposition" has been established in Britain for some time, the existence of a formal Opposition structure, as distinct from largely informal social contacts among leading opposition politicians, dates only from early in the twentieth century. About half a century later, in 1955, Attlee elaborated the structure further by designating Opposition spokesmen who were to speak for the Parliamentary Labour Party on specific topics on a continuing basis. This action went beyond merely perpetuating the existence of a Shadow Cabinet to establish a Shadow Government. Having traced the historical development of this Opposition structure, Professor Punnett

discusses its components. He focuses upon the Leader of the Opposition, the selection and composition of the Opposition teams, the organization and activities of the Opposition, and the extent to which Opposition spokesmen specialize in particular subjects. His concluding chapter includes suggestions for improving the Opposition's effectiveness.

Despite the difference in subject matter, Punnett makes several points similar to those of McKenzie in *British Political Parties*. While he does not go as far as McKenzie did in contending that the constitutional system requires a particular form of political organization, he does maintain that the Opposition structure takes the form it does because of the great influence of the British governmental system upon it. Speaking of the behavior of Opposition leaders, for example, he comments (p. 358), "Even in their utterances outside the House [of Commons], Spokesmen recognized the principles of individual and collective responsibility." Furthermore, like McKenzie, he indicates that the Conservative party is more in tune than is Labour with this governmental system. The Conservative Shadow Cabinet tends to function more nearly like the real Cabinet, tends to spend more time discussing policy. The differences between the main parties are not great, but Labour's concern with intraparty democracy does tend to make it behave in opposition less in accord with Government practices than is true of the Conservatives. Thus the transition from opposition to Government requires a greater transformation for Labour than for the Conservatives.

Curiously, Punnett indicates that Heath has been more true to the spirit of Attlee's innovation than has Wilson. The basic thrust of Attlee's action was to depart from the tradition of the amateur and move toward increased professionalism. By appointing as spokesmen those Conservative MPs whose Parliamentary activity already had established them as specialists in some area, Heath strengthened this move. Furthermore, on coming to power, he brought a substantial number of these spokesmen into the Government, usually assigning them the same area of responsibility they had had in opposition. In contrast, Wilson was inclined to designate MPs as spokesmen for areas in which they had not specialized previously. When he became Prime Minister, he included a smaller proportion of these spokesmen in the Government and, even when he did include them, was more likely to assign them to a subject matter different from their responsibility in opposition.

Thus in Punnett's view the Conservatives

make better use of their time in opposition to prepare for their return to power. Yet even in the case of the Conservatives, he maintains that the Shadow Government cannot properly be seen as a device to ready a party for a future period in office, but is better understood as primarily a means of enabling the Opposition to criticize the Government more effectively. This point appears to be one of the principal conclusions of this study.

To elaborate Punnett's findings beyond this is difficult, for despite the meticulous nature of his study, it lacks theoretical perspective. Although he tends to introduce each new topic with a series of rhetorical questions, these usually are merely questions of fact and not analytical hypotheses. He fails to explain why his questions are more worth asking than any others; he provides no linkage, no hierarchical ordering, of his questions. The bulk of the book is a collection of rather interesting details. And while one is inclined to believe that having all this information available is useful, yet it never is clear to what purposes the information has been gathered and presented. Had he done no more than clearly state a central thesis, his study would have been strengthened.

Punnett tends to be repetitious and to belabor the obvious at times. Too often one encounters such bromides as, "there is no fixed number of years that constitutes the ideal time for a party to spend in or out of office. Much will depend on the circumstances . . ." (p. 193).

These shortcomings should not obscure, however, what Punnett has accomplished in this book. He obviously has made a thorough study of the available biographies and diaries to collect information on the historical development of the Opposition structure. His use of *Hansard* to ascertain the extent of specialization by opposition spokesmen is an interesting study which helps to clarify the effect upon parliamentary politics of greater formalization of the Opposition. And although confidentiality prevents him from attributing particular comments, he is able to include a good number of useful insights obtained in interviews with several leading political figures. Thus this clearly is a substantial work which is likely to be regarded as a standard source of information concerning the Opposition in British politics.

JORGEN S. RASMUSSEN

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**Race and Politics in Urban Malaya.** By Alvin Rabushka. (Stanford, California: Hoover Institution Press, 1973. Pp. 148. \$6.50.)

This slender volume breaks new ground in Malaysian studies. The author commissioned

the first systematic public opinion poll ever conducted in Malaya on the topics of race and politics. The pioneering survey was completed in early 1967 by Survey Research Malaysia, private public opinion and market research firm affiliated with Gallup. The study is based upon interviews with randomly selected respondents in 352 households in Kuala Lumpur and 307 in Penang, Malaysia's largest cities. The Malayan city is seen as a microcosm of Malayan multiracialism, and the author seeks to explore the connections between the attitudes of his urban respondents and their political behavior at the local, state, and national tiers of the Malayan polity.

Professor Rabushka uses more than half of his text building a conceptual framework in which to display his data. The frame turns out to be more provocative than the survey portrait itself. The author re-examines F. S. Furnivall's concept of a "plural society," and adapts it into a theoretical model for his analysis. Like most students of Malaysian politics, Rabushka finds that Malaysia fits Furnivall's notion very closely; one finds residentially separated communities, economic specialization by race, uneven distribution of wealth, little intermarriage, and a single dominant political community. Rabushka defines politics in terms of choice and holds the ideal society to be one in which all choices are made in a market situation on the basis of supply and demand. Man is viewed as rational and motivated by self-interest.

The plural society model assumes the following characteristics: intracommunal consensus, intercommunal conflict, perceptual consensus and intense ethnic preferences. With sparse evidence, Rabushka posits these features in the political process of Malaysia's plural society. He observes further that despite racial cooperation both before and after independence, race has grown in salience as a political issue in Malaya as political entrepreneurs have risen to exploit the issue at the expense of the moderate multiracial Alliance coalition. The Government has increasingly resorted to electoral manipulations and the politics of redistribution in order to stay in power.

With this model framework and skeletal overview of the Malaysian political setting, Rabushka turns to his survey data on interracial values and social behavior. He finds that the urban Chinese prefers his own community, is culturally ethnocentric, yet paternalistically tolerant of the Malay, and that he isolates daily money problems as his major political concern. The urban Malay is found to be more outgoing and less culturally self-centered, but



also less tolerant toward the Chinese; as with the Chinese, the Malay identifies economic problems as the major political issues.

The data on voter registration, participation, and party preference produce few surprises. Malay registration in both cities is higher than Chinese. In Kuala Lumpur, Chinese vote more (36 per cent) than Malays (31 per cent) but in Penang, Malays (62 per cent) vote more often than Chinese (39 per cent). A huge majority of the Malays choose the dominant United Malay National Organization (UMNO) as their party preference, while only about a fourth of the Chinese picked the Malayan Chinese Association (MCA), the Chinese component of the ruling Alliance Government.

In his concluding chapter Rabushka suggests that Furnivall was right in his thesis that the races in conflict-prone plural societies can peacefully meet *only* in the marketplace, buying and selling in a laissez-faire exchange economy. Recent Malaysian government attempts to increase the administrative role in the economy can only lead to greater racial conflict. Rabushka prescribes the reduction of the role of government in Malaysian society and greater reliance on an expanding private sector. This provocative thesis is supported only by tendentious evidence and requires more systematic investigation before it can be seriously entertained.

The book's five major findings are very close to trite: "... government intervention in a community's cultural affairs may well lead to political turmoil"; "majorities often alter the rules of politics to suit their own ends"; "democracy is not easy to maintain in Malaya"; "multiracial living experiences do not necessarily promote racial tolerance or political unity"; and, "... higher levels of education ... do not ensure political unity or democratic stability" (p. 101).

Unfortunately, this book will fuel the fires of those with antisurvey research bias. For example, in interpreting the responses to a major question in his survey, Rabushka writes: "The results are revealing. First and most important, most of the respondents were unwilling to answer the question" (p. 68). On this base he later (p. 70) confidently reports findings. Moreover, the data analysis is quite primitive. No indicators of correlational strength are provided. The N for Malay respondents is too small for meaningful controls. The book is further marred by overstatement (e.g., "Indians ... are unimportant in the mainstream of Malayan politics," p. 56) and absurd dicta (e.g., "... archaeology is not a rewarding enterprise in Malaya," p. 16) and occasionally

error (e.g., the wrong date for the May 10, 1969, General Elections is used throughout the book). Despite its faults, the book is a stimulating addition to the shelves of the Malaysian scholar.

FELIX GAGLIANO

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**Politics and the Stages of Growth.** By W. W. Rostow. (Cambridge: Cambridge University Press, 1971. Pp. 410. \$9.50.)

Few books of the 1960s were as influential as Rostow's *Stages of Economic Growth*. Students of political development were particularly attracted to it, and some of them expressed the hope that Rostow would go on to relate political and economical change more explicitly. This book is Rostow's answer to that challenge: he intends it, as he says, to help create in the study of political development the same kind of common vocabulary and theoretical framework that now, in his view, unite students of economic development (p. 5).

Rostow approaches his task by way of a "framework for a general political system" and nine synoptic case studies (of Britain, France, Germany, Russia, Japan, China, Mexico, Turkey, and—in a separate chapter—the United States). His framework, which he intends among other things to fill out the "suggestive empty box" of Eastonian theory (p. 346), serves mainly to direct attention to what Rostow argues are the "three abiding tasks of government," namely security (external defense), welfare and growth, and domestic constitutional order. In the case studies, he attempts to examine how, at each of his well-known stages of economic growth, the respective societies mastered (or failed to master) each of these tasks. The aim of this enterprise, as he puts it, is not the attainment of "neat Newtonian propositions," but rather of "limited scientific generalizations," "the isolation of rough uniformities in types or species of cases" (p. 18).

The results unfortunately do not reach even this modest goal. The case studies cannot be managed on so ambitious a scale in a single book; and they often degenerate into a bare-bones presentation of tabular data (all too often not comparable cross-nationally) or, worse, into a capsulized rehash of elementary history. To be reminded, for example, that "from 1815 to 1871 France straddled the issues of monarchy versus republicanism, catholicism versus anti-clericalism" (p. 109), or to be told once again the story of the failed German revolution of 1848 (pp. 78–79), quickly becomes a bore, and an irrelevant bore to boot, since these discussions are put into no clear

relationship with the few historical generalizations that Rostow advances.

Those generalizations will in any event strike most students as pretty weak tea, much of it warmed over from the earlier book. We are told, for example, that the initial transition away from traditional society has usually been "initiated by some form of intrusion from abroad" (p. 62); that the "preconditions" stage of economic growth is "not usually conducive to democratic practices" (p. 287); or that the "preconditions" and the "take-off" stages contain temptations to "external adventure" and to "power . . . at the expense of less advanced societies" (pp. 63, 103). At a fairly early point, however, Rostow as much as admits that he will not be able to satisfy the hopes of his early admirers. Any "excessively refined linking in time of economic stages and specific political events," he contends, would be "unrealistic" (p. 98); yet some such "refined linking" was exactly what many had hoped for in this work. The failure to achieve it is nonetheless instructive. Rostow's work, it seems to me, offers us two important negative findings of significance.

As he himself concludes in an appendix that deals with other theories of modernization, there is little historical support for the usual sociological definitions of modernization that include such elements as increasing autonomy of subsystems, structural differentiation, and cultural secularization. "In truth," Rostow accurately observes, "a wide variety of social structures, suffused by quite different cultures, has exhibited a capacity to absorb modern science and technology . . ." (p. 343).

Equally valuably, Rostow's historical research tends to demolish one aspect of his own view, namely that the politics of modernizing societies can usefully be viewed as a Manichaean struggle between "pro-" and "anti-" modernizers. Rostow planted this way of looking at transitional politics in his original *Stages of Growth*; and, despite some reservations, he is not inclined to retreat from it now (see page 60). Its influence on political scientists and historians has been great, as, for example, the present debate about whether the German and Italian Fascists were "for" or "against" modernization indicates.

Yet Rostow's own efforts to apply his scheme to concrete history in this book seem to me to establish the general pointlessness of such questions. The Prussian reformers of 1807 represented, as Rostow notes, "a clear modernizing reaction" to Jena (p. 68) in many of their aims; yet they proposed land reforms that would have created a fragmented peasantry on

the French model, and only the subversion of these reforms by "reactionary" opponents allowed the subsequent rationalization of Prussian agriculture and the rapid creation of an urban work force. Can we say, even today, who in such a situation really favored modernization? And can we assume that the participants themselves could have answered the question?

"The rhythms of political life," as Rostow muses at one point, "have a timing of their own" (p. 98). Certainly they do not relate to economic development in any of the simple ways early theorists of modernization assumed; nor did those rhythms always take as one of their main themes the question of accelerating or retarding economic change.

These central concerns occupy only part of the book. Roughly its last third, although organized into three chapters, is in reality a series of essays on such diverse problems as American race relations, the origins of the cold war, the problems of contemporary Latin America, and the "youth revolt" of the 1960s. Rostow does not, alas, refrain from expounding here once again his remarkable views on the war in Viet Nam and on Southeast Asia generally, e.g., "Asians knew that their destiny was in the balance . . . would mainland China inherit suzerainty . . . ?" (p. 310). One possible explanation for those views is Rostow's manifest tendency to mistake the utterances of squalid little dictators for popular opinion; this reaches grotesque proportions in his ode to Korea's Park Chung Hee (p. 295). These concluding essays, taken as a whole, have their moments of wit and insight (the sketch of obstacles to Latin American economic growth is particularly penetrating); but they have little to do with each other or with the central subject of the book, and they too often fall into mere sermonizing.

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**Military Rule in Latin America: Functions, Consequences and Perspectives.** Edited by Philippe C. Schmitter. (Beverly Hills, California: Sage Publications, Inc., 1973. Pp. 336. \$12.50, cloth; \$7.50, paper.)

The reader will not find a common theoretical framework in this volume of essays on military rule in Latin America; the five contributors utilize a variety of approaches and methods, and address themselves to distinct problems and hypotheses. Moreover, as the editor of the volume acknowledges, neither the theories nor the methodologies employed are particularly new. And there is a major gap in the

coverage: none of the essays systematically focus on the socioeconomic and political structural factors internal to Latin America that may be associated with military rule. Despite these limitations, this is a very informative and interesting volume—one of the best on the topic. A broad range of current issues, particularly those related to Latin America's subordinate position in a hemispheric system dominated by the United States, are thoroughly and competently analyzed.

The volume is divided into three sections. In the first, "Changing Functions of Military Rule," Alain Rouquié analyzes the emergence of reformist military regimes by focusing on the specific historical experiences and roles of two military institutions and their relationships with the "inter-American military system" dominated by the United States. He argues that reformist military regimes have emerged only in countries where traditionally "the army played a hegemonial role" (p. 42) and that their emergence is related to a changing U.S. policy, specifically its willingness to "give up certain sectors [i.e., accept the nationalization of some extractive industries] in order to guarantee others [i.e., manufacturing investments]" (p. 38). Both the military reformers and the U.S. policy makers apparently also share a common concern for the necessity of strengthening the state apparatus in order to direct and control the process of social and economic change. Rouquié's essay proposes a series of complex and persuasive interpretations which merit further research. In particular, his all too brief identification of the socioeconomic and political characteristics common to the countries in which reformist military regimes have appeared needs further elaboration, as does the identification of the class and group interests represented by these regimes, a problem Rouquié never addresses directly.

In the second section, "Consequences of Military Rule and Military Aid," Jerry L. Weaver tests a number of current hypotheses concerning the policies of military governments (their propensity to defend the interests of traditional elites, of the middle classes, or of U.S. investors, and to increase military spending); P. C. Schmitter statistically analyzes the impact of three indigenous factors (gross domestic product, military intervention and political instability), and two exogenous factors (U.S. military aid and "military spending in adjacent 'competing' countries") on the evolution of military budgets.

Although informative and logically developed, Weaver's analysis suffers at times from a

reductionism not uncommon in behavioral political science. For example, he addresses himself to José Nun's argument that military regimes in contemporary Latin America fundamentally defend the interests of the middle class. Demonstrating that the policies of military regimes do not favor *all* sectors of the middle class, Weaver dismisses Nun's analysis. In fact, Nun argues that the interests of different sectors of the Latin American middle class are irreconcilable to the point of prohibiting the resolution of conflicts through the democratic process, therefore creating the necessity of military rule to defend the interests of the class as a whole, but not necessarily of all its sectors. While Weaver's concern for clarifying and specifying the concept of middle class is unobjectionable, his operationalization of Nun's hypothesis is hardly satisfactory. Weaver's essay, however, does raise significant issues, and his analyses of those hypotheses that can be quantified more easily are well executed.

Schmitter's essay on military spending is methodologically sophisticated. Utilizing a number of refined statistical techniques, he cogently demonstrates that the primary factor determining the level of domestic military spending in Latin America is the gross domestic product, while U.S. military assistance has a "tendency to raise domestic military spending above levels predicted by gross national product alone" (p. 163), and that military rule has not uniformly resulted in higher military spending (a conclusion corroborated by Weaver). His identification of arms races, however, remains inconclusive, and one of his results is open to serious question: that political instability has no effect on military spending. The index of instability available to Schmitter was so problematic that any results derived from using it are highly dubious.

In the third section, "Future Perspectives for Arms Control and Military Rule," Geoffrey Kemp analyzes the military capabilities of Latin American armed forces for waging international wars, in order to identify the factors that would have to be considered in any negotiations concerning arms control in the area. James R. Kurth elaborates a theory of "international hegemonial systems" to probe the relationship of United States dominance to military rule and underdevelopment. Both essays bring together a wealth of information and material. Kemp's analysis of international military capabilities and strategies is the first of its kind that I have encountered in the academic literature on Latin American armed forces. Kurth, as well as elaborating his own theory, provides a critical review of the major explana-

tory frameworks that have been used in analyzing the causes and effects of U.S. foreign policy in Latin America.

Anyone interested in questions concerning military rule in Latin America or U.S.-Latin American relations will find these essays stimulating and useful reading. The specific methodological techniques as well as the general interpretive schemes developed in the essays, while not entirely new nor uniformly excellent, are utilized in ways that will make this volume valuable to any student of comparative politics with an interest in the military.

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#### **Reflections on the May Fourth Movement.**

Edited by Benjamin I. Schwartz. A Harvard East Asian Monograph. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 132. \$4.50.)

The May 4th Movement, still vivid after sixty years, has long been debated both by its own leaders and by later historians. The pros, such as Mao Tse-tung, think that the movement was the beginning of a real (socialist) revolution in China, whereas the cons, like Hu Shih, deprecated it as being too politically oriented and charged that it badly jeopardized a more meaningful "New Cultural Movement" which was gaining momentum during the period.

Despite their differences in evaluation, however, few have questioned the basic issue of the Movement which, as Chow Tse-tsung has clearly pointed out, was the search for "new thought" to replace the old. Benjamin Schwartz and his collaborators are certainly right in stating that "not one of the famous 'conservatives' of the May Fourth period was really a man who lived wholly within old China or was prepared to defend the traditions of the past with the weapons provided by those traditions" (p. 3). In other words, every noted intellectual of the time was advocating some sort of "new thought" to substitute for the old which had been badly abused by the so-called Confucianists.

Professor Schwartz is also right to view the May 4th Movement not as "a mountain range rising up abruptly out of a flat plain, but simply a somewhat higher range in a long stretch of complex mountainous terrain" (p. 4). Unfortunately, members of his symposium have failed to carry this thesis any further. While recognizing the importance of the "complex mountainous terrain" existing before the "higher range," none of the scholars has attempted to explore the underlying historical relation-

ship between these features of the historical terrain.

From a historical perspective, it may not be an exaggeration to say that the May 4th Movement is the final stage of (to quote a Fairbankian term) "China's response to the West." The so-called "response" was limited to a handful of top intellectuals in the upper class and had been started by the early "barbarian experts" and their immediate successors, with Chang Chih-tung serving as their spokesman. It was only after the failure of their attempt at technological reform that K'ang Yu-wei and Sun Yat-sen began to promote an overall political restructuring of the nation. The former preferred peaceful means while the latter preached violence.

But the political restructuring work, which was finally completed in 1912 under Sun Yat-sen's spiritual leadership, failed utterly to accomplish the expected goal. The worsening situation in the early Republican period thus led a new generation of intellectuals to take another step. They were convinced now the causes of all China's previous failures lay precisely at the very root of its three-thousand-year old Confucian tradition, i.e., the "old thought." Being unable to explain the traditional evils more scientifically, the May 4th intellectuals, including the most moderate Dr. Hu Shih, appealed to emotion. And their emotionalism, which has been recently revived on the mainland with new vigor, matched comfortably the rising egoism of the modern educated youths who had inherited a long Confucian ideology of sensitivity to national political issues.

Members of the Schwartz symposium do not seem to look seriously into this immediate intellectual background of the May 4th Movement and instead seek only to explain the separate phenomena from a culturalist point of view. Among them, the most ambitious is Dr. Yu-sheng Lin, who produces lengthy passages to explain—quite unnecessarily—orthodox as well as Neo-Confucianist thought. While ignoring the immediate past, Dr. Lin is daring enough to predict the "future" of Chinese liberalism which makes his synthesis more ambiguous than debatable.

In summary, a thoughtful reader will not find much new information or even fresh inspiration from the book though more might have been hoped, considering that the symposium was held at the zenith of a double cultural revolution which was going on simultaneously on both sides of the Pacific.

Aside from the shortcomings of the book, however, one must give Professor Schwartz and

his collaborators high credit for assembling such an academic convention at a time when the scholarly world had swung into the confusion of overwhelming revolutionary cries which, as a Chinese rural proverb describes it, sounded like a gathering storm but came down only as little drops of rain. The book under review is undoubtedly one of such refreshing raindrops in a dry thunderstorm.

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**Mexican Business Organizations: History and Analysis.** By Robert Jones Shafer. (Syracuse, N.Y.: Syracuse University Press, 1973. Pp. 397. \$15.00.)

To appreciate this book, *Mexican Business Organizations*, one must realize that the author is concerned with those organizations that represent the private business sector in modern Mexico, not with the organization of individual business units. Those who are primarily interested in the internal operations of individual business units will be disappointed with this book; but if they persist long enough to get through the purely historical section, they will find the discussion of how private enterprise represents itself to other sectors, particularly the government, most informative. In this respect, *Mexican Business Organizations* is certainly one of the more important books recently published about Mexico. As indicated by the title, this study traces the history and development of the various organizations that represent the business sector and analyzes their operations. The analysis of these organizations examines not only their structure, but also their programs and doctrine.

The first chapter of this study, "Mexican Society and Private Enterprise," is, as should be, an introductory discussion of some of the prevailing conditions of present-day Mexico. Unfortunately, Professor Shafer tried to accomplish two objectives with his introduction and, as is often the case, fell short of both. On the one hand the first chapter is an attempt to highlight certain social conditions, political and economic characteristics, and interest-group relations. Although the author does quite a good survey, there is little in it for those familiar with the development of modern Mexico. For those not familiar with Mexican development, this short introduction is inadequate to give them the background necessary to appreciate and understand the very detailed examination of business organizations that follows. The author would have been better advised to have concentrated on the second objective, that of indicating the extremely important position and

necessary function exercised by formal business organizations in Mexico. The first chapter hints at this aspect in the discussion of interest groups and communications among institutions in Mexican society, but the role played by these organizations is not really made clear until much later in the book.

Although the author does not hesitate to use historical sketches to illustrate particular points in his analysis of the organizations that represent the Mexican business sector, the formal history of their development is given in chapter two. In that chapter the author traces the formation and subsequent changes those organizations underwent as Mexico experienced a dramatic revolution, a period of social reorganization, and finally a period of sustained economic growth.

Among the many interesting historical sections, two ideas stand out as extremely important for an understanding of why Mexico has developed in the particular way it has. The first of these is how business organizations have functioned as a means of communication between the government and the private sector. The foundation on which the use of business organizations for communication purposes was built is clearly described as the author traces the actions of the Minister of Finance Limantour, resulting in the enactment of Mexico's first Law of Chambers. It was this Chambers Law of 1908 which gave business organizations legal recognition and a special set of laws which was to govern their activities. Although the Revolution and post-Revolution changed many aspects, the communication function of these organizations continued and, indeed, was enhanced.

A second important aspect of the development of business organizations highlighted by this section is the flexibility of the Mexican system in responding not only to important economic groups but also to geographic developments. The first of these is illustrated by the formation of the Mexican Association of Bankers (ABM) in 1928 as a civil association not subject to the Chamber Law. The second is illustrated by the formation and subsequent important position of the Employers Confederation (COPARMEX) in 1929 primarily as a response of the Monterrey business community to the labor legislation of that period.

The remainder of the book, chapters three through eight, is intended to analyze how the business organizations function within the structure of the Mexican system. Unfortunately, some parts of these chapters are more concerned with description than analysis. This is particularly true with respect to chapter three,

"Organization and Resources," which because of its extremely detailed description is rather dull. In contrast, chapters four and five, in which the author examines the relations between organizations and the government, are both interesting and important. These two chapters do an excellent job of showing how Mexico has developed its own system of communication between the private business sector and the government. The contrast between the Mexican system of indirect communication, such as newspaper advertisements, and the American system of direct communication, through lobbying, is of particular interest in these days of ITT and Watergate. Perhaps the United States could do with a bit more institutionalization of the channels of communication.

Probably because all the programs and functions of business organizations in Mexico are secondary to their relations with the government, chapter six which describes these other functions will be less than absorbing for most readers. Chapter eight, the concluding chapter, does a good job giving the overall structure and showing how the institutional framework fits together.

To be sure, this book has its high and low points as viewed by different interests and, therefore, will attract several groups. Although it is not for the casual reader of Mexican history, it is essential reading for all serious students of either the political processes in Mexico or the operations of the private business sector of that country.

ROBERT T. AUBEY

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**Sinners and Heretics: The Politics of Military Intervention in Latin America.** By Mauricio Solaun and Michael Quinn. (Urbana: University of Illinois Press, 1973. Pp. 228. \$8.95.)

*Sinners and Heretics* is perhaps best seen as a sequel to Johnson's and Lieuwen's analyses of military intervention in Latin American politics written some ten years ago. Judged in relation to its predecessors, this book is a definite step forward, toward an explicit theoretical formulation and systematic use of empirical data. Nevertheless, it is in general both theoretically and methodologically inferior to more recent country-specific studies of the political behavior of Latin American militaries.

In two areas, Professors Solaun and Quinn make a substantial contribution. First, they rightly point out as a source of considerable confusion in the literature the failure to distinguish between interclass and intraclass coups.

Military coups may result from intense interclass conflict during periods of increasing political participation and demands for a redistribution of wealth and power or from an inability to manage purely political conflicts within the socially dominant groups. Although both the "participation problem" and the "organization problem" are frequently present in varying degrees in any particular coup, most authors have tended to focus exclusively on the factors explaining one type of coup, while denigrating the rather different arguments advanced to explain the other. Whereas studies of intraclass coups have tended to stress relatively static aspects of the Latin American context—political culture, weak electoral mechanisms, and governmental ineffectiveness—those analyzing interclass coups have focused on social mobilization, class conflict and violence, and U.S. intervention. During the period studied, 1943–1967, both types of coup have been common, and the authors argue convincingly for the necessity of integrating the two perspectives in any comprehensive treatment of Latin American coups.

Secondly, the authors incorporate into their analysis a cultural dimension which has been neglected as the study of Latin American politics has increasingly become a subfield of comparative politics. As indicated in the title, there is a peculiar style to political discourse in Latin America, a tendency to view one's opponents as not merely wrong or misinformed, but also as immoral (Sinners) or evil (Heretics) or both. This phenomenon is not restricted to periods of interclass conflict, where fundamental value premises are in question, but also pervades the traditional conflict between those in and out of power. The verbal totalitarianism characteristic of this sectarian view of politics contributes to military intervention directly through the rapid escalation of political conflict into all-or-nothing, life-or-death crises, as well as indirectly through the "diffusion of illegality syndrome" in which over time all political actors and institutions become tainted in the eyes of the military and the participant public. While the authors do not provide a convincing explanation for this phenomenon, merely citing it as evidence of a "non-secular political culture," they have perhaps provided an important clue to the question left unanswered by Huntington—Why is it that so few Latin American countries have been able to develop strong political institutions?

In other respects, *Sinners and Heretics* is less satisfactory. While the distinction drawn between the precipitating (situational) and endemic (structural) causes of military coups

is important, little is gained when the precipitants—among them, escalation of interclass or intraclass conflict, problems of governmental effectiveness and illegitimacy, U.S. intervention, personal interests of a leader, and industrial lag—are not mutually exclusive nor comparable units of analysis. In analyzing the structural causes of military intervention, the authors propose a curious amalgam of Huntington's and Almond and Powell's theories of political development, stressing the relationship between participation and organization, the latter variously identified with the capacity for political control, regulative capability, and structural differentiation or complexity of the regime. In postulating an equilibrium between participation and organization as a prerequisite for stability, the authors leave unanswered the question of how these variables could ever be measured to define empirically the equilibrium line or the degree of disequilibrium. Moreover, while "over-participation" may induce pressures to strengthen political organization or reduce mass participation by force, it is less certain that "over-organization" inherently generates pressures for a return to a point of greater equilibrium. From a theoretical point of view, the chief virtue of *Sinners and Heretics* is its reintroduction of the cultural variable to explain why the supposedly greater stability of democratic regimes, as an ideal type, is so seldom realized in Latin America.

As an empirical study of Latin American coups, the book is of limited value. With data drawn primarily from *Time* magazine, the *New York Times*, *Keesing's Archives*, and the *Hispanic American Report*, there are inevitably many inaccuracies, such as counting the Ecuadorian coup of 1947 as two separate coups. Moreover, the nonsystematic selection of coups to be studied provides a shaky basis at best for any generalizations. Out of a total of 63 coups, 23 of the 36 occurring prior to 1955 were included in the sample, compared with only 7 of the 27 occurring from 1955–1967. This renders meaningless the use of sample data to conclude that "intraclass conflict continued to rival acute interclass conflict as a cause of *golpes* [coups] during the postwar period," (p. 86) or that in cases of interclass conflict, "the military was only slightly more likely to side with the . . . reactionary groups than with the reformers" (p. 87). Finally, there is a deficiency in the logic which evaluates the importance of causal factor X by the percentage of coups studied in which X was judged to be present. By that logic, the desire to issue military manifestos is the most important cause of military coups, since such

documents are present in virtually every coup. As the authors note, we are still a long way from a comprehensive theory of military intervention in Latin American politics.

JOHN S. FITCH III

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**In Search of Southeast Asia: A Modern History.** By David Joel Steinberg, David Wyatt, John R. W. Smail, Alexander Woodside, Tom R. Roff, and David P. Chandler. (New York: Praeger Publishers, 1971. Pp. xii, 522. \$12.95.)

An understanding of Southeast Asia requires a knowledge of a variety of heterogeneous peoples in numerous unrelated countries: their cultural and historical developments, their social institutions, their religions, their languages and literatures, their systems of education, their economic and social structures, and their political ideas and ideals. To delve into Southeast Asia is to enter civilizations which are, in many instances, a marked change from what we know and have experienced in the West.

Various historians in the '60s endeavored to treat and interpret the entire region of Southeast Asia in their individual works—for example, the books of Hall, Cady, Bastin, Harrison, and Benda. Each contribution was limited because one person endeavored to extend himself beyond his country of specialty to cover the complex history of the entire region.

This book, however, is different. One of the reasons is that each of the six men who carefully researched this work has specialized in a particular geographical area of the region. Also, as each scholar has sufficient linguistic knowledge relating to his area, he has been able to use original source material in the language or languages of that area with which he is associated. Beyond that, this volume is not a traditional symposium in which each writer submits a chapter or chapters on a particular geographical area of the region. Rather, it is a work in which each scholar can take equal pride in the entire production because it was a joint intellectual pursuit. The editor, Associate Professor David Steinberg, has performed a masterful work in welding together the wide range of concepts of his colleagues.

The format of the work is presented in five parts: Part One—The Eighteenth-Century World—depicts the numerous indigenous institutions and manner of living which prevailed in Southeast Asia prior to the advent of the Europeans who imposed technical skills and an exercise of colonial power brought from the West. Within this interesting opening segment one is introduced to the peasant world, the up-

land peoples, authority and village society, provincial powers, religious life and leadership, traders and markets, Buddhist kings, Vietnamese emperors, Malay sultans, Javanese kings, and Spanish governors.

Part Two—New Challenges to Old Authority—covering the period from about 1750 to 1875, relates the appearances of various new powerful dynasties with their Southeast Asian leaders—dynasties which altered the power structure in almost every society of the region. This period was characterized by warfare, dynastic changes, population disturbances, and intensive struggles for power and wealth. Concurrently there emerged a commercial and political offensive by Europeans with greedy ambitions fed by the industrial revolution in the West. In time, new economic and social structures, plus new landowning elites, were to emerge all over Southeast Asia. By 1870 nearly all of Southeast Asia, except Thailand, was under European control and management.

Part Three—Framework for Nations—pictures the utter transformation of the region as political dominance by the West was established in the form of strong colonial states. Newly created political states cut across former indigenous lines of political patterns and separate societies with new dominant bureaucratic systems tied to the respective mother countries in the West. Beyond that, an economic change was altering the whole Southeast Asian way of life.

Part Four—Social Change and the Emergence of Nationalism—outlines those elements which brought about radical social change and served as the groundwork for the gradual development of nationalist aspirations that took definite form prior to World War II, with final fulfillment during and after the Japanese occupation of Southeast Asia. These elements took different form in the various Southeast Asian countries, but essentially had the same impact: urbanization, education, and language serving as channels of social and political change. The intensification of Western domination interfered variously in peasant welfare and social values, thus aggravating episodes of agrarian peasant revolt. The seeds of rising nationalism were found, however, chiefly in the towns, where the new urban elites—influenced by urbanization and Western education—generated nationalist ideas in political organizations which agitated for independent nationhood. It appears that another factor which augmented this embryonic drive of nationalism was

Part Five—The Preoccupations of Independence—the most stimulating portion of the entire historical account, relates the ironic role which Japan played in eventually destroying the Western colonial era. The British, French, and Dutch all endeavored to reestablish in some form their prewar imperialist controls, only to discover that the momentum of the nationalist movements prevented putting colonial Southeast Asia back together again. It is emphasized that whereas in the immediate postcolonial period the institutions of the Western democracies were adopted or emulated, recently there has been a drifting away from democracy in Southeast Asia because the younger leaders place more emphasis upon economic development than upon constitutional democracy. In this postwar arena one learns of the new trends in population change, urban developments, international politics, education, and the sociocultural change involving religion.

A full twenty per cent of the volume is devoted to an appendix enhancing immeasurably the value of the book: succinct discussion of the Southeast Asian languages; valuable footnotes with precise references; a glossary, arranged alphabetically; and for a Southeast Asia bibliophile, it was a delight to see a full 60 page evaluative bibliography—all arranged and geared to fit each of the preceding 35 chapters; and closing with a most useful index.

The authors are right in their opinion: asserting that "most Westerners are ignorant of and behave irresponsibly toward contemporary Southeast Asian history and the peoples who make it" (p. 342). This work goes a long way to dispel this ignorance and to create an international outlook of a higher order. This work offers a major contribution in understanding Southeast Asia, and places these historians in the front rank of social scientists in this generation.

CECIL HOBBS

*Consultant on Southeast Asia  
Library of Congress*

**Das Politische System der Schweiz.** Edited by Jürg Steiner with Erwin Bucher, Daniel Frei and Leo Schürmann. (Munich: R. Piper & Co., 1971. Pp. 241, index. DM 19.80.)

Four Swiss have collaborated to produce a book on Switzerland that, while it has some of the feel of a text, actually represents an attempt to bring maverick Helvetia into the House of Modern Social Science. Jürg Steiner, presently at Chapel Hill, and Erwin Bucher



the soil of the Swiss political system into "fruitful ground for general theory construction" (p. 9). While this is in part a necessarily descriptive enterprise, particularly in Bucher's dry rehearsal of Swiss history and Schürmann's parochially traditional constitutional section, both Steiner and Frei focus less on Switzerland than on the general propositions they believe the Swiss case precipitates, and the systematic hypotheses such propositions suggest. Steiner, whose contribution on "Political Processes" occupies nearly half the book, offers fifty-eight "propositions" distilling the Swiss political experience with parties, economic groups, voluntary associations and the mass media—all in a rigidly adhered to framework of "articulation and aggregation of interests" and "transformation of interests into [legislative] decisions" (pp. 75–76).

With the exception of the massive study of integration in Switzerland undertaken by Karl Deutsch and Hermann Weilenmann (whose death has apparently postponed publication), and Steiner's own study *Gewaltlose Politik und Kulturelle Vielfalt* (published originally in 1970, available now in a rather unfortunate translation called *Amicable Agreement Versus Majority Rule*, 1974) from which most of the material in this collaboration has been taken, there are no studies that fasten onto Switzerland as a system model for the testing of comparative hypotheses about input and output functions in legislative systems. The sparse literature available in English includes only text-style descriptions like George Coddington, Jr.'s *The Federal Government of Switzerland* and C. J. Hughes's *The Parliament of Switzerland* or thematically specialized case studies like R. C. Brook's prewar *Civic Training in Switzerland*, Hans Kohn's elegant *Nationalism and Liberty: the Swiss Example* and Kenneth McRae's *Switzerland: Example of Cultural Coexistence*. Steiner and his colleagues remedy this oversight. The question is, to what effect and with what rewards?

On the affirmative side, it can be said that among the several small nations that have insisted on their uniqueness and their immunity to the probes of comparative researchers, Switzerland has carried its claims farthest: that is to say, too far. Steiner's attempt to recast its celebrated idiosyncrasies—cultural plurality, religious diversity, maddeningly cross-cutting cleavages, direct democracy, extreme decentralization, communal autonomy and national neutrality—in a systematic language that permits comparison, generalization, and testing can, on this score, only be welcomed. If his rendering of the system model is some-

what laborious, a too-conscious mimicking of the no longer so fashionable Almond-Powell-Easton-Dahl paradigm, it is nonetheless accurate and for the most part aptly deployed. If his use of Swiss materials glosses over substantive controversies in favor of formal processes that lend themselves to comparative generalization, we nonetheless sense that he knows the Swiss scene well, and appreciates the obstacles its peculiarities present to the systematic observer. The overriding theme of his section, which ought to be of genuine interest to comparativists, is that Switzerland's political system depends on participatory processes of *amicabilis compositio* rather than on majority rule—on collaborative consensus rather than pluralistic interest promotion as in other Western democracies. It is this feature of the system that gives to Steiner's otherwise commonplace descriptions and predictable hypotheses their special interest.

On the deficit side, many of these propositions and hypotheses seem trivial, tautological or merely forced: extravagant reformulations in systems language of the least surprising mundanities; as, for example, in the case of the hypothesis (p. 88) that suggests that parties will accept institutions that deter the fall of governments to the degree that they participate proportionally in governmental power (presumably the lesson "don't cut off your nose to spite your face"). Where Steiner is too formalistic, however, his colleagues are there with their more substantive historical and constitutional contributions providing a context, telling us as Schürmann does (pp. 54–55) that "Rousseau is more helpful for the understanding of Swiss institutions than Kant or Hegel," and themselves elaborating this Rousseauian context. Daniel Frei's final essay on Switzerland as part of the international system is particularly useful, for without surrendering the systematic approach urged by Steiner, he manages in just a few pages to put substantive problems of neutrality and nation building in a theoretically universalist context where the interface between internal cultural and ideological issues and external political and historical issues is sharply silhouetted: the place of foreign workers in a multicultural nation fearing for its national integrity, the role of military neutrality in an ideologically committed country, the pressures of international economics on a traditionally independence-oriented state—each of these dilemmas is explored with insight into the peculiarities of Swiss history but also with an eye on the common features of comparative political systems. Steiner has, then, been well-served by the collaborators whom he

too has served well. Since comparativists are no longer expected to read un-American languages—(statistics excepted, it is the Esperanto of imperial science)—some publisher might want to consider a translation of this well-conceived little book. They could do worse (and, no doubt, they will).

BENJAMIN R. BARBER

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**Journey Between Two Chinas.** By Seymour Topping. (New York: Harper & Row Publishers, 1972. Pp. 459. \$10.00.)

As a young reporter in China during the 1940s, Seymour Topping witnessed the collapse of the Nationalist government and, incidentally, met his wife Audrey, daughter of Canadian diplomat Chester Ronning. Nearly a quarter century later, his father-in-law's personal intercession with Chou En-lai enabled Topping, now Assistant Managing Editor of the *New York Times*, to obtain a visa to return to China. This lengthy volume (431 pages of text and appendices) recounts his impressions of the "two Chinas," twenty years apart, plus those of an interim assignment in Vietnam during the early 1950s.

Topping's account of the battles, political as well as military, preceding the demise of Chiang Kai-shek's government is not new, but it is well told and his tales of conversation with the great and the small of those affected by the momentous events then taking place should command the interest of generalist and specialist alike. Similarly, his glimpses of life in Saigon before Dien Bien Phu are fascinating, though somewhat marred by gratuitous sermonizing. Statements such as "the American purpose in Southeast Asia should be the restoration of peace and respect for borders, and the fostering of a community of independent, self-determining and viable societies" (p. 189) are unhelpful as policy guides for the future and in fact were the very goals that those who led America into the Vietnam war had in mind. This is a minor part of the book, however, and Topping's account moves smoothly and reads well up to this point.

It is with his return to China in the late spring of 1971—after ping-pong diplomacy but before Henry Kissinger's secret trip to Peking—that originality becomes harder to sustain and the reader's interest may flag. The China specialist will have read many such accounts of guided tours and banquets. The nonspecialist may find Topping's flashback technique confusing, as a trip to a commune recalls the agricultural policies of the past or a visit with a

certain leader is rounded out with a summary of the political activities of the last decade.

For the most part, Topping lets the story of his five-week trip tell itself. Skepticism is wielded lightly, as one would expect from one who hopes to strengthen Sino-American contacts from the uncertain basis they rested on at this time. Chinese officials say exactly what they are expected to; the common folk interviewed, generally chosen at random from whatever commune or factory being visited at the time, tell their stories on cue. This woman's father was murdered by the evil landlord before liberation, that man's young sons were conscripted by the KMT and never heard from again—in repetitious and none-too-subtle variations on Mao's dictum, "Remember past bitterness and contrast [it with] present sweetness." In many ways, this Maoist slogan also summarizes Topping's book; there are repeated comparisons drawn between the ragged, starving refugees of the past and the rosy-cheeked, enthusiastic peasants of today. While the comparison is undoubtedly valid in the main, it is perhaps less than completely accurate to describe this as a *Journey Between Two Chinas*—it is more precisely a journey between a China in the final convulsions of transition and a China in a not entirely typical period of stability. One wonders, for example, if the author would have found the same plethora of apple cheeks and smiles during the period of economic crises which followed the failure of the Great Leap Forward or during the struggles which accompanied the Cultural Revolution. Even as Topping was visiting, the drama of Lin Piao's alleged plot to assassinate Mao and Lin's eventual fall from power was being played out. As the Chinese People's Republic's own media have shown, venality and overreaching ambition have not been confined to the Nationalists.

It is somewhat unfair to criticize Topping for not making these points. Good diplomatic relations are not built by highlighting the deficiencies of a country and Topping was, after all, present as guest and journalist, not as political scientist and critic. Moreover, the book is no mere mouthing of official propaganda. Topping has attempted to verify his hosts' claims for the value of acupuncture in curing deafness by speaking to Western medical experts, and his reservations on the value of revolutionary art are clearly, though politely, stated. In addition, he shows himself an acute observer, noting for example that the Anshan steel mills are being supplied with low grade, locally mined ore in contrast to the high quality ore which the Japanese founders of the Anshan complex had

hauled to it from Hainan Island, far to the South, thirty years before.

In summary, this is one of the better books of its type. The specialist will find in it little that is new, however, and the generalist, little that he or she could not find elsewhere. The book deserves to be read for what it is—an interesting compendium of recollections.

JUNE TEUFEL DREYER

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**Hitler's Strategy, 1940–1941: The Balkan Clue.**

By Martin L. Van Creveld, (New York: Cambridge University Press, 1973. Pp. 248. \$13.95.)

Hitler never considered the Balkans as a central factor in his overall military strategy. Why, then, did he launch the Balkan campaign in 1941? In a well-researched, relatively brief volume, Professor Martin L. Van Creveld has advanced some convincing answers. He tells us that while the Balkans played no central role in Hitler's military thinking, they did provide the Third Reich with vital raw materials. From the beginning, then, Hitler recognized the need to establish a clear German sphere of interest in the Balkans. He hoped this might be done through diplomacy, since a Balkan war would disrupt the flow of raw materials to Germany.

Failure to knock Britain out of the war, and the ultimate abandonment of plans to invade the British Isles forced Hitler to reevaluate his entire strategy, including his plans for the Balkans. Thus in the summer of 1940 he adopted a "peripheral strategy" aimed at forcing Britain out of the war by systematically depriving her of vital positions at Gibraltar, Suez, and the eastern Mediterranean. Under this new strategy, southeastern Europe remained what it had been for Hitler, a source of raw materials. But the Axis occupation of Greece now became the key to expelling the British from the Near and Middle East. Greece became, for a very short time, the focal point of an indirect offense against Britain. Van Creveld argues, contrary to some other accounts, that Hitler had no objections to Mussolini's attack on Greece, until it proved to be a disaster.

By November, 1940, Hitler was becoming more concerned with his projected attack on the Soviet Union. In the context of "Operation Barbarossa," the Balkan situation took on new meaning. The peripheral strategy against Britain gave way to the requirements of the more important conflict with the Soviet Union for the conquest of *Lebensraum*. Whereas Greece had been part of an indirect offense against Britain, it now became little more than a worrisome

thorn in Hitler's right flank in the Russian campaign. The entire Balkan area, in fact, had to be made secure so as not to endanger Hitler's advance into Russia. Since Mussolini had bungled the job in Greece and Albania, Hitler felt obliged to send in his own forces. The Greek campaign required the massing of troops in Rumania and Bulgaria, and there was always the threat of Yugoslav and Turkish intervention. A potentially anti-German *coup* in Yugoslavia in March, 1941 convinced Hitler of the need to occupy that country, too. Thus, the Balkan operation, which was nothing more than a protective maneuver to secure the right flank for Hitler's Russian campaign, ultimately involved the commitment of large amounts of manpower and equipment. Van Creveld argues convincingly that such a large-scale commitment was unnecessary; Hitler had overestimated the strength of Yugoslav and Greek opposition.

It has been argued by others that Hitler's Balkan campaign seriously delayed the beginning of the Russian invasion, thereby contributing to the ultimate defeat of the Third Reich. Van Creveld maintains that the Balkan campaign had practically no adverse effects on "Operation Barbarossa." The Balkan campaign went much faster than the Germans had expected; many of the divisions originally earmarked for the Balkans never saw action—some were never sent there. If the Russian campaign was delayed, the cause was a general shortage of equipment for Hitler's armies, not the Balkan campaign.

The Balkan campaign was a success, the last of Hitler's successful *Blitzfeldzüge*. It was, as Van Creveld indicates, a totally rational strategy, remarkably free from the ideological and racial theories which often influenced Hitler's reasoning. The author attributes this rational attitude to the fact that, "Greece and Yugoslavia were, after all, only a small item in Hitler's strategy" (p. 183). This book is, nonetheless, an important contribution to the scholarship on the Third Reich, with some particularly good insights into the relationship between Hitler and Mussolini.

DAN P. SILVERMAN

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**Cast, Race and Politics: A Comparative Study of India and the United States.** By Sidney Verba, Bashiruddin Ahmed, and Anil Bhatt. (Beverly Hills, Calif.: Sage Publications, 1971. Pp. 279. \$12.50.)

Ever since Warner, Davis, Gardner, and Gardner's *Deep South* (work unfortunately not referred to in this volume), comparison of the

position of American blacks with that of Indian harijans (untouchables) has intrigued social scientists. The obvious points of the analogy—both are at the bottom of their respective social hierarchies, they got there by birth, and certain kinds of social interaction have been forbidden between them and the dominant sections of society—have been reported from time to time (see e.g., J. Dollard, *Caste and Class in a Southern Town* and H. Isaacs, *India's Ex-Untouchables*). Nevertheless, very little systematic comparative work has, in fact, been carried out.

It is appropriate that one of the outstanding American students of comparative politics should also be one of the first to attempt an analysis of political participation among both groups. Unfortunately, although some of the analyses are ingenious, and although two of the authors are Indian, the book exemplifies the aridity of a student exercise curiously uninformed by an understanding of Indian society and culture, tainted by the projection of American images into an Indian context and by a certain measure of sloppiness. The net effect is analysis in a bell jar, an elaborate game, bound by its own rules and conventions, but not coming to grips with the reality it seeks to understand.

To illustrate these criticisms, let us consider the treatment of only one variable of many—group consciousness. Group consciousness is an important concept in this study because it represents one route to political mobilization, particularly for deprived groups. The authors report that harijans have substantially lower levels of group consciousness than blacks, although they do vote with surprising frequency. Such an assertion entails at least two major problems. The first concerns the nature of Indian society; the second, the peculiar method the authors have chosen to operationalize this crucial concept.

The first problem is that there is no way in which it is reasonable to consider harijans a group as opposed to an aggregate. "Harijan" is a collective label, imposed from outside, that covers hundreds of smaller groups called castes (or jatis). Each of these groups has a strong internal identity. Group in-marriage is prescribed; if there are sufficient numbers, members of each group live together; and if one asks them what they are, they respond with their caste name. As the authors themselves report in the curiously dissociated historical section of the study, some political leaders have tried to forge a shared identity from the experience of abuse and have been unsuccessful.

Any assertion suggesting that these people

lack group consciousness, then, must be wide of the mark. Rather, they simply lack the particular group consciousness that the authors are trying to impose upon them. It is about as reasonable to expect a Chamar, a Bauri and a Nattar (all untouchable castes) to consider themselves part of the same group as it is for a Polish American, a Norwegian American and a Lebanese American to identify with each other as fellow "ethnics." (A few in universities probably do so. Those in analogous positions in India probably do so as well.)

In a way, however, this discussion is beside the point, because the operationalization of group consciousness simply does not do what it purports to do. These are the items that comprise the measure:

In many communities there are groups opposed to each other. Thinking about this community (mention name of local government unit), what are the major groups that oppose each other here?

When you think of your own situation and that of your family what are the problems that concern you most?

What is the most important problem of this community?

Now think about the country as a whole. What are the most important problems facing the United States (India) today?

(And in India) What groups or organizations are most influential in this village/town?

With the exception of the first item, which itself contains a suppressed premise I will discuss below, none of these can be considered a measure of group consciousness where group is defined as one's caste or race. They are measures of tensions, or problems, or sources of conflict, but there is no inherent reason why these tensions or problems ought to be based on ascriptive group membership. Even the first item operates under the assumptions that group consciousness is forged in conflict, which is not always true, and that group consciousness must be consciousness of one's race or caste rather than one's position in the labor force or some other such variable, which is also not always true.

What we see here is more than ignorance about how one takes concepts and uses them across cultures. It is a peculiar chauvinism that projects the current milieu into other settings. *Right now* (as measured in 1970), this measure would probably correlate highly with group consciousness among American blacks, and might even be a good index of it. That is, at the time of the study, American blacks and whites were defined as adversaries, and blacks were in fact highly concerned about their treatment by whites. A black in America can with

some justification see race problems as the most important problems in his society today. It is sheer ethnocentrism, however, for a researcher to anticipate that the pattern would be the same in India, where problems of famine, disease, and general survival loom so large. Even when group conflicts occur in India, it is not always easy to see them as caste conflicts. Conflicts between workers and managers or land owners and landless laborers may often—where caste is relevant at all—represent the dominant caste fighting all the others, touchable and otherwise. Politics in Tamilnadu follow this model, where Brahmins and non-Brahmins (which means everybody else) are in conflict. In summary, the authors' operationalization may "work" in American society, but is completely inappropriate elsewhere.

In their charming and disarming introduction, the authors discuss the reasons both for and against comparing caste in India with race in the United States, and judiciously settle for some middle ground. I share that middle ground, but as the above discussion illustrates, comparisons have to be made with sensitivity both about how other societies are organized and how one's perceptions of his own society influence the judgments one makes.

The authors' lack of sensitivity is reflected in another methodological decision, namely, to treat harijans as a homogeneous group and to report what are, in effect, averages. One of the striking features of Indian political life is the enormous range of political participation and effectiveness of different untouchable castes. The Mahars of Maharashtra, for example, have produced important leaders and are a major force in Maharashtrian politics. Other groups, such as the Bauris of Orissa, behave more as low-status groups are expected to. Comparisons among the deprived groups themselves may shed more light on the political process and political participation of deprived groups as a whole than does a method that treats them as homogeneous and undifferentiated. The authors' own findings suggest that this strategy would have been more sensible than the one they chose: "The difference in participation rates between the deprived and dominant groups is relatively small to begin with and disappears when one controls for a composite measure of socioeconomic status . . ." (p. 161). My guess is that comparison of the low participators in the high participation group and the high participators in the low participation group would yield more interesting findings.

The mechanical quality of the analysis is illustrated by the authors' discussion of income

differences between the lowest stratum in each country. The authors report: "Eighty-one per cent of harijans are concentrated in the two lowest income levels, as compared to only fifty-three per cent of blacks." Inspection of the relevant table (p. 90), however, reveals that this difference is an artifact of the way they have located the cutting points between income categories. For American society, the two lowest categories have been set so as to include only 20 per cent of the total population; for India, by contrast, the two lowest categories are grouped to contain 60 per cent of all Indians, with five additional categories demarcated for the remaining 40 per cent of the population. The reason for setting the category boundaries as they do is not obvious, and the authors offer no explanation for it. Nonetheless, following the rules involved in such an exercise, the authors even go so far as to report a measure of association, Somers' *D*, for race to income in the U.S. (.38) and caste to income in India (.34). Aside from the fact that this report almost contradicts the verbal statement, the use of a measure of association in such a case strikes me as absolutely pointless, given the way the data are organized. The mechanical trotting out of such measures whenever one has a table to present is one of the more dreary and misleading practices of those playing the data game. (Nowhere in the report do the authors present a discussion of the strengths and limitations of Somers' *D*, although it is the measure of association they rely on most heavily.) This is only one table, and not the most important. But it illustrates a more general problem with the analysis.

All of this work is in the service of building models as a guide to understanding participation in the political process. "The data on white Americans," the authors observe with perfectly straight faces, ". . . fits the normal model fairly well" (p. 215). Blacks follow a "group consciousness" model developed on the basis of responses to the questionnaire items discussed above. But, alas, "it is hard to characterize caste Hindu voting unambiguously in terms of our four models" (p. 224). And, "the path to voting for harijans is a close match to the partisan mobilized model." The reader can only gape at the unself-conscious narrowness of cultural orientation that allows model-building of this type.

It is going outside the framework of the authors' analytic approach to raise one final question, but it seems to me important to raise it nonetheless. This is the question of the validity of interviews with low-status Indians. Historically, Indian villagers have had unfortunate

experiences with high-status outsiders who ask questions. I and some of my Indian colleagues have received responses to interview questions that could be roughly paraphrased as: "Please go away. I am an honest and law abiding man. I do my duty as a citizen, and I do not know anything. Please go away." Recognizing that one might have been given that sort of response would enable one to account for many of this study's findings about harijans better than the authors do. They report that harijans vote in surprising numbers, and for the ruling Congress party. But harijans do not know anything, the study reports, and they seldom participate in any political activities besides voting. (A state-by-state breakdown would have been useful here. Did untouchables in Bengal report voting for the Congress or for the Communists?) The authors' explanation that they are mobilized by external forces seems inappropriate. Historically, low-status people—American blacks are one example here—have acted "dumb" and "docile" to avoid trouble. Untouchable respondents may be doing the same. Active political participation in anything besides voting may get them into trouble—even attempting to vote sometimes does—as may telling high-status outsiders (the interviewers) what they really think. Consequently, one cannot assume that because harijans do not report their opinions, they have none. In this context, assuming that observable political activity must result from mobilization from outsiders is an explanation that does not explain, but that may have the consequence of minimizing the extent of harijans' genuine feelings of grievance. Further, it lends support without documentation to the "outside agitator" explanation of political demands so popular with ruling groups.

The authors of this study have commendably attempted systematic analysis of a problem that has intrigued social scientists for the last thirty-five years, and they have provided us with valuable data. Unfortunately, their lack of sensitivity to the issues involved substantially decreases the usefulness of the study.

RICHARD P. TAUB

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**Revolucija Koja Teče—Memoari [Memoirs on a Flowing Revolution].** By Svetozar Vukmanović [Tempo]. (Belgrade: Komunist, 1971. 2 vols. Pp. 938. No price indicated.)

The struggle is ended, the bourgeoisie defeated, and the former illegals have stacked their arms and retreated. Enter now from backstage the miscellaneous array of politicians, headquarters men, and arrivistes, all eager to

assist their victorious Party. With cunning and duplicity, the representatives of this "papier-mâché world of proletarian revolution" gain control of one echelon after another in the emerging state. The whole process, as General Vukmanović (known in the Party as "Tempo") views it, can have only one outcome: a lost cause, as has been the case with every revolution the Communists have won.

But according to the author, who brings to this study an impressive background, this has not held true in Tito's Yugoslavia. For several decades the general was a leading official of the Alliance of Communists of Yugoslavia (SKJ; formerly the CPY) and, according to Stalin, a "dubious Marxist." He served Tito in a variety of diplomatic, military, and political capacities, and was a member of the Politburo of the CPY.

In 1941, he writes, two difficult tasks faced the Yugoslav Communists: to get their movement organized and to force the class enemy to slip the noose around its own neck. Both struggles claimed the lives of many of the best cadres. Learning very early that the Bolshevik manual on revolution was dated, the rebels had to improvise a new manual under fire. Here Tito's contribution was particularly significant. He profited from the political illiteracy and organizational weaknesses of his enemy, says Vukmanović, and, in particular, from the peculiar policies of the Western allies, who while sponsoring Nationalist Draža Mihailović, were helping Tito's Partisans.

In the account of his stay in Macedonia, 1943–44, Vukmanović offers a classical pattern of Communist penetration into no-man's land. Even though he had only a passing acquaintance with the customs or the language of the people or the terrain, he was sent by Tito to set up Partisan units, to ignite a rebellion against the occupation forces, to neutralize the Četniks, and to settle a score with the Communist parties of Bulgaria, Greece, and Albania—not a small assignment. The wonder is that he was so successful on all counts—largely, he says, because of Tito's astute political directives, his own resourcefulness, and the blunders committed by the adversary.

After the victory, as chief political commissar of the People's Army he undertook the transformation of Partisan detachments into a modern army. In his frequent contacts here with Soviet generals, he was appalled by their seeming lack of appreciation of the contribution of foreign Communists to socialist victory. He began to suspect at this point that the Red army had become merely the army of a big

power rather than an armed sword of the proletarian revolution.

His next assignment was to the economic front, where he became one of the principals in the building of socialism. Convinced that the only way out of utter chaos lay in transplanting Soviet patterns to Yugoslav soil, he established an impressive network of commissions and consultative bodies, all under the thumb of the Party. The first Five-Year Plan set ambitious goals for collectivization of private farms and for drastic reorganization of every sector of the economy. The statistics, when released, fell far short of convincing the leaders that the country was on the road to recovery. On all sides they saw shortages of basic commodities, empty shelves, inefficient transportation, and insufficient energy to meet growing demands. The peasants were resisting forcible collectivization, while the workers seemed to have as little say as before.

Following the 1948 dispute with Stalin, the CPY found itself forced to build its own road to socialism. The promulgation in June 1950 of the Law on Workers' Councils marked the turning-point. Slowly but relentlessly the worker and the agricultural proletarian acquired the weapons to put the bureaucrat in his place. Now, for the first time, says Vukmanović, Marxism took on a humanistic tinge, with the forces of resistance putting up a stubborn fight. For this reason the revolutionary fire had to be constantly rekindled.

An era of reform and innovation followed that ushered in a series of Yugoslav NEP's. Institutions were changed, business enterprises were reorganized, and the social fabric and the economy were subjected to constant alteration. Through it all the SKJ (CPY) managed somehow to rule politically without governing. In short, it kept the revolution flowing—a situation that still exists in today's world. Vukmanović's reflections in this context are frequently simplistic, his summaries much too sketchy; and he tends to belabor his arguments and to dwell on trivia. He also makes generous use of propaganda in describing the governments' achievements.

Nevertheless, his thoughts on the rise and fall of a professional revolutionary merit attention. The Marxist professional, Vukmanović tells us, must be both artisan and artist of revolution: to retain his gains, he must become accomplished in public relations and in intra-party diplomacy. He must be willing to "look all his personal problems in his attaché case"; and for Vukmanović this involved his acquiescence in the execution of his own brother. In

his relations with his party comrades, the author styles himself a Bolshevik. But when they committed serious political sins, he turned promptly against them.

By bringing to light an abundance of data not generally available, General Vukmanović's study makes a valuable contribution to the area expert as well as to the student of East European affairs, and the author's active participation in the events he describes gives his work added value.

MILOŠ MARTIĆ

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**Agrarian Development and Modernization in the Philippines.** By Dov Weintraub, Miriam Shapiro, and Belinda Aquino. (Jerusalem: The Jerusalem Academic Press for the Institute of Asian and African Studies of The Hebrew University of Jerusalem, 1973. Pp. 107. \$5.00.)

The slim volume is one of a proposed set of case studies on the Comparative Analysis of Processes of Agricultural Development and Modernization. It consists of two rather disjoint parts—a brief chapter presenting a summary of the conceptual structure underlying the entire project and a long chapter on the Philippines. The conceptual chapter is a nearly complete reprinting of an article by Weintraub in *Rural Sociology*. The empirical chapter is drawn mainly from a secondary analysis of data and publications. These two parts are not linked in any persuasive way. Furthermore, the theory itself is not a particularly useful guide to analysis. The empirical chapter, however, is a useful introduction to the literature on the rural Philippines and Philippine politics.

The analytical framework is drawn from Shils's distinction between the center and periphery of a society. This distinction is supposed to be "analytical and not geographical" (p. 15) but "periphery" really means "the rural sector" while the meaning of "center" is less clear; often "center" refers to the activities of the central government, to the shared values of the national elite, to the capital, and so on. The analytical framework leads to five questions: (1) How much does the center care about rural welfare and development; (2) How effective is the center in carrying out its aims; (3) How capable is the rural sector of mobilizing resources for its own development; (4) How much does the rural sector participate in the Center's decision making; and (5) How well integrated are overall national responsibility is the periphery. For a particular country the theory specifies five dichotomies charac-

terizing the country as high or low on each of these characteristics of the two sectors. The profiles are then asserted to relate to prospects for rapid or sustained rural development of one sort or another.

The problem with the analytical construct is that none of the significant variables can be measured in any satisfactory way. How do we know if a country's rural sector is or is not "capable of mobilizing its resources for its own development"? We might measure the proportion of agricultural capital investment generated from rural savings. This is hard to do, and it is not done in this volume. How do we tell if the rural sector exerts a large or small influence on policy making in the center? Analysis of sectoral power relations across countries with quite different political systems is not easy, and this volume doesn't do it. How do we tell if the organization of the rural sector is such as to create "a separate and differentiated collective consciousness"? (p. 16). And so on. Finally, how are we to measure the output of all of this, the rate of rural change, or its pattern, or whatever?

The review of secondary sources on the character of Philippine social and political structure is competently done. Whatever deficiencies are exhibited in this review are drawn directly from the deficiencies of that literature. For example, there is a general tendency to homogenize the "rural Philippines" into a stereotype consistent with common theoretical constructs of a "peasant society." In the stereotyping process all of the sharp and interesting regional variation is lost, the range of economic activity found in rural areas is ignored, the differences between industrial-type sugar production and rice farming are glossed over, and so on. Drawing from secondary sources also makes it impossible for the authors to evaluate the data on which their conclusions are based or to make any adjustments. Since the literature on Philippine politics has focused heavily on the role of the elite and their machinations, we find the same model in Weintraub et al., without any separation of the roles of barrio and national elites and without much real understanding of the ease with which ordinary country people can and do approach the national politicians with their problems and demands.

Despite these shortcomings, however, the empirical section of the book will be useful to those wishing a quick introduction to the basic literature on Philippine politics and society.

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**Vietnam and the Chinese Model: A Comparative Study of Vietnamese and Chinese Government in the First Half of the Nineteenth Century.** By Alexander Barton Woodside. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 358. \$9.95.)

Alexander B. Woodside's *Vietnam and the Chinese Model* is the first serious work of American scholarship on Vietnam in the early nineteenth century. As such, it is a major event in the development of Vietnamese studies in the U.S., along with David G. Marr's *Vietnamese Anticolonialism* (Berkeley: University of California Press, 1971), also published in 1971. Professor Woodside brings to this pioneering task an obvious skill and sensitivity in the portrayal of cultural patterns on the basis of literature and court records of the period. He has an enviable knack for finding the social and cultural significance of even the most obscure regulation. Moreover, he is a gifted student not only of Vietnamese but of Chinese and Japanese language and literature.

It is precisely because of the author's considerable talents that this book is a disappointment. For he has chosen to restrict his inquiry by focusing narrowly on the elite institutions of the Nguyen dynasty—the court, the bureaucracy, and the examination system—and their relationship to the Chinese model. While this approach to the study of Nguyen government has the merit of permitting the author to bring into play his extensive knowledge of both Chinese and Vietnamese political institutions and culture, it also has the effect of taking the government of the Nguyen dynasty out of the social context of that period. Dr. Woodside's analysis relates the adaptation by the Nguyen of Chinese structures and practices to the static features of the Vietnamese landscape—geography, population and its Southeast Asian, as distinct from Sino-Vietnamese cultural background. Perhaps the most constant theme running through the analysis is the impact of the differences in scale between China and Vietnam on Vietnamese borrowing in the administrative and educational spheres.

What is lacking in this analysis is some sense of the main issues which were raised within the society in this period and how the governmental institutions handled them. For it suggests, unintentionally, a degree of harmony and stability which simply did not exist in early nineteenth-century Vietnam. Woodside's failure to put historical flesh on the institutional bones which he describes is certainly not due to a lack of awareness of the broader social context in which the Nguyen dynasty appeared. As he



points out in his preface, the Nguyen dynasty began in 1802 after the defeat of the Tay Son Rebellion, a massive social upheaval and civil war which had, for more than twenty years, from 1771 to 1792 "summarized the socio-economic discontents of the Vietnamese peasantry in a more dramatic and large-scale fashion than ever before. . . ."

The Nguyen dynasty, then, was not re-emphasizing the borrowing of institutions from China merely because of the traditional cultural ties between the Vietnamese elite and China. The Nguyen period saw the restoration of conservatism after a long period of social instability and experimentation, marked in particular by a high level of peasant unrest. A very significant pattern of peasant protest, moreover, persisted throughout the first half of the nineteenth century. Dr. Woodside has pointed out elsewhere that as many as 105 peasant uprisings are recorded in the eighteen years of Emperor Gia Long's reign from 1802 to 1820. Except for a passing reference to Gia Long's fear of political instability in the villages, however, there is no indication in his analysis of the unresolved social tensions that led the Nguyen dynasty to reassert Confucian values and institutions. Indeed, the Vietnamese peasantry hardly receives more than perfunctory mention in the book.

By defining the main issue in Vietnamese society implicitly as a cultural conflict—Sino-Vietnamese vs. indigenous Southeast Asian—rather than as the concrete social and economic grievances of the peasantry, Dr. Woodside's study cuts itself off from the most important historical themes of the Nguyen period and the most fruitful lines of inquiry for the historian. While the author notes that the Nguyen "probably tried to approach the Chinese model more closely than the emperors of any other Vietnamese dynasty," he does not attempt to explain why this was so. Nor does he seek to explain why the Nguyen dynasty failed so miserably to prepare the country for the onslaught of the French, retreating instead further into the sterility of Confucian institutions just when the challenge of the European should have stimulated intellectual and social reform. Again, there is a suggestion at one point that the emperor's interest in Western technology and ideas was offset by the need to consolidate the bureaucracy's control over potential unruly peasants. But the point is not explored.

This is a criticism, of course, of the book which the author did *not* write. It was not his intention to explore the broader issues of early nineteenth-century Vietnamese history, but

rather to write "comparative institutional history." Nevertheless, the narrow conceptual focus of that approach, whatever its value, does not seem worthy of Dr. Woodside's capabilities or of the pioneering work on this period.

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**The Taiping Revolutionary Movement.** By Jen Yu-wen. (New Haven, Conn.: Yale University Press, 1973. Pp. 616. \$19.50.)

The great Taiping Rebellion (1851–64) for at least the past forty years has been a subject of unusual interest to Chinese and Western scholars. The publication of Taiping documents and Imperialist memoirs in the 1930s and the availability of files of the British Parliamentary Papers and the North China Herald provided solid documentary underpinning. When the Chinese Communists came to power in 1949, they adopted the Taipings and assigned to them the role of pioneering peasant revolutionaries. The 100th anniversary of the Rebellion in 1951 was officially celebrated. In the 1960s the world of Western scholarship saw the publication of sophisticated syntheses in English. Three of these deserve mention: Franz Michael's *The Taiping Rebellion: History and Documents*, 3 vols. (Seattle: University of Washington Press, 1966–71); Su-yü Teng's *The Taiping Rebellion and the Western Powers* (London: Oxford University Press, 1971); and Vincent Shih's *The Taiping Ideology: Its Sources, Interpretations, and Influences* (Seattle: University of Washington Press, 1967). The appearance in 1973 of Jen Yu-wen's *The Taiping Revolutionary Movement* now caps the climax; it is hard to imagine the appearance for some time to come of another study of comparable value.

Mr. Jen has been a worker in the lore of the Rebellion for more than fifty years. In that time he has so mastered the detail and the totality of the movement that he can write a history of it from the rebel point of view. His publications in Chinese include four collections of source materials, three scholarly articles, and seven books, culminating in his three volume *T'ai-p'ing t'ien-kuo ch'üan-shih* (Complete history), 1960. Fortunately the late Professor Mary Wright of Yale encouraged Jen to condense his multi-volume Chinese studies and form this new one volume synthesis in English.

A number of Jen's new findings interest me. For one thing, while the Taipings were confirmed iconoclasts so far as Taoism and Buddhism were concerned, they did not persecute

Catholics. At the Heavenly Palace in Nanking there were female servants but *no* eunuchs. The polygamy indulged in by the highest *wangs* at Nanking, though often criticized as inconsistent with the monogamy decreed for the rank and file, is shown to be consonant with Chinese and also Old Testament ideas of the privileges due those of high rank. Mr. Jen shows that the Taipings did not neglect trade, exporting raw silk and tea, expanding international waterborne trade, and striving to maintain free trade. He has a detailed description of their evenhanded, equal treatment of women, of the experiment at segregation of the sexes, and of female warriors and leaders.

In his discussion of Taiping theology, Jen has particular praise for their modification of the Ten Commandments as a monument of rebel creativity. Taiping neglect of Christian love and the burden of Christ's message he ascribes to the pre-eminent influence of Liang A-fa's *Good Words to Admonish the Age*; Taiping treatment of the gospel was influenced by the social and cultural milieu, the needs of the revolutionary movement, and a residual reverence for the truths of Confucianism.

Jen gives full consideration to the strengths and abilities of well-known imperialist opponents such as Tseng Kuo-fan. At the same time, he supplies full notice of the exploits of many other imperialist commanders. Tseng's cruelty appals him. Chinese jealousy of Tseng at the Manchu court was always strong. Also, Taiping treatment of the Chinese people after the capture of their cities was always far better than that of their opponents.

Relations with foreigners were damaged not only by Taiping arrogance and religiosity, but also by abysmal ignorance and the lack of good interpreters. After 1860 doubt of whether the Taipings could win strengthened the foreigners' decision to intervene. In these last years of resistance Hung Jen-kan played a major role as a military leader in relieving pressure on Nanking as well as that of a modernizing reformer.

Historians have previously said that Hung Hsiu-ch'üan committed suicide a month before the fall of Nanking in July 1864. But Jen has persuasive evidence for his death of illness. He ranks Hung among the great patriots of Chinese history, refusing offers of partition with the Ch'ing dynasty and staunch in his faith in the Chinese and in the Taiping religion.

So far as his broad conclusions are concerned, Mr. Jen insists that he is dealing not just with a rebellion but with a genuine revolutionary movement that planned to replace the ruling dynasty, to install a new ideology, to

change the system of land tenure, and to alter the social structure. Nor was it a peasant movement nourished on class antagonism. Apparently Taiping leaders had contempt for the farmer's lot and did not glorify the peasant class, contrary to Marxian interpretations. The Revolution is properly viewed as a precursor with an "evolutionary relationship" to the nationalistic Chinese Revolution of 1911-12.

The reader of this work will notice a preponderance of detailed military narrative which reflects incessant fighting. The loss of life and substance through combat and pillage was by all accounts catastrophic. The Taipings never knew peace; so the opportunity to put their land reform or Hung Jen-kan's progressive, Western ideas into practice never came. But their warlike work did demonstrate to Chinese of Sun Yat-sen's generation that the Manchus could be outfought, that a way to their overthrow was possible. Their example was heeded.

There is one conspicuous flaw in an otherwise admirable achievement. The campaign maps on pages 566-574, while helpful, are not numerous enough to assist readers of the narrative, who will search in vain for many key locations.

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**The Imperial Republic.** By Raymond Aron. (New York: McGraw-Hill, 1974.)

Raymond Aron, as was de Tocqueville, is a rare Frenchman who admires the United States and is mesmerized by the problem of its destiny. Tocqueville was awed by America's *social* influence upon his world; Aron, now, by its *imperial* influence. Tocqueville deemed the prevalence of the former to be inevitable; Aron sees the prevalence of the latter as necessary. A national culture influences its environment to the extent of its dynamism, but imperial influence—entailing the exertion of political power to maintain some system of order—requires will and determination.

Aron now tackles the formidable task of evaluating the American record in the last quarter-century. He says: "Grounds for outrage abound in our times the reader will pardon my sparing use of a commodity of which the supply exceeds the demand" (p. xxi). The commodity Aron supplies instead is a formidable display of political history, economic analysis, strategic doctrine and practice—and of the views and interpretations of innumerable commentators and pundits over three decades. This makes for a dense, discursive, and elliptical book. The reader who most easily will absorb what he

writes will be one who already knows much of what Aron knows.

Allusions on nearly every page of this complex book help to bring one's mind into accord with assertions about prerevisionist historical reality. Here are some: Soviet-American rivalry began, he argues, almost as soon as World War II ended; but it was the Korean War, endorsed and fueled by Stalin, which marked the beginning of the American cold-war military phase. It was the Europeans who asked for NATO, and who asked for Eisenhower to lead it, rather than the United States which imposed the institution and the man on them. The commencement of the political Cold War in 1947 coincided with the partition of Europe, rather than with any serious Western challenge to that partition. The Cold War—some revisionists to the contrary—served to obscure the U.S.'s conversion from universalist dreams to the reality of spheres of influence. American diplomacy gradually resigned itself to, or determined on, resistance and hostility to the Soviet Union, rather than the mounting of a deliberate offensive against it. Stalin, who out of fear of Hitler had betrayed that fear by cunning reticence and caution, had no such fears of the Western leaders he confronted—despising "what seemed to him a mixture of hypocrisy, weakness and blindness" (p. 42).

Accepting the adage that who controls the past controls the future, Aron in the second half of his book locks horns with revisionists and especially economic determinists to influence the perception of that past. Far from contesting the question of America's economic ascendancy of the world economy, Aron praises it. The twenty-five year (1947-72) phase of post-war economic and monetary experience Aron perceives as an unmitigated triumph of American purpose. None of the economic nightmares which obsessed World War II policy planners materialized; they were deterred. There was no worldwide depression, no widespread reversion to strict national autarky, no mass unemployment, no collapse of capitalism; instead, he says, there was sustained economic growth, and a growth rate of international trade exceeding the internal growth of national economies.

Aron would explain this success as largely due to American liberal trade doctrine and practice, and the successful use of the dollar as new surrogate for gold and the pound sterling as the international unit of exchange. If an interdependent world market economy would exist and flourish, it would flourish under the umbrella of a dominant American economic system.

We are left—in Aron's line of argument—in a strange impasse on the question of American economic imperialism: how far can we "explain" America's international political conduct on economic grounds? As to the New Left charge that the United States is the great predator of Third World resources and the great exploiter of their economies, Aron notes the obvious fact that it is the advanced economies of Europe and Japan which consume far more of those resources than does a more self-reliant United States. Moreover, American investment capital displays a striking preference for developed societies rather than for poor countries. The neo-Leninist charge of the necessary "law of capitalism" to explain the linkage between U.S. military force and U.S. overseas investments gains little credibility when one notices that other advanced capitalist and mixed economies have not chosen to imitate America's imperial activities, and most have gone out of the business of imperialism entirely.

Bowdlerized New Left theories to Aron, are absurd: the United States did not spend \$28 billion per year on the Vietnam War in order to protect non-existent Indochinese tin mines; a foreign policy determined by economics in the Middle East would have required the abandonment of Israel. Is it American capitalists who today protest Nixon's economic détente with the Soviet Union and China? Such protest now arises, rather, from a peculiar common-law marriage of Senator Jackson's strategic *Realpolitik*, the idealism of a few intellectuals, and the objections of organized labor.

Yet an overwhelming "fact" is suggested by Aron's line of reasoning and marshalling of evidence: The American concern of a benign international environment *includes* a deep commitment to principles of an international market economy—and these fly in the face of the state-trading practices of the Communist world. The political economy of freedom does not segregate politics from economics.

Perhaps the core of Aron's analysis concerns his evaluation of the nature, meaning, and consequences of the American policy of containment. He approves of the policy and considers it to have been a success. That the policy came to be extended from its original zone of application—Europe and the Middle East—to both China and the Soviet Union in Asia, Aron would say was not a decision imposed by the United States, but was a consequence of Soviet and Chinese aggression. The Soviet-fueled North Korean attack in 1950 came at a point in time when the United States was on the verge of wholly abandoning Chiang Kai-shek

and recognizing the new Communist regime. Washington had deliberately redrawn its strategic perimeter to exclude Korea and the whole Asian mainland. The aggression, when it came, elevated Korea to global significance. In the eyes of America's European allies, it tested the credibility of America's guarantees. Also, since no Japanese government would have tolerated a Soviet-dominated Korea, America as Japan's protector did not choose to do so either. That the aggression was widely perceived in Washington as a concerted Sino-Soviet test of America's imperial will-to-resist, may some day prove to be untrue; but policy makers hardly can wait patiently for historians to supply the full story.

When Aron judges the whole record of containment, the persistent reader comes across some perplexing contradictions which arise not so much from the author as from the very ambiguous world of *détente*. "The United States," he writes, "has achieved its proximal goal of containing the expansion of Communist control almost everywhere" (p. 149). But then, he says, the United States has not been able "to prevent the Soviet Union from hoisting itself to an equal level of military power. . . . There was only one global power in 1947; today there are two" (p. 149).

If there are indications now that the Soviet Union uses *détente* as an occasion to press forward its production and deployment of ever-greater strategic and conventional forces on land, sea, and air space, the obvious question arises whether they intend to transform strategic parity into clear strategic superiority. But in the West generally it is assumed that the essential nature of *détente* reduces the need for military strength. The most obvious question which a realist might ask is: In what ways can one now safely choose to distinguish between the "fact" of successful containment (defined as the checking of "Communist control") and the fact of the continuing sheer outward thrust of the Soviet Union's strategic presence and capabilities? Maybe a "new" U.S.S.R. of Brezhnev's successors may convince itself of the futility of the quest for strategic superiority, as it already appears to have abandoned its quest for the global triumph of its home-grown Socialism. Aron suggests no easy answer to this very difficult problem. What he does suggest obliquely, is that, while the objective elements in the superpower relationship have not changed very much since the early 1960s, "the question is whether the will power of one or the two states capable of playing a world-wide imperial role is slackening" (p. 149). He is enough of a skeptic to acknowledge that, just

as the much-touted American world supremacy in the Kennedy time was "apparent," so the American reputational collapse occasioned by Vietnam was merely "spectacular." He adds (p. 72) that "however sensational the sudden changes of fortune may have seemed at the time, they may perhaps merit no more than a few paragraphs in the accounts given by historians in the future—unless, of course, they prove to be indicative of the destiny of the American Republic and its incapacity to master the tendency to veer between crusading and withdrawal."

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**Die Berlin-Frage, 1949–1955.** By Dennis L. Bark. (Berlin and New York: Walter de Gruyter, Veröffentlichungen der Historischen Kommission zu Berlin, Band 36, 1972. Pp. xiv, 544. DM 96.)

**Berlin in der Weltpolitik, 1945–1970.** By Hans Herzfeld. (Berlin and New York: Walter de Gruyter, Veröffentlichungen der Historischen Kommission zu Berlin, Band 38, 1973. Pp. xxiv, 666. DM 118.)

Through its archival and publishing programs, the Historical Commission of Berlin has made an unusually noteworthy contribution to scholarship on Berlin. It has sponsored careful scholarly studies on numerous historical topics as well as an historical atlas that expands from year to year; and its multivolume chronicle of the first postwar decade (to be continued, one hopes), two volumes of sources and documents, and a 1012-page bibliography provide a wealth of information on modern Berlin.

The two volumes reviewed here are the Historical Commission's first to deal exclusively with the international political situation of postwar Berlin. Hans Herzfeld, dean of Berlin historians, has analyzed its development in his long-awaited study of *Berlin in World Politics, 1945–1970*. His treatment, primarily chronological, emphasizes the formative period up to 1949. Thorough and thoughtful chapters deal with wartime planning and indecision, the confusion and clashes that ensued when the wartime allies actually began their occupation of defeated Germany and realized how much their aims differed, the breakdown of interallied unity, and the Soviet blockade of Berlin followed by the West's airlift. By the end of 1949 separate governments had been created in East and West Germany, and West Berlin's anomalous position between the two became clear—even if not officially recognized until more than

two decades later in the quadripartite and inter-German agreements of 1971–1972.

The latter half of Herzfeld's study analyzes the pushing and shoving between East and West that characterized these two decades. Crises punctuated the era: the uprising in East Berlin and the GDR in June 1953, the "ultimatum crisis" that began in November 1958, the construction of the Berlin wall in August 1961. But more important for West Berlin was the fact that the new status quo became ever firmer. The prospects for a reunited Germany, with Berlin as its capital, diminished as each day went by.

Of particular interest is Herzfeld's attention to the interaction between policy and perceptions in the West. In the continuing Berlin crises—which are not over yet—how writers have defined the situation has made an important difference, for better or worse, to actual Western policies. Although some stoked the fires of the cold war, the more dominant trend was toward coming to terms with the post-1949 status quo, in effect preparing public and policy makers alike for its ultimate acceptance.

Dennis L. Bark's *The Berlin Question, 1949–1955* is a more detailed analysis of the critical years when West Berliners and others began to realize that ending the blockade had not been as unqualified a Western success as they had at first proclaimed. The first inkling came at the Paris foreign ministers conference just days after the transit routes were reopened. In the ensuing months the major powers no less than the FRG and GDR hardened their positions, trading propaganda and charges of perfidy rather than making the mutual concessions that would be required to move toward the announced goal of reunification. The June 1953 uprising merely confirmed Western fears that the Soviet Union had no intention of relinquishing its control in Germany. Meanwhile, the FRG was negotiating on both the economic and political fronts to regain its place in the sun—albeit a West European sun only. By 1955 the FRG was tied firmly to NATO, the GDR to the Warsaw Pact, and the division of Germany and Berlin was sealed.

The isolation enjoyed by West Berlin in 1949 did not dissolve during these years, but simply assumed different proportions. The city's leaders not only had to ward off subtle and not-so-subtle efforts by the East to dominate aspects of life in West Berlin, such as transit travel to the FRG or the transportation network in West Berlin itself. They also faced a serious danger from the West—to be forgotten. Some West Germans, their attention riveted on their own political and economic reconstruction, sim-

ply put West Berlin and its problems out of their minds. Others were reluctant to invest too heavily in the city's uncertain future. A few, harboring longstanding hostility to Berlin and what it had stood for, consciously sought to write it off. Concurrently, new patterns of communication, traffic, provision of such services as water and sewage, and attitudes were entrenching the political split between east and west within Berlin itself.

The task of West Berlin's leaders, then, grew ever more complex. They had to govern a city of two and a quarter million citizens, create and maintain jobs for its working force (a third of whom had been unemployed in the early 1950s), retard infrastructural developments that cemented the city's east-west spit, and, above all, strengthen the West's commitment to both the defense of West Berlin and its viability within the framework of the West German political system. The task on both the international and local level had, in the words of Henry A. Kissinger, turned into "a struggle to capture the symbols which move humanity" (Bark, p. 523).

However valuable these two volumes are for students of world politics and Berlin alike, make no mistake about one thing: Both are conservative in style of scholarship (e.g., atheoretical, with virtually no "hard" data) and, more importantly, in tone. If we ask what motivated Soviet leaders and such men as Walter Ulbricht to undertake the extraordinary and even dangerous policies they did toward the West, Germany, and Berlin, it may be comfortable to accept a traditional cold-war response such as their hunger for power and territorial expansion in the guise of ideology. The analytic task then is merely to document Eastern perfidy and Western steadfastness or indecisiveness.

But if we respond that ideology was the primary motivating factor—that Stalin, Ulbricht, and their associates were willing to take risks to realize their vision of a better world—then we must ask a second and equally critical question: Given this desire, and given the West's power and obstinance, then how *should* the East have proceeded to accomplish its goal? Both Herzfeld and Bark, and indeed most writers to date on the Berlin problem, are so convinced that the first answer is more nearly the truth that they neglect to explore seriously the far-reaching implications of the second. Those interested in investigating both approaches will nonetheless find ample food for thought in these volumes.

It is regrettable that the publisher's pricing policy—at spring 1974 exchange rates, the vol-

umes cost \$48 and \$39, respectively—has put these important studies virtually out of the financial reach of individual scholars.

RICHARD L. MERRITT

*University of Illinois, Urbana-Champaign*

**The League of Nations and the Great Powers: The Greek-Bulgarian Incident, 1925.** By James Barros. (Oxford: Clarendon Press, 1970. Pp. xiii, 143. \$6.50.)

The League of Nations' handling of the Greco-Bulgarian incident, which occurred three days after the signing of the Locarno Pacts, is often cited as one of the most important successes of that organization. Following the cessation of hostilities, the affair was finally brought to a close when Greece reluctantly accepted the findings of the International Commission of Inquiry and made reparations to Bulgaria on February 15 and March 1, 1926.

In the opening chapter Professor Barros interestingly suggests that what started as a simple frontier incident—the death of a Greek border sentry—escalated into a serious conflict because of faulty intelligence on the Greek side, and distorted messages on the scale of the Bulgarian attack sent by local Greek commanders to Athens. The subsequent three chapters deal with the difficulties the Great Powers (Britain, France, Italy) had in assessing the changing military situation; the increasing coordination of their policies aimed at moderating the actions of the Greek government and urging Bulgaria not to take the offensive; and, against the background of the deteriorating military situation, the transference of the question to the League Council.

The Council, convened on October 26 under the acting presidency of Aristide Briand, reiterated the request for orders to be given for the withdrawal of the respective forces and the completion of this operation within sixty hours. In addition, mediation initiatives were undertaken by Turkey, Yugoslavia, and Rumania. Briand seems to have regarded the latter efforts, however, as an attempt "to mask the League's action" (p. 79). Later discussions in the Council centered on the possibility of League sanctions—the withdrawal of chiefs of diplomatic missions and a naval demonstration at Piraeus—against Greece. But the growing diplomatic pressure on Greece proved sufficient to bring about a cessation of hostilities on October 28. The remaining part of chapter five is concerned with the work of the Commission of Inquiry headed by Sir Horace Rumbold and the problem of indemnities. The League Secretariat's discussion of the legality of sanctions is contained in the appendices, which are fol-

lowed by a bibliographical note and short index.

This book is undoubtedly impressive in its detail and draws heavily on *inter alia* British, Greek, Italian and League archival material. Not all the available archival material has been used, and secondary sources, including press citations from the main parties, have been omitted from the text. Methodologically the concentration on governmental and diplomatic exchanges is made at the expense of any significant treatment of the internal influences on Greek and Bulgarian policies and the wider significance of the Balkans in the international politics of this period. An introductory chapter dealing with, for example, the relationship of the *Komitadjis* to the Bulgarian government, Greek political and economic difficulties under the Pangalos regime, and the interests of the Powers in the Balkan region would have been helpful. We learn little also of the practical effects of the Treaty of Neuilly on Bulgarian military capabilities. Considering too the primary focus of the book, the role of the League Secretary-General and the reasons that the Pangalos regime accepted the League Council resolution warrant fuller and more systematic treatment.

Nevertheless, this book is an excellent reconstruction of the positions and policies of the Great Powers. In particular the attitudes and initiatives of British diplomatic personnel are given detailed and valuable elucidation. In short, Professor Barros has provided us with a specialist book on the policies of the main European Powers over the Greco-Bulgarian incident rather than on the intricacies of relationships among minor Balkan powers.

R. P. BARSTON

*University of Lancaster, England*

**The Cambodian Incursion—Legal Issues.** Edited by Donald T. Fox. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1971. Pp. xiii, 89. \$7.50.)

This small book contains the May 1970 proceedings of the Fifteenth Hammarskjöld Forum of the Special Committee on the Lawyer's Role in the Search for Peace of the Association of the Bar of the City of New York. The book's editor (Chairman of the Special Committee) wisely includes also a pertinent Documentary Supplement (p. 55) and well-balanced Selected Bibliography (p. 79).

Leaving aside the editor's foreword, which clarifies that the Forum took place before the ill-fated Cooper-Church and Hatfield-McGovern amendments, the Forum's proceedings determined the book's ordering. A contextually

oriented introduction by NYU law professor Andreas F. Lowenfeld begins the book, making the point—too wistfully—that “international law, which many thought dead at least as regards the security of the great powers, suddenly seems to have revived” (p. 1). Then follows, first, a constitutional debate between Assistant Attorney General (now Supreme Court Justice) William H. Rehnquist and NYU Law Dean Robert B. McKay, and then an international law debate between former State Department Legal Advisor John R. Stevenson and Harvard law professor (erstwhile Legal Advisor) Abram Chayes (with Rehnquist-Stevenson and McKay-Chayes speaking, respectively, for and against the Administration). Rebuttals followed by floor discussion, including statements by Columbia international law professors Friedmann, Gardner, and Henkin, conclude the volume.

Regarding the constitutional law debate (pp. 4–23), both Messrs. Rehnquist and McKay agree that the United States may engage in hostilities without a declaration of war, and, further, that the designation of the President as Commander-in-Chief grants a power of “substantive authority” (the single power on which the Administration based its decision to cross the Cambodian frontier). Their disagreements, to quote Dean McKay, “relate only to the definition of the limits of Presidential power” (p. 16); and in terms of both precedent and policy, Dean McKay emerges the more persuasive. Except possibly in relation to the President’s power to determine how ongoing hostilities shall be conducted, none of Mr. Rehnquist’s cited precedents appear sufficiently apposite. And as for the conduct-of-hostilities issue, Dean McKay’s basic policy argument—that “major” foreign policy decisions never should be taken without congressional approval, particularly in the absence of substantial popular consensus—is compelling. Unless one considers the much-criticized Gulf of Tonkin Resolution conclusive, no such approval (let alone consultation) was secured in this case. On the other hand, given the obvious interpretative problems involved, wisdom dictates heeding Professor Henkin’s counsel that “we cannot afford to deal with Presidential error by curtailing Presidential power. We have to elect better Presidents” (pp. 54–55).

Regarding the international law debate (pp. 24–41), judgments ultimately must hinge on whether one accepts Washington’s legal position vis-à-vis the conflict in South Vietnam itself. Explicit consideration of this broad and factually complex issue, however, was deliberately eschewed in favor of the narrower question of whether the Cambodian operation met

the requirements of prior “armed attack,” recipient-government “invitation” or “consent,” and plurilateral “consultation,” as variously distilled from the Final Declaration of the 1954 Geneva Conference, the SEATO Pact, the UN Charter, and other sources. Predictably, Mr. Stevenson answers in the affirmative and Mr. Chayes in the negative—the former stressing North Vietnam’s long history of “continuing armed attack” from the Cambodian sanctuary (pp. 26–27), the latter turning this same history to advantage his thesis that American policy was born not out of “instant and overwhelming” threat, but out of “convenience” (pp. 39–40). Despite the legal and political acumen of Messrs. Stevenson and Chayes, however, their debate parses too heavily into nice lawyers’ arguments. One has to read too much between their parries and thrusts of “necessary self-defense” and “proportionality” to see that they really are debating about how wisely to husband a legal order which seeks, albeit timorously, to advance the common inclusive interest of limiting justifications for armed force. And on this plane, it is difficult to fault Mr. Chayes. “Suddenly,” he sardonically notes, alluding to Prince Sihanouk’s downfall and citing former Defense Secretary Laird, “there opened up an opportunity in the change of government in Cambodia to do something that *we* thought would help *us*” (pp. 44–45, emphasis added).

Such, then, is the essence of this small book, appropriately dedicated to the late Quincy Wright. Although students interested in greater detail should consult a later, similar symposium partially reproduced in the January 1971 issue of the *American Journal of International Law* (Vol. 65, No. 1), it is a welcome addition to the literature. A tantalizing question remains, however: why does the title carry the word “incursion” (the Rehnquist-Stevenson term) rather than the word “invasion” (the McKay-Chayes term)?

BURNS H. WESTON

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**Bonn’s Eastern Policy, 1964–1971.** By Laszlo Gorgey. International Relations Series Number Three. Published on behalf of the Institute for International Studies, University of South Carolina. (Hamden, Conn.: Archon Books, 1972. Pp. 191. \$8.50.)

Professor Gorgey’s first book is a conventional historian’s account focusing on the evolution of West Germany’s Eastern policy from 1964–1971, therefore not including the subsequent developments between Bonn and her

Eastern European neighbors, particularly East Germany and Czechoslovakia, in the last four years.

Mr. Gorgey's analysis proceeds from a traditional and sound point of view about the geopolitical strengths and limitations of Germany's foreign policy: "Germany occupies a strategic central position in Europe. This fact of geography, both a curse and a blessing, has inevitably had a major impact on the political thinking of the German nation" (p. 3). Traditionally, this middle position has led to a policy that tried to preserve a balance between relations with the East and those with the West, either by abstaining from commitments or through alliances with one side. In the interest of "national security," Germany has also embarked on conquests, both East and West, from her pivotal central position.

In the aftermath of World War II, West Germany, under Chancellor Adenauer's leadership, sought to reestablish itself as a power by exclusive alliances with the Western allies. "The cost of an exclusively Western-oriented policy was *immobilisme* toward the Soviet bloc, more particularly toward the Eastern European countries . . . the inflexibility of Germany's Eastern policy in this period is attributable not only to Chancellor Adenauer, but to the Soviet Union as well (p. 5). Whatever the causes, Adenauer's policy failed to move toward German reunification or even to the more limited goal of a better accommodation with Bonn's Eastern neighbors.

The opening chapters deal particularly effectively with the difficulties of overcoming the Adenauer legacy. Professor Gorgey tells us that succeeding governments found it necessary to make more fluid a foreign policy that had become frozen under slogans such as "the policy of strength" and misguided dogma such as the Hallstein Doctrine. Changes in these policies had, moreover, to be worked cautiously so as not to alarm Bonn's Western allies, who themselves, particularly during the brief Kennedy Administration, appeared to seek a *rap-prochement* with Moscow that downgraded West Germany's security and ostensible reunification priorities.

Professor Gorgey points out that the Hallstein Doctrine, under which the Federal Republic threatened to break off diplomatic relations with any nation that recognized the Pankow regime, was a particularly rigid and even hidebound initiative. While threatening to alienate Third World countries and, more than subtly, threatening the principal Western allies as well, the Doctrine proved to be a holding operation at best for these purposes, while

strangling West Germany's conceivable opening to the East.

The author points out that under Erhard, Kiesinger, and then successfully under Brandt, German governments proceeded to circumvent the Hallstein Doctrine and finally to repudiate it *de facto*. The principal thrusts included a rapid expansion of trade with East European neighbors, a deliberately conceived economic policy, with political goals in view, and, at the same time, an assertion of "moral responsibility" according to which West Germany had to accept responsibility for the territorial consequences of the War and had to place the welfare of the East German people above doctrinaire legalisms.

In the process of ending her political isolation from Eastern neighbors, West Germany sought to establish what Mr. Gorgey calls "a new consensus" among its domestic political constituencies and tendencies. The several chapters devoted to the redirection of the principal political parties in this process are unnecessarily superficial; in particular, the evolution of S.P.D. leadership is treated too lightly, and the discussion of the German rightwing is too brief to be useful. Indeed, the interrelationship of domestic with foreign policies receives unsophisticated treatment in this volume.

In the remaining chapters of this book, Mr. Gorgey traces the halting evolution of a new Eastern policy in which he emphasizes the limitations placed upon these initiatives. His fundamental convictions from which the analysis proceeds are stated in his conclusions:

At the same time Bonn's Eastern policy must be carried out within the context of the West's policy of *détente*, for which the outlook is grim. The Western powers seem to ignore the fact that differences between their systems and the communist states are not a matter of nuance. They are fundamental. Not only the tactics but the overall strategy of Soviet foreign policy have changed in the last fifty-three years. Today the Kremlin seeks to achieve its goals by indirect means, by maneuvering its opponents into a series of partial political capitulations (p. 172).

From this series of assertions, it is easy to understand why Mr. Gorgey is convinced that the division of Germany into two states will continue indefinitely, and therefore also why Bonn's new Eastern policy "is very likely to suffer serious setbacks in the future" (p. 171); this also accounts for his description of the cumulative impact of the new Eastern policy as being of secondary importance, if not inconsequential.

I respect Mr. Gorgey's account of events but wonder about the accuracy and conse-



quences of his analysis. There is, after all, little question that, especially under Chancellor Brandt, the Federal Republic achieved a position of genuine strength as a Western partner and also as a pivotal power between East and West. Granted that its maneuverability is susceptible to limitations that can be imposed by the Soviet Union or the United States, the emergence of China as a nuclear power gives new dimensions to a balance that Germany can maintain or exploit. A new Rapallo is not in sight; and Brandt has played his role with a greater sense of responsibility than Gustav Stresemann before him. But the opportunities for West Germany in the East are not exclusively of its own making, and successive governments in Bonn might play a more adventurous role if opportunities were to come their way.

It is therefore perilous to hold that this or forthcoming Soviet regimes are so incessantly driven by ideological messianism that they would reduce the chances of attaining immediate objectives out of dogmatic fervor, for fright of a Germany which, even if reunited, would not pose the threat an emergent China does today.

GERALD FREUND

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**International Organization: An Interdisciplinary Bibliography.** By Michael Haas. (Stanford, Calif.: Hoover Institution Press, 1971. Pp. 944. \$35.00.)

In this bibliography Michael Haas offers scholars in the international organization field an extensive (nearly 8000 entries) reference work compiling the literature on international institutions of the ancient and contemporary worlds. This work brings together in a useful way references covering several disciplines and languages and including guides to libraries, periodicals, bibliographies, textbooks, and documentary sources.

Perhaps the most outstanding aspect of this bibliography is its organization, which features a chronological, functional, and geographical breakdown, as well as a separate author and subject index. Another particularly useful aspect is the treatment of separate chapters of books as articles, permitting them to be organized under the specific subject with which they deal. In addition, the book is well laid out with large, well-spaced entries in large type which are numbered and indexed individually as well as by pages.

Of the few shortcomings, some are created by the nature of the enterprise rather than by error or oversight. It is testimony to the grow-

ing interest and activity in the field of international organization (and the cut-off date of December 1965) that much of the very interesting literature on regional organization and integration is not included. Although interdisciplinary and multilingual, the bibliography is heavily weighted toward history and political science studies in the English language, with only a token representation of the extensive literature in French, for example. Another curious omission from bibliographical references is the *Peace Research Abstracts* compiled by the Canadian Peace Research Institute which is an important source in the field of international organization.

These flaws, however, do not greatly reduce the utility of a reference work which should benefit those for whom it is intended.

W. ANDREW AXLINE

*University of Ottawa, Canada*

**Turkestan zwischen Russland und China.** By Baymirza Hayit. (Amsterdam: Philo Press, 1971. Pp. 414. Hf 120.-.)

At a time when Russian and Chinese statesmen quarrel over the boundaries of the Soviet Union and the People's Republic of China, one loses sight of the fact that their states include lands which ethnically and culturally are neither Russian nor Chinese. Part of the vast region of Central Asia, which was carved up by the Tsarist and Manchu governments and retained by the Communist regimes, was once known as Turkestan, after the Turkic peoples inhabiting it. China conquered Eastern Turkestan between 1755 and 1765; she referred to it simply as Hsi-ju (the Western Region) until 1884 when she made of it a Chinese province by the name of Sinkiang or Hsin-chiang (the New Frontier). Russia subjected Western Turkestan between 1717 and 1895. Western Turkestan existed briefly as an autonomous Soviet Socialist Republic, but was divided up in 1924 into the republics of Uzbekistan, Turkmenistan, Tadzhikistan, Kazakhstan, and Kirghizstan—all within the framework of the U.S.S.R. As a result, Soviet writers and in their wake American and English specialists using Russian sources, now commonly speak of "Central Asia and Kazakhstan." But Dr. Hayit, a believer in the unity of the Turkic peoples, retains the traditional name of Turkestan.

Dr. Hayit holds a German Ph.D., yet he was born and raised in what is now the Uzbek Soviet Socialist Republic. While serving in the Red Army during the Second World War, he was captured by the Germans and made a member of the Turkestan Legion (later renamed the National Army of Turkestan),

which was part of the Wehrmacht, and worked in the National-Turkestan Unity-Committee in Berlin.

Hayit begins his study with the breakup of the Turkestan empire following the death of Timur (Tamerlane) in the fifteenth century (chap. 1). He continues with a description of the independent states of Turkestan on the eve of Russian expansion (chap. 2). He traces the development of early Russo-Turkestan relations, with emphasis on the eighteenth century, when diplomatic missions were exchanged between Russia on one hand and Bukhara and Khiva on the other hand and when the Tsarist government successfully incited the various steppe peoples against each other (chap. 3). He examines in detail the forty-two-year war between Russia and Turkestan, which was capped in 1895 by the Pamir Agreement by which Great Britain recognized the new Russian frontier (chap. 4). Hayit then turns to the struggle between China and East Turkestan in the eighteenth and nineteenth centuries (chap. 5). He reverts to the administration of Turkestan by the Tsarist regime (chap. 6) and to the national struggle for independence in the region, notably the ill-fated uprising of 1916 (chap. 7). The author depicts the impact of the Russian revolution of 1917 on Turkestan (chap. 8), the establishment of various national republics in Turkestan between 1917 and 1924 (chap. 9), and the continued national struggle for independence during the Soviet period (chap. 10). Turning once more to Eastern Turkestan, Hayit delves into the consequences of the Chinese revolution of 1911 and the proclamation in 1933 of the Turk-Islamic Republic of East Turkestan, which was set upon by Chinese and Russian forces (chap. 11). The book concludes with a discussion of the domination of the "pseudo-national states" of Uzbekistan, Kazakhstan, Kirghizstan, Tadzhikistan, and Turkmenistan by Moscow (chap. 12).

As will be seen from the above outline, more attention is devoted to Russian than to Chinese relations with Turkestan. This has been dictated partly because of the author's inability to read Chinese. Nevertheless, his study is monumental. The result of years of research in libraries and institutes in Turkey, Pakistan, Germany, England, and the United States, it is based on hundreds of books and articles in Uzbek, Tatar, Turkmen, Turkish, Russian, French, German, and English.

Dr. Hayit's scholarly and heavily documented history, published under the auspices of the Institute for the Study of Turkish Culture at Ankara, is girded with nationalism. He

begins with a quotation from Mir 'Ali Shir Navayi, a fifteenth-century Uzbek poet: "Though a people may consist of hundreds or thousands of tribes, mine will always remain one Turkish nation" (p. iv). Linking the past with the present, Dr. Hayit observes in his final paragraph:

The world at large knows already that Russia and China, each for itself, strive to extend their spheres of influence to all of Turkestan; the Russians want to drive the Chinese out of Eastern Turkestan, the Chinese the Russians out of Western Turkestan. In the question of the sole domination of Turkestan, Communism released in these powers their original imperialistic instinct. Turkestan stands between these rival powers alone and weak in every respect, without giving up its national will for survival (p. 353).

To Russian and Chinese scholars Hayit's activities and background will appear hostile. Yet his outlook is no more biased than their own. For too long, Western historians writing about Central and Inner Asia have perpetuated Russian and Chinese versions of history because they were able to read only Russian or Chinese.<sup>7</sup> Hayit's book provides a much-needed corrective.

GEORGE ALEXANDER LENSEN

*Florida State University*

**Frontier Defense and the Open Door: Manchuria in Chinese-American Relations, 1895-1911.** By Michael H. Hunt. (New Haven, Conn.: Yale University Press, 1973. Pp. xiv, 281. \$12.50.)

Michael Hunt has written an extraordinary first book: a model study of relations between China and the United States that renders obsolete two generations of uni-archival work. Professor Hunt's subject is conceived imaginatively as an investigation of why these two countries failed to cooperate between 1895 and 1911 despite a mutual interest in containing Russian and Japanese expansion in Manchuria. He focuses his analysis on Chinese efforts to regain control over their northeastern provinces and American efforts to preserve the Open Door—to preserve opportunities for the expansion of American economic interests in Manchuria.

With impressive use of Chinese sources, primary and secondary, Hunt constructs dialogues and evidences of bureaucratic competition and compromises among the Chinese such as are common to only the best analyses of the American decision-making process. Chinese figures are portrayed, warts and all, as clearly as their American counterparts. Familiar American figures—Hay, Rockhill, Root, Theodore Roose-

velt, Knox, and Taft—appear in familiar poses, but at long last, there is a sensible devaluing of Willard Straight's currency. In a brilliant appendix Hunt explains how historians can inflate the importance of minor figures, like Straight, who leave large and literate manuscript collections. Corroborating evidence for Hunt's interpretation of Straight can be found in the papers of Roger S. Greene and Thomas W. Lamont, two collections opened recently at Harvard.

Longstanding estimates of the bungling and ignorance of American officials are further substantiated by evidence in Chinese sources—evidence of an incredible ignorance of Chinese politics and society that made sound policy all but impossible. Unable to speak or read Chinese, representatives of the United States in China reported on the country with the accuracy of the blind men describing the elephant in the famous Indian fable. Hunt also scores the arrogance of Americans, especially Rockhill, in their paternalistic approaches to helping China. Few Americans seemed able to believe that the Chinese were capable of planning for their own defense; that the Chinese might know best what had to be done to save Manchuria for China and for American economic opportunity. Rockhill was unquestionably sympathetic to Chinese aspirations, but had his own program and succeeded in obstructing Chinese efforts.

On the Chinese side, Hunt provides an elaborate analysis of Yüan Shih-k'ai's policies in 1905, indicating more clearly than ever before why he had such a good reputation in American circles—why the United States was so receptive to his leadership in 1912 and 1913. Central to the plans of Yüan and his coterie was the establishment of a development bank in Manchuria to facilitate railroad construction. Overestimating the financial power of the United States, the Chinese hoped to draw Americans into this project. When private capital was slow to appear, the Chinese hoped in vain that Boxer indemnity funds remitted by the United States might be used. T'ang Shao-i's trip to Washington on the eve of the Root-Takahira agreement was part of the effort to gain American support for Chinese plans in Manchuria. Hunt's criticism of the American government for rejecting T'ang's overtures seems too harsh, however, and might benefit from a broader perspective—a sense of the greater importance of improving Japanese-American relations at that point in time. Too often Hunt's analysis of Roosevelt's actions reflects Howard Beale's errors, especially in the discussion of Roosevelt and the anti-American

boycott of 1905. He may also exaggerate the importance of T'ang's failure in explaining the subsequent fall of Yüan.

Hunt presents an interesting discussion of Straight's efforts to create anti-Japanese sentiment in Washington and notes Roger Greene's initial success in undermining Straight's campaign. At the same time he takes Huntington Wilson's post-Pearl Harbor memoir too seriously as evidence of his anti-Japanese attitude during the years of the Taft Administration. Richard Eppinga's recent biography of Huntington Wilson argues persuasively that Wilson was driven by an intense nationalist fervor rather than by hostility to the Japanese. The continued rise of Greene while Wilson's "activists" were shaping East Asian policy might also suggest that Wilson was not engaged in an anti-Japanese vendetta. But this is a minor complaint and must not obscure the fact that the book is important—and very well written.

WARREN I. COHEN

*Michigan State University*

**Foundations of British Policy in the Arab World: The Cairo Conference of 1921.** By Aaron S. Klieman. (Baltimore, Md.: The Johns Hopkins Press, 1970. Pp. 322. \$10.00.)

The allocation of Middle Eastern mandates by the Paris Peace Conference ostensibly provided a balanced compromise between French and British territorial aspirations in the former Ottoman Empire, on the one hand, and wartime promises of national recognition to Arabs and Jews on the other. The compromise proved unsatisfactory. During the summer of 1920 the Arabs mounted a series of uprisings against Western tutelage in Syria and Mesopotamia. For the French, the answer to this unrest lay in a tough and bloody suppression of Feisal's Syrian kingdom, and the establishment of a de facto protectorate for the Levant. It was a surgical approach that proved less feasible for the British. Bedevilled by a festering Irish rebellion, the relentless infiltration of Soviet agents through Turkey and Iran, and clamorous domestic pressures for demobilization and economic retrenchment, London sensed that the long-range solution to Middle Eastern unrest was to placate Arab sensibilities in a way that would insure peace, order, and above all economy in the Fertile Crescent. It was to devise a formula for this complex and involuted problem that, in March, 1921, Colonial Secretary Winston Churchill summoned Britain's military and civilian proconsuls in Egypt, Palestine, and Mesopotamia to an urgent conference in Cairo.

Dr. Klieman, an Israeli scholar currently teaching at Tel Aviv University, has assidu-

ously plumbed the archives of Britain's Public Record Office, as well as an extensive range of private papers and memoirs, to provide us with a solid and probably definitive account of that historic Cairo conclave. With admirable thoroughness, he has clarified the manner in which Churchill and his advisers appeased Arab nationalism by putting Feisal, a refugee from Damascus, on the throne in Baghdad, and Abdullah, a hitherto underrated Hashemite claimant, on the emiral throne of a newly created Transjordan. The formula worked, by and large. The extent to which it met Britain's salient concern for economy is made clear in the author's fine summary analysis of steadily reduced expenditures during the remainder of the 1920s. At the same time, Dr. Klieman's rigorous investigation of primary sources offers some choice revelations. Feisal, reputedly "elected" by a "representative" collection of Iraqi sheikhs and notables, in fact was jammed down the throats of the Iraqi nation by Sir Percy Cox and the threat of British fire-power; his principal competitor, Sayyid Talib, was forcibly deported afterward. Abdullah, miraculously appearing in the Transjordanian desert as a "last-minute" answer to the lawlessness of east Palestine, actually was regarded as a potential British client well before the opening of the Cairo Conference. The Zionists, presumably content with their national home west of the River Jordan, in truth had been continually encouraged by Balfour and Curzon to press their demands for the Transjordanian hinterland, and were altogether outraged by the loss of this fertile and spacious territory.

The book's weaknesses are almost as considerable as its virtues, however, and bespeak the volume's doctoral dissertation origins. The Cairo Conference was after all but a single brief episode in three years of British policy reformulation in the Middle East, and it resolved only the very narrow issues of Mesopotamian and Transjordanian unrest. Yet to this three-week assemblage are erroneously attributed far-reaching and portentous decisions that affected the future of Zionism in Palestine, Arab-Jewish and Anglo-French confrontations in the Levant, even British policy in the Suez and in the oil sheikhdoms of the Persian Gulf. The social and economic background of Arab nationalism is not so much as touched upon, nor does the breath of human interest emerge from a flat, one-dimensional sequence of diplomatic memoranda and *pour-parlers*. Even so, Dr. Klieman's study is a diligent and useful contribution to the emerging historiography of Britain's well-intentioned, but ultimately doomed, stew-

ardship of the Middle East's seething cauldron of nationalities and religious communities.

HOWARD M. SACHAR

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#### **American Policy and the Division of Germany:**

##### **The Clash with Russia Over Reparations.**

By Bruce Kuklick. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 286. \$9.50.)

In a closely reasoned and erudite diplomatic history, the author shows the primacy of economic strategies in U.S. plans for postwar Germany. He argues convincingly that the State Department's intransigent and uncompromising stand on reparations despite Yalta and before and after the Potsdam Conference was the primary cause for the partition of Germany and the Cold War confrontation that followed. Deploping the weakness of political theory without an historical dimension, Professor Kuklick uses a full range of government documents, memoirs of policy makers—Hull, Welles, Morgenthau, Harriman and Byrnes—and many less well-known sources to demonstrate how U.S. reparations policy reflected an economic and political world view, long espoused and promoted by the State Department. He defines this *Weltanschauung* as "multilateralism" and in a penetrating analysis shows how the State Department's position triumphed at Potsdam in 1945, causing the breakdown of U.S.-Russian cooperation over Germany and partition.

In an Appendix on Revisionist Historiography, the author eschews the conventional approach of orthodox historians—George Kennan, Hans Morgenthau, and Robert Osgood—who interpreted U.S. foreign policy in terms of the antithesis of "ideals" and "self-interest," conflicts between legalistic moralism and political realism (p. 238). Contrary to this historical interpretation, Kuklick maintains that we tend to see the world and see it whole, with ideology and interests as inseparable. The author argues convincingly that such ambitious goals as "multilateralism," international morality, and U.S. politico-economic supremacy were at best unrealistic and at worst based on a gross overestimation of U.S. strategic strength and a total misconception of the goals and power capabilities of other powers, Great Britain and Russia in particular. Thus over-optimistic American policy makers based strategy on a number of false assumptions: a belief in the success of postwar reconstruction under President Wilson, a belief in Russian weakness after both World Wars I and II, an overestimation of the British challenge to Amer-

ican power and a belief in the absolute righteousness of the American cause.

But when the American design was thwarted, Communist Russia easily replaced Nazi Germany as the embodiment of evil treachery. The USSR was not only held responsible for problems the U.S. faced all over the world, but it was also regarded as a totalitarian enemy with whom there was no common ground. A whole new generation of diplomats at the State Department accepted Secretary Hull's special interpretation of the history of the interwar years (1919–1939) characterized by: remnants of isolationism, high Republican tariffs and European tariffs, and quotas—all resulting in economic autarchy, decline of world trade, chronic unemployment, and worldwide depression. Economic misery brought the dictators to power in Europe, weakened democracy around the world and contributed to the growth of economic autarchy and spheres of influence in international politics—the very antithesis of Wilson's 14 Points. Hull was firmly convinced that “unhampered trade dovetailed with peace; high tariffs, trade barriers and unfair competition with war” (p. 12).

Within the framework of multilateralism, the author argues the following theses: First, the “Morgenthau plan” was neither unrealistic nor pro-Communist, but in accord with State's priority for the expansion of American commercial influence. Second, in implementing multilateral principles at Yalta and Potsdam, U.S. decision makers actually insured that a multilateral world would not be realized. The irony is that they overestimated their own strength and underestimated that of Russia. Finally, State's concept of a world political-economic system precluded an American compromise on the German issue, where the U.S. was largely in control and where great power agreement may have been possible. Professor Kuklick suggests that any four-power agreement on Germany or Berlin would be difficult at best, and therefore if the Allies were to agree on a *modus vivendi*, some degree of Soviet influence would have to extend throughout Germany.

The author concludes this sophisticated diplomatic history with a trenchant critique of the State Department's formulation and implementation of American foreign policy during and after World War II. He finds that in far too many instances that policy was not based on consensus or a logical rationale—but owed its shape rather to rivalries between State, Treasury, and War Departments or to personal animosities and individual idiosyncrasies or ad-

ministrative inefficiency. Even worse, decision-making often resulted from ignorance of facts or avoidable misunderstandings with other powers or without knowledge of past commitments. Yet at the same time, Kuklick correctly notes that our foreign policy has not been a mere history of chaos.

Professor Kuklick finds a key to understanding American foreign affairs from 1939 to 1946 in the multilateral concept of an international politico-economic system as the single most important thread through the tangled maze of great-power strategies. Elites in State, Treasury, and War Departments all believed that only an expansion of U.S. foreign trade could bring America domestic prosperity. Increased exports to the USSR would somehow compensate for U.S. losses entailed by the “deindustrialization” of Germany. Washington naively assumed that under America's concept of a multilateral, international politico-economic system, small nations would have security, parity, and equality with large nations, as suggested in the Atlantic Charter, signed by Roosevelt and Churchill in 1941, especially the clause providing for equal access of all nations and peoples to foreign markets and essential raw materials. Had not the United States given up intervention in Latin America? Secretary Hull maintained that Americans must give Russia “a concrete example of how we thought she should act” (p. 228). The author correctly notes that eventually Truman, Harriman and other elites at State, Treasury and War concurred in this approach along with its assumptions.

The author concludes that the Potsdam decisions for zonal autonomy and against Russian reparations from the Western zones, except for a mere trickle, meant that American desires for a multilateral globe were ruled out. This was the main turning point of the post-war era, which fixed the fate of Germany. Professor Kuklick's book is a highly intelligent and critical analysis, based on meticulous research—with more than 400 footnote references to standard sources and less well-known materials—combined with a clear, lucid style that will hold the reader's attention. The book is indexed with a bibliographical note appendix.

The author's major thesis is elaborated in a brilliant introduction, a compact conclusion and the appendix on revisionist historiography and is ably supported by nine sweeping chapters with sufficient detail for students of German history and politics as well as the average reader. A highly intelligent and balanced book,

it is less polemical and therefore more useful than other works on this complex subject.

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**L'Europe de Strasbourg: Une première expérience de parlementarisme international.** By Charles Melchior de Molènes. (Paris: Editions Roudil, 1971. Pp. 774. Fr. 75.00.)

On May 5-6, 1974, the Council of Europe celebrated its twenty-fifth anniversary with special observances in many of the 17 member countries and a solemn session of the Consultative Assembly in Strasbourg. These events passed almost unnoticed amid the excitement of a presidential election in France, a crisis in the Common Market caused by the Labor government's demands for renegotiation of Britain's ties, the Italian government's efforts to adopt emergency import restrictions, and the mounting vituperation between the United States and Europe. Its founders had intended that the Council of Europe play a vital political role in shaping the future of the continent, yet in just twenty-five years that body has become virtually moribund. Why did that happen, and what, if anything, can be done to revive its elan?

M. de Molènes, long an active militant in the European integration movement, provides a partial answer to those questions in this massive book. He surveys the doctrinal, institutional, and political foundations of the Council of Europe and describes the evolution of its relations with the European Parliament of the EEC and the Assembly of the Western European Union which together form the parliamentary arm of the European integration movement. Relying on a vast array of quotations from officials, scholars, and public figures, the author recounts the origins of the Council of Europe, stresses its devotion to the ideal of a "liberal and humanistic" community, and describes the Consultative Assembly and the Committee of Ministers. These accounts contain much that is of interest. For example, de Molènes cites writings by Pierre Dubois (a 13th-century legalist), Sully, William Penn, Kant, Rousseau, the Encyclopedists, Bentham, and a host of more contemporary figures to show that the European Movement has its roots in medieval history, Enlightenment philosophy and a humanistic, Catholic culture.

A review of the early sessions of the Consultative Assembly shows how it contributed to the formation of the Coal and Steel Community, the elaboration of a European Charter of Human Rights, and other milestones on the

path to greater integration. De Molènes emphasizes the structural problems that beset the Council as a result of the decision to combine parliamentary forms of representation with traditional diplomatic practices. The Consultative Assembly, composed of 140 parliamentarians elected by their respective national parliaments, exercises extensive deliberative powers. Its resolutions, however, are only advisory; final decisions are made by the Committee of Ministers which is made up of civil servants delegated by the various Foreign Ministries. Indirectly elected and with no control over the decisions of the Committee of Ministers, the Consultative Assembly has been unable to command the attention of either national governments or public opinion.

Moreover, with the formation of the Western European Union and the Common Market, questions of military security and economic welfare have become the special province of those organizations. Hence, although it is the only representative body that includes delegates from all the non-communist countries in Europe (except Spain and Greece), the Consultative Assembly has found even its range of deliberation reduced to what are essentially legal, administrative, cultural, and social problems.

These structural problems are further compounded by the conflicting attitudes of the member governments. De Molènes offers a critique of German, Swiss, Belgian, and French approaches to European integration, paying special attention to the views of the federalists, the Académie Française, and the Holy See, as well as to the relations between the Council of Europe and the countries of North and South America and various international organizations. These conflicting viewpoints and competing priorities further complicate the process of integration as embodied in the Strasbourg Assembly.

In his determination to cast a very wide net that will gather as many shades of opinion as possible into the European integration movement, de Molènes fails to set out clearly what must surely be the most important cause of the present disarray and the greatest barrier to future improvement: the lack of agreement among governments on long-run goals. For some, Europe is seen as a region characterized by a laissez-faire economy capable of continued growth, at the cost of continued economic and military dependence on the United States. A second group views Europe as a potential third superpower capable of rivalling the United States and the Soviet Union or of dividing the world in collusion with them, at

the cost of a massive increase in armaments and in the risk of military confrontations.

A third group envisions a vaguely "socialist" society aiming at increased welfare and greater economic equality at the cost of limiting growth in the economy, increasing the size of the bureaucracy, and accepting higher levels of political and military insecurity.

These alternatives emerge only dimly from the mass of quotations with which the author overwhelms his reader, but they are essential to the explanation of the current situation. Given the uncertainty about long-run goals, it is inevitable that the multitude of conflicting immediate interests will prevent any meaningful action within a parliamentary body that lacks effective leadership. To the extent that such divisions mirror the cleavages of domestic opinion within some of the larger states, the possibility of reaching meaningful agreements in the Committee of Ministers becomes still more remote. Without agreement among the three principal governments of Western Europe on a common vision of the future, there is little to be expected from a quasi-parliamentary regional body that tends to amplify the divisions within and among the member states.

By attempting to show the broad scope of the European movement, rather than to present clearly the differences that divide it, the author is led to seek remedies by tinkering with the institutions. This approach frustrates the reader rather than advancing the debate about what sort of Europe should be sought. Despite its confusing style of argument and the rather anodyne conclusions, this book contains much that will interest American students of European integration.

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**The Challenge of Hunger: A Program for More Effective Foreign Aid.** By I. W. Moomaw. (New York: Frederick A. Praeger, 1966. Pp. viii, 222. \$5.95.)

**The Challenge of World Poverty: A World Anti-Poverty Program in Outline.** By Gunnar Myrdal. (New York: Random House, 1970. Pp. xviii, 518. \$8.95.)

Although the two books were written nine and five years ago, respectively, the two challenges mentioned in their titles—hunger and world poverty—could scarcely be more current, for both darken the horizon of human history ever more ominously as the decade of the 'seventies unfolds. Both authors must be applauded for their farsighted concern with these overarching problems, the dreadful sig-

nificance of which is still ignored by most people, including a depressing proportion of the academic community and of major world leaders.

Unfortunately the second book so eclipses the first that little else can be said about the two together. Indeed the book by Dr. Moomaw is so anecdotal, so excruciatingly detailed on utterly trivial points, and so naive in its unquestioning faith in the potential efficacy of development assistance to actually produce development that it is difficult not to overlook the basically sound insights it occasionally offers. Persons with experience in the field of development may find the book of some interest for the comparisons it can generate, for Dr. Moomaw has obviously had long and intimate exposure to development assistance programs all over the world, but it is far too careless in its generalizations to be useful to the uninitiated.

Professor Myrdal's, on the other hand, is one of those rare books which everyone—student, teacher, citizen, politician, bureaucrat, military man—could profit from reading. Although its author, in Kenneth Boulding's words, "may very well be the world's top social scientist" and is an economist to boot, the book is innocent of equations, charts, graphs, and even tables; total reliance being placed on clear, well-organized prose, carefully defined concepts, and straightforward explication of all assumptions and value premises.

Intended to be both a summary and a continuation of his massive three-volume work *Asian Drama: An Inquiry into the Poverty of Nations*, to which it is exhaustively cross-referenced, *The Challenge of World Poverty* is primarily a book of policy conclusions and recommendations—as its subtitle suggests, *A World Anti-Poverty Program in Outline*.

Basically that program consists of a multi-pronged attack on the severe inequalities which have made marginal men and women of the vast majority of the inhabitants of the less developed countries. Thus, while making birth control available to all, restructuring the direction and content of education, rationalizing public administration, and generating expanded levels of aid from the industrialized countries are all essential elements of his development program, the *sine qua non* of success, according to Myrdal, is effective land reform through which the masses can be provided with the opportunity and the incentive to strive for improvement of their productivity, their land, and themselves. In short, Myrdal makes it unavoidably apparent that the most formidable barrier to development is social injustice.

Several themes recur like litanies, sometimes rather tiresomely, throughout this wise and compassionate book. One is the criticism of what Myrdal has dubbed the biased "postwar approach to economic planning, with its evasion of the problems of attitudes, institutions, and the productivity consequences of very low levels of living, and its diplomatic tendency to avoid awkward problems and remain overoptimistic" (p. 120). He is particularly harsh with his fellow economists on this score, chiding them for their uncritical transference of models developed in the industrialized countries to the crucially distinct milieux of the less developed regions and their refusal to speak out against the corruption and distorted priorities characteristic of the "soft states" in the Third World. In a similar vein he deprecates their willingness to make use of the woefully unreliable statistics currently available on LDCs. Political scientists, of course, are equally vulnerable to both these charges.

Other frequently repeated themes of particular interest to political scientists are those dealing with the passivity of the poverty-stricken masses, the relative lack of pressure from below for needed reforms, the monopoly of power held by the upper classes, and the need for political leadership willing to challenge that monopoly in the interests of the masses if development is to occur. These themes indicate the extent to which this book is a classic example of political economy, for time and again the economic factors in the development process are shown to be determined by the realities of power, and solutions to development problems are revealed as dependent on political will.

Contrapuntally two other important themes are interwoven throughout the book. One deals with the absolute necessity that the major responsibility for development programs be shouldered by the LDCs themselves—that they alone can alter their institutions and arrange their priorities in the ways necessary. Nevertheless, in case after case, the second theme documents the delays, failures, perennial postponements, and skewed policy outcomes which have thus far characterized almost all governmental responses to the challenges of development in LDCs, particularly perhaps in the politics attempting to emulate the liberal, pluralistic, democratic politics of Western industrialized countries. Myrdal reluctantly but honestly, albeit belatedly and almost in passing, recognizes that authoritarian regimes may prove more able to carry out the necessary reforms than would democratic regimes (p. 436). In

a trenchant critique he blames the West and particularly the United States for "gross misjudgment of facts and betrayal of ideals," resulting in conditions under which "the awakening of the masses and their becoming conscious of their interests and prepared to fight for radical reforms needed for development shall happen in a world political constellation where they find themselves projected into a movement of national Communism" (p. 435, his emphasis). This is the legacy of our having been so "readily prepared to condone the absence of reforms in underdeveloped countries, or the perversion of reforms, preferring stability—in fact, a sort of continuation of colonial practices" (p. 436).

In my view, only the sort of development efforts outlined by Myrdal hold forth any reasonable hope for improving the lot of the majority of mankind, for only such measures as he proposes can possibly create conditions under which presently wasted human productive potential can be actualized and family size motivations can be reduced, thus eventually defusing the population bomb—although not before it will have wreaked unparalleled havoc. Unfortunately, however, the prospects for the necessary redistribution of opportunities are practically nil, given the selfishness, shortsightedness, and disproportionate power of privileged elites within both the LDCs and the so-called developed countries.

The degree of enhanced understanding of the problems of development to be gained from a careful perusal of *The Challenge of World Poverty* can only be hinted at in a brief review. Suffice it to say that its straightforward clarity, rationality, honesty, and humanitarianism as well as its unstintingly critical approach to evidence and to accepted theories make reading it a refreshing, instructive, and even inspiring intellectual experience. For students of development there is, of course, no substitute for actual fieldwork, but this book provides a useful surrogate until such an opportunity presents itself.

I cannot recommend too highly that this book be read by all social scientists, both neophytes and more experienced specialists long caught up in the constraining refinements of their disciplinary subfields. If it could be read and its wisdom learned by every literate member of the world's increasingly interdependent yet dichotomized population, the outlook for the quality of life of future generations would be far brighter than in fact it is.

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**Retreat from Empire? The First Nixon Administration.** By Robert E. Osgood et al. (Baltimore and London: The Johns Hopkins University Press, 1973. Pp. vii, 350. \$14.50, cloth; \$3.95, paper.)

Robert Osgood and his colleagues at Johns Hopkins embarked in 1967 upon a series of quadrennial reviews of American foreign relations at the end of each presidential administration. Thus *Retreat from Empire?* is Volume II of *America and the World*; Volume I appeared in 1970.

A high standard of analysis is sustained throughout this symposium, from a set of perspectives not very distant from those of the first Nixon administration itself (except perhaps on Vietnam, which, however, is rather loftily transcended by the grander geopolitical foci of these authors' attention). Their preoccupation is with the implications of the Nixon Doctrine, or Nixon Strategy as Robert Osgood prefers to designate it in his masterly introductory essay: "military retrenchment without political disengagement" (p. 9). The question mark at the end of the book's title refers to a sense that as America's troops "retreat," the bonds of her "empire" are being modified—but how much? The answer that rightly pervades the book is "not very much, at least not yet, and not in the intention of the Nixon Administration." The emperor has been so over-dressed for so long that he can now get along without a good many of his clothes and still, a little less pretentiously, remain emperor. Most of the authors welcome the disrobing; all of them regard it as inevitable; yet none of them would rejoice to see it complete. The adroit circumspectness of this Nixon/Kissinger striptease receives wide approbation among them. Writing in 1972, none showed revulsion from the idea of a second Nixon administration; most of them rather optimistically saw opportunity in it for the (usually modest) revisions of choreography that each desired.

Reading the book in 1974 one is incidentally struck by the authors' inattention to the implications for foreign policy of the potentially vulnerable thinness and factitiousness of Nixon's general domestic support. The authors did deal with manifestations of these qualities in the possibly brittle support that particular Nixon foreign policies were receiving. But the personal ascendancy of Nixon/Kissinger in the American domestic arena overall was unanimously taken for granted by these writers. McGovernism was casually dismissed, Watergate unmentioned. Of course? It is indeed easy to say "of course"; but what we now know of

the "plumbers," their origins and aftermath, establishes a link from foreign to domestic policy in the first Nixon administration that shifts back to ensnare some foreign policy in the second Nixon administration in ways that one wishes at least one of these political scientists had considered worth speculating upon. By 1972, McGovern and other critics had already sounded an alarm at some of these tendencies, and the authors do devote loving care to dissecting some much less plausible possibilities overseas.

A broader objection one might raise to the book, also related to the domestic setting of American foreign relations, is that the authors avoid coming to grips with the radical school, notably the economic determinist critiques of American foreign policy; there is little evidence of attentiveness, much less of readiness to incorporate ideas. Perhaps more fundamental is the fact that only one of the authors, David Calleo, shows himself fully at home in both economic and political analysis of any school; Benjamin Rowland comes close but seems somewhat more comfortable with the economic than the political; none of the others evinces much more than a perfunctory concern with economic influences. Arguably this weighting toward power politics was appropriate for the analysis of world affairs in the generation after Munich; certainly in that era the conventions of American political science prescribed power political, not economic emphases. But the slighting of the economic dimensions has become increasingly challengeable, and *Retreat from Empire?* does not do as much as it might do to right the balance.

Yet there is much in the book of great value.

The essay by Calleo is, I think, one of the most powerful succinct statements an American has yet made of the thesis that the United States should retreat gradually from its empire in Western Europe and Japan (as all the authors see Nixon tending to do in other parts of the world). The two essays by Osgood, which are the other best papers in the book, gingerly approach a similar recommendation in a finely balanced argument, but Osgood never quite declares a committed judgment. Together the three papers provide an elegant dialectic. Part of the difference that remains between the authors on this major topic clearly rests on personal style and temperament: Osgood is more conservatively cautious, Calleo more enthusiastically venturesome. Another part of the difference might lie in Calleo's possibly keener regard for European civilization per se, and in his sense that America does not

deserve to dominate it indefinitely. But a more prominent difference is Calleo's emphasis on the way United States military deployments in Europe and Japan put the advanced capitalist countries under economic strains that he considers eventually unbearable; rather than waiting until the wonderful one-hoss shay of international economic patching breaks down suddenly, the United States ought to be devolving power and responsibility, military and even nuclear, now. Osgood skirts this economic line of argument. And even Calleo, I think, fails to substantiate a probability of economic disaster quite so high as to be obligatorily an over-riding consideration even in the minds of those who (like Nixon/Kissinger) regard other kinds of concerns as balanced heavily against American withdrawal.

Of the authors in this symposium, the most thoroughly committed against withdrawals is George Liska. Despite a mysteriously opaque and elliptical presentation, so bare of path markers that one apprehends the argument with the frustration and mounting excitement of one deciphering hieroglyphics, the effort is ultimately well rewarded. The reader finds the puzzle fits together in an ingenious brief for facilitating the survival of the fittest regimes in the Third World by Great Power condonement of interstate as well as intrastate conflicts among them; Liska urges that we foster the emergence of a hierarchy of states, in which strengthened Middle Powers will oversee their respective regions, buffering for the Great Powers. Liska finds this new "possibility at once melancholy and reassuring" (p. 343) as a foothold on the slippery slope of Nixon's retreat from Liska's 1967 vision of *Imperial America*. "Realism can merge with resignation" (p. 343) in advocacy of the new formula for meeting "the need to integrate a bothersome but indispensable third world into a 'new' American global policy before the void created by the 'liquidation' of the Vietnam war has evolved into a dangerous vacuum of purpose underneath the diplomatic conference tables of the great" (p. 340). In thus modifying his own brand of neo-imperialism Liska has of course not chosen to go far enough to mollify most of his past critics. (Could the arcane presentation be itself designed to trap them into at least exposing themselves to all his argument? Not likely; the wide audience the argument deserves would be better held by the less compressed and more lucid presentation it receives in his recent *States in Evolution*.) Meanwhile, even as it stands here, Liska's incisive iconoclasm again performs well its perennial function of stimulating the imagination of open-

minded specialists on international affairs.

Robert Tucker's essay on American opinion and foreign policy in the first Nixon administration also merits special commendation, for the breadth, insight, and judiciousness with which it traces Nixon/Kissinger success in the domestic interplay of ideas and power with foreign policy elites and arousable publics. His theme is the restabilization of a hegemonial permissive consensus, likely to last at least until and unless the traumatized youth of the 1960s reach middle-aged power.

Francis Rourke's essay supplements Tucker's like a competently fact-filled and useful appendix. Benjamin Rowland's performs a similar function for Calleo's, tracing political economy in U.S.-Latin American relations. Laurence Martin's analysis of military strategic concerns is thorough but rather ephemeral. Herbert Dinerstein's speculations on Russia and China seem less fully digested than the other essays in the book. Yet the contributions of these four authors, while less distinguished than those of Osgood, Calleo, Liska, and Tucker, uphold a sufficiently high standard to confirm the worth of the volume as a whole and advance the incipient quadrennial series of which it is a part.

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**Peace, War, and Numbers.** Edited by Bruce M. Russett. (Beverly Hills, Calif.: Sage Publications, 1972. Pp. 352. \$17.50.)

Commissioned in the spring of 1969, most of the eleven papers in this volume were read at the September 1970 meeting of the American Political Science Association in Los Angeles, and were then revised for publication and prefaced with an introduction. They were intended to be a "sampling" of "recent social science research on war," the paper writers being instructed to develop specific hypotheses about the causes or conduct of international violence, to test these with actual data about international behavior, and to indicate the relevance of the findings for theory and for policy.

A short review written several years later cannot do justice to the work of the score of authors represented in this volume, the more so since several of the papers are no more than progress reports of elaborate and extended research undertakings. But the review of such a collection can, it is hoped, throw some light on some common characteristics, the strengths and the weaknesses, of some recent research on war and peace.

The volume itself, rather than being a "sampling," is more nearly the product of one "in-

visible college" in this field (of the fourteen contributors, five hold, or have held, appointments at Yale; four have had training at Stanford, and four are connected with Michigan). Its strength undoubtedly lies in the care the authors take in presenting and analyzing their material. It is not just that the editor's instructions have been closely heeded but it is also plain that the authors are in the habit of being commendably careful and precise about delimiting a researchable hypothesis, crisply marshaling some well-organized data, and assessing the findings in a spirit of caution and circumspection. These are good pieces of work even if there is a certain sameness about them: three out of four papers principally rely upon some form of bivariate or multivariate analysis (as the result of which the volume is liberally sprinkled with correlation coefficients and the like, more than a thousand of them), and there is also much reliance on some common data sources even while (to my taste) not enough raw data are presented directly to the reader so that he might attempt his own interpretations; but the overall impression is clearly one of serious and persevering work informed by firm method.

Matters become less clear, however, when we move from methodology to results. On the crucial matter of war causation no new or convincing findings really emerge. Singer, Bremer, and Stuckey test two alternative theories, asking whether parity among Great Powers, or else preponderance of one Great Power over others (that is, a high degree of concentration), is more likely to conduce to war, and others take up similar themes. But as Hanson and Russett point out in an analytical introduction, "neither theory has very general predictive power . . . further research on appreciably more refined hypotheses will be required." In need of refinement might be the concept of "concentration," and analogies from industrial organization could be useful here; one also wonders if the introduction of a distinction between land and sea power might not reveal more pronounced tendencies toward preponderance than the complete aggregation of data. Wallace's analysis of the relationship of the onset of war to status inconsistency appears more promising, but here one wonders about the ease with which the concept has been indexed by the distribution of diplomatic missions. Attention is paid to the attributes of nations, but then who doubts that power, the degree of economic development, or the character of the political regime have an important bearing on foreign policy?

An interesting if unpremeditated general finding common to several of the papers is "the

great stability in patterns of conflict and cooperation over time" (p. 16). For instance, J. D. Sullivan finds that the most important indicator of common alignment was past alignment, and Jonathan Wilkenfeld notices a tendency on the part of most states to maintain existing levels of foreign conflict. Jeffrey Milstein identifies a persisting, action-reaction system of violence in the Middle East since 1948. All these could be evidence of a general "law of social inertia," but it could also be that massive organizations engaged in international operations find it particularly difficult to maintain the flexibility that is required for coping with the changing environment. "This emphasis on the structural properties of the national system, the global international system, and of conflict sub-systems should sober us," write Hanson and Russett (p. 17) but suggest no further implications of this thought for theory or policy.

But if structural questions are indeed so important, then we are entitled to ask why the volume as a whole does not more thoroughly examine them. This shortcoming is due in part to weaknesses in conceptualization and imaginative theory building. The knowledge enterprise, it must be remembered, does not consist solely or even predominantly, of hypothesis testing. A shining research design is no guarantee of insight, of explanatory, or predictive power; arguably, it might even be counterproductive. For it tends to inhibit, as well as guide, imagination, innovation, serendipity; it leaves no room for the leap into the unexpected. Thus powerful tools may be employed to search for correlations between tired concepts. Or else crucial concepts remain vague: for instance, that of "Great Power" has at least five operational definitions in the volume.

Puzzling, too, is the reluctance of empirical researchers to test their analytical constructs not against their own (rather excessively standardized and narrow) data but, on the principle that data may also be found outside data banks, also against external reality and in the plain light of common sense. The number of wars or alliances being, as it is, rather small—only a few score wars per one hundred years—statistics on such a limited universe are not all that is needed for full understanding. Yet nowhere in the book is there a discussion of a single war or conflict as an event in its own right, either for its own sake or even by way of illustration. To coin a phrase and mix metaphors, wars, when used only as machine-fodder, will maintain their dignity, and refuse to yield understanding. The authors also display a marked resistance to illustrating the concepts

they are using, even if these may be novel or unfamiliar. J. D. Sullivan, for instance, writes a whole paper on informal alignment (essentially an artifact of a data-collection system) without once giving a concrete example of such a structure. While replete with empirical data (extending, in four papers, into the nineteenth century and, for the others, for shorter or longer periods since 1945) the volume nevertheless seems remote from historical reality.

To me this suggests that some recent work on war and international behavior may have suffered from premature specification. Some of it may have brought powerful mechanical procedures to bear on concepts or data not yet refined enough to withstand such treatment. The global society, which is the only one we have, is not just a collection of homogeneous and substitutable elements or compartments; it is a complex system informed by an organic unity of pattern that has structure, persistence, and a life cycle all its own, and one that exhibits, if you will, interdependence. Particular care must be taken not to parcel out this unity into a multitude of ill-fitting even if easily manipulable boxes, for its properties might be lost in the process of cutting up. That is why the true service of this volume might indeed lie in unexpectedly pointing out the tendency toward stability and continuity in world political arrangements.

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### **The Vienna Convention on the Law of Treaties.**

By I. M. Sinclair, C.M.G. (Dobbs Ferry, N.Y.: Oceana Publications, 1973. Pp. 145. Index. \$6.00.)

At its first session in 1949 the United Nations International Law Commission placed the law of treaties high on its agenda as a subject of study for codification. This book by one of the legal advisers of the Foreign and Commonwealth Office of Great Britain quickly reviews the work of the Commission up to 1968 and then analyzes with consummate skill the Vienna Convention on the Law of Treaties.

Students of international law will find in this volume an objective, tightly structured exposition of the "treaty on treaties," which was open for signature on 23 May 1969 at the conclusion of the United Nations Conference on the Law of Treaties. The Convention covers, *inter alia*, how treaties are concluded and enter into force, how treaties are to be applied, interpreted, amended, or modified, and how treaties become invalid, or may be terminated or suspended in their operation. These matters had long been largely governed by customary

international law and early in the deliberations of the International Law Commission it was felt that an expository code would be sufficient, but later the Commission recommended and the Vienna conference adopted a multilateral treaty. The Convention not only codifies rules with extensive precedent and doctrine behind them, but also contains some "progressive development" of international law [Article 9 (2)].

The Vienna Convention on the Law of Treaties, although comprehensive in its scope, has many restrictions in its applicability. Only written agreements between States are covered by the Convention. Moreover, the rules of the Convention are not generally retroactive, but in large measure they are operative unless the treaty itself should provide other rules. Do treaties come into force upon signature by the parties if the articles are silent about the need for ratification or other confirmation? The Convention has not resolved this knotty issue. Finally, the customary international rules relating to treaties in cases of State succession and State responsibility, and the effect of the outbreak of hostilities upon treaties have not been treated by the Convention.

One of the most useful parts of this very competent technical study of the evolving law of treaties deals with those rules not clearly and universally acknowledged, but alleged to be emerging international norms now ready for the legal cachet of the Vienna Convention. In this tendentious area the Convention posits that the objection by one State to reservations made to a treaty by another State does not preclude entry into force between the objecting State and the reserving State unless a contrary intention has been stated by the objecting State; that a State must refrain from acts tending to frustrate the object of a proposed treaty when that State has agreed to enter into negotiations and is engaged in such negotiations; and that treaties are invalid for a violation of manifest and fundamental internal law.

Another important issue on the law of treaties has been the effect of "coercion" upon a party. States have hesitated to upset the legal fiction of consent that made treaties of peace viable. The drafters of the Vienna Convention were convinced that a norm of international law had been created over time that coercion could be grounds for the invalidity of a treaty, but it remains a slippery political concept for the law to define.

The chapter on *ius cogens* is of the greatest value. Obviously drawn from municipal law, if not natural law, and the notion of acts being null when contrary to public policy, *ius cogens* is a peremptory norm of international law from

which States are not permitted to derogate by treaty. Considerable apprehension was expressed by States about this sweeping advance into the uncharted waters of the law of nations. Only a definition of a peremptory norm "accepted and recognized by the international community of States as a whole" somewhat allayed fears.

The study closes with a good review of the protracted negotiations that finally led to a political compromise through which parties to a dispute on the issue of *ius cogens* may submit that dispute directly to the International Court of Justice if after twelve months no solution has been reached under the Article 33 of the United Nations Charter, unless the parties have agreed to arbitration. But for other disputes, the machinery established by the Convention was a great disappointment for those hoping for stronger third-party settlement devices.

In all, Mr. Sinclair's study is indispensable to a contemporary reading list on the law of treaties. Sadly the book itself does not contain the text of the 1969 Vienna Convention, which is dreadfully inconvenient in perusing such an otherwise splendid analysis by a legal craftsman who was present at the birth of this far-reaching treaty on treaties. If and when thirty-five states deposit instruments of ratification or accession with the Secretary-General of the United Nations, the Convention will be in force.

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**Appeal To Force: American Military Intervention in the Era of Containment.** By Herbert K. Tillema. (New York: Thomas A. Crowell Co. 1973. Pp. 260. \$3.95, paper.)

Professor Herbert K. Tillema has attempted to explicate the puzzles of American military intervention in a methodical and insightful study. The author defines overt military intervention as "the open and direct use of military force by one country against another" (p. 3). Overt military intervention comes in four varieties as discussed by Tillema and applied to selected case studies: (1) deployment of combat-ready units by the U.S. to another country; (2) naval bombardment of another country; (3) aerial bombing or firing missiles; (4) close combat support for another country's forces in battle. The author isolates four cases of American overt military intervention since World War II as crucibles for the development of theory about the cause of intervention: the Korean War, the Lebanon crisis, the Dominican intervention, and Vietnam.

The thesis argued by Tillema is that United States overt military intervention is a "rare event" and that the explanatory puzzle is the *absence* of intervention in numerous cases of possible entanglement, rather than the presence of overt military intervention in any case. A theory of restraints on intervention is proposed as the structure of insights required for understanding this set of processes. The casual reader is likely to underestimate the importance of this reformulation of the problem. Tillema asks us to concentrate on the doughnut rather than the hole, and challenges us to account for the *refusal* of decision makers to intervene with overt military force in response to crisis and threats. The emphasis on restraints against intervention reveals the atypicality of that event as a matter of historical fact, despite the expressed concerns of many writers that American forces are used too often too soon and in a manner that is operationally too detached from political objectives.

The author categorizes the restraints against overt military intervention as restraints inherent in the international system, in the elite decision-making process, and in the "shared moral values" of foreign policy makers. His theory is stated with deceptive simplicity as follows: "On those occasions when a Communist threat was thought to exist and when none of the other restraints was operative, intervention has followed. In the absence of a perceived Communist threat or when one or more of the other restraints have operated, overt military intervention has not occurred" (p. 179). In formal terms, the necessary condition for overt military intervention is the perception of Communist threat against a previously non-Communist country, and the sufficient conditions involve the absence of any perceived restraints.

The reader should consult the text itself for refinements of these generalizations, for the author almost tediously recapitulates and specifies the various dimensions within each category of restraint. The intuitive sensitivity of this analysis will be obvious to students of defense and military policy, and the book can be recommended strongly on those grounds. Additional tidbits are provided by the first appendix, which cross-classifies instances of American nonintervention with the author's checklist of restraints operating in each case.

I do have three methodological reservations: First, the logical entrapment of case-study analysis, in which the categories are derived from the cases and then validated by reference to the cases, is not completely transcended in the study. Second, the issue of the threshold

separating overt military intervention from covert involvement is resolved only arbitrarily by the author, who acknowledges the elusive character of this problem (pp. 188-189). This raises enormous difficulties when the character of the intervention can change drastically *after* the initial commitment; intervention theory and escalation theory become hopelessly intertwined for analytic purposes.

Third, the author makes numerous assertions which can only be verified by evidence about the perceptions of decision makers, and the relative paucity of data about shared perceptions within decision making groups is a severe handicap. For predictive purposes, the entire

theory of restraints reduces itself to a set of assumptions about the *perceptions* of restraints by national leaders. Tillema has derived a great deal from the existing literature on this subject, but his and earlier studies only reinforce the need for more intensive microanalytic investigations of foreign policy decision making. In terms of its own scope and objectives, however, this study succeeds in parsimonious conceptualization of previously underformulated subject matter, and it will be instructive for specialists as well as stylistically acceptable for general readers.

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VOL. LXIX

1975

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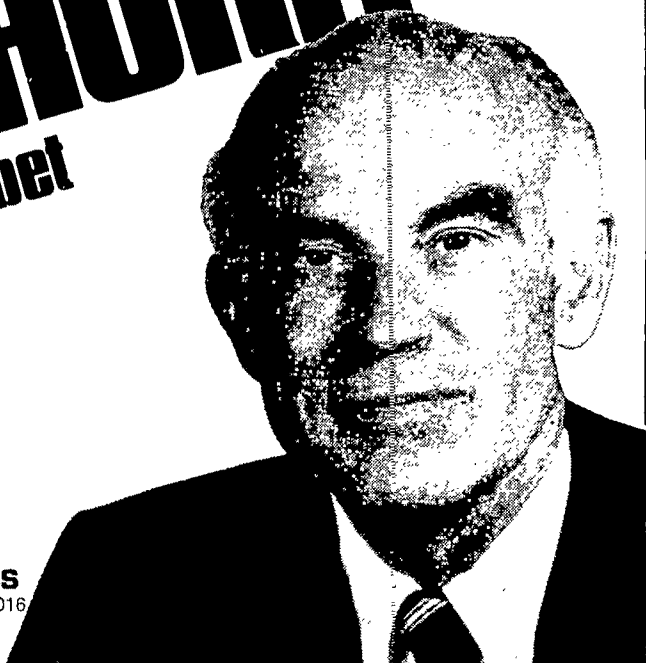
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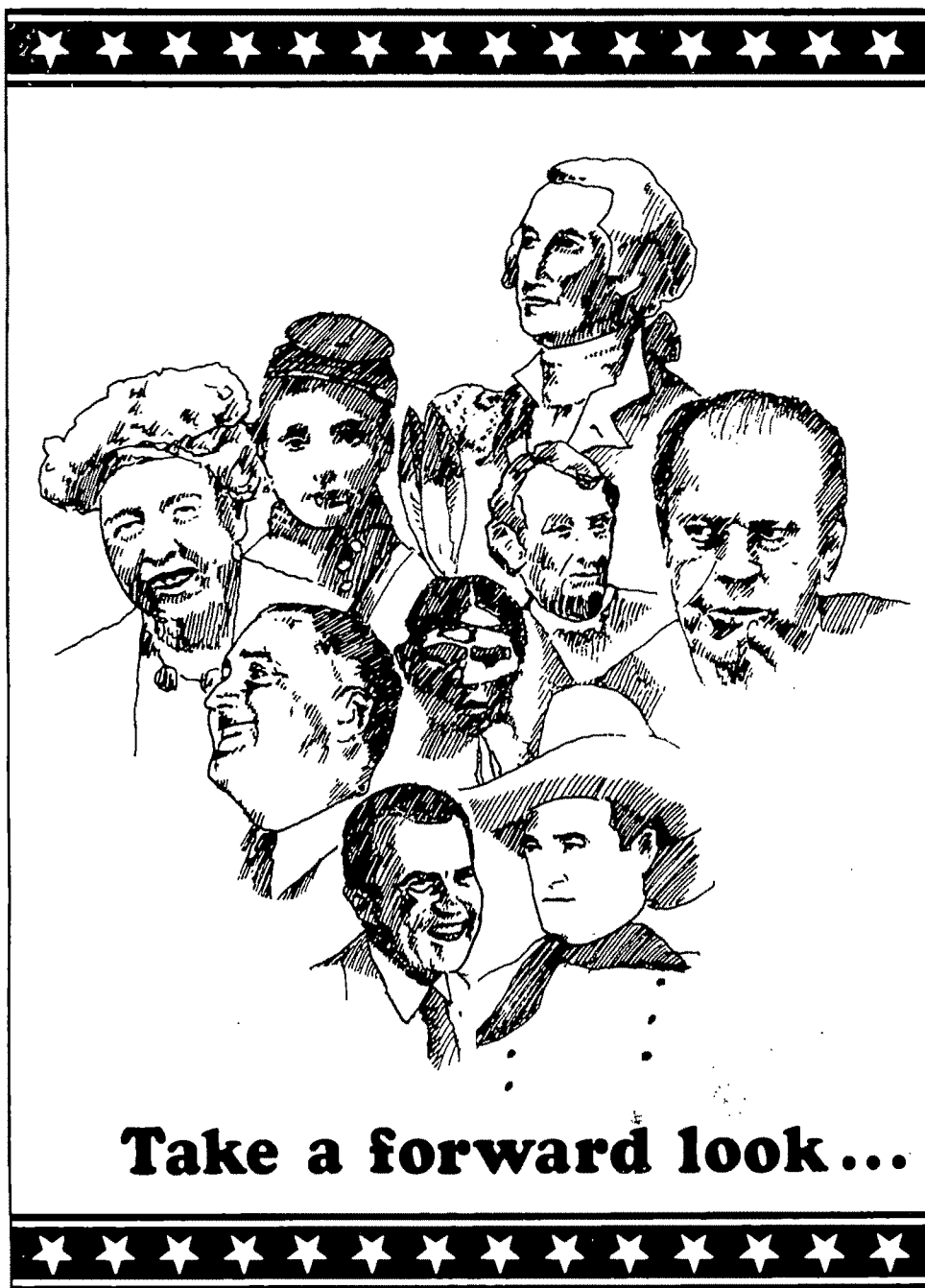
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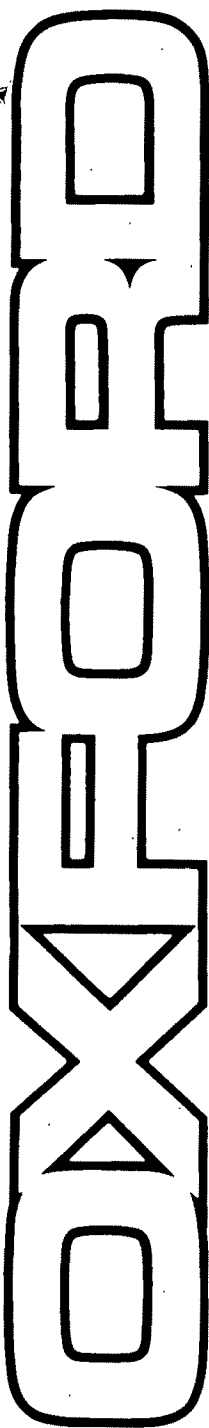
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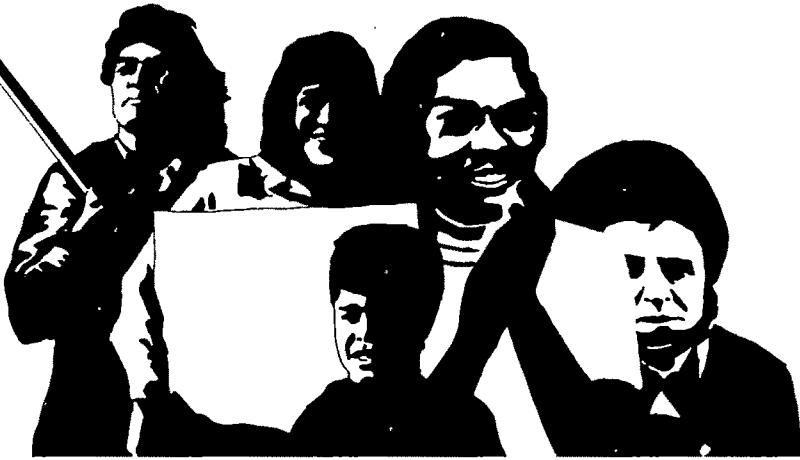
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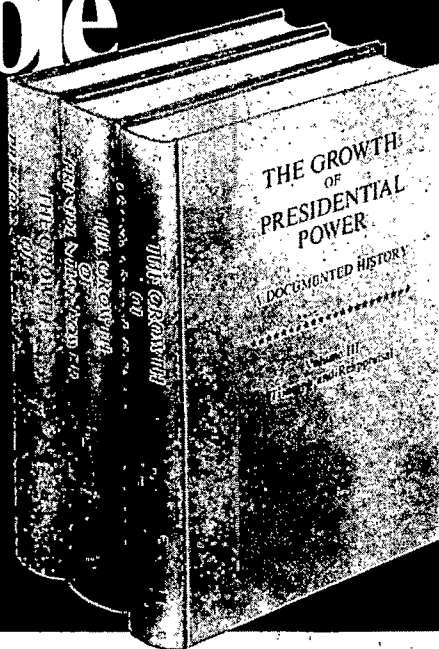
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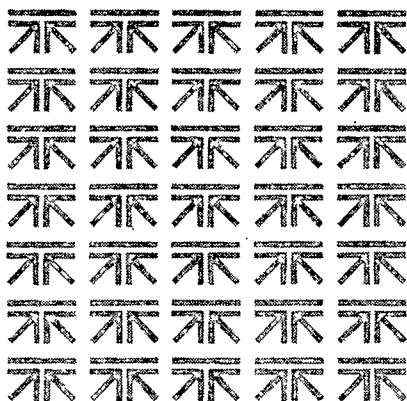
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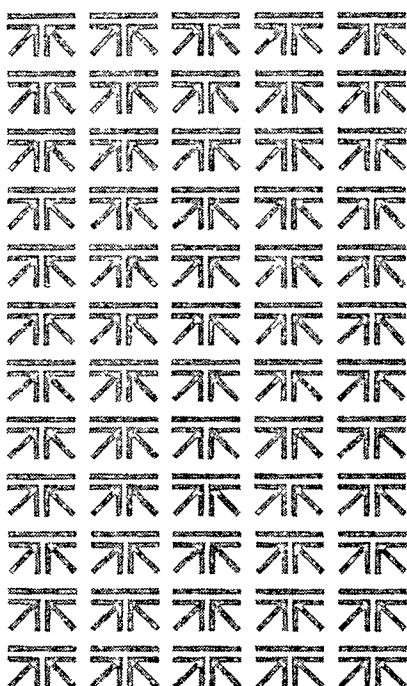


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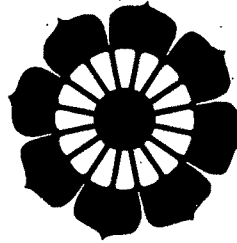
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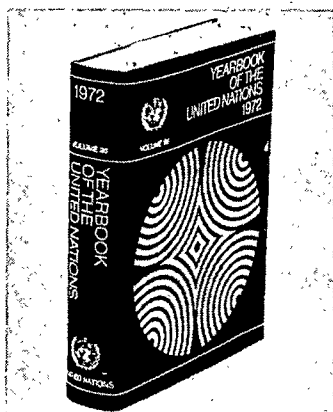
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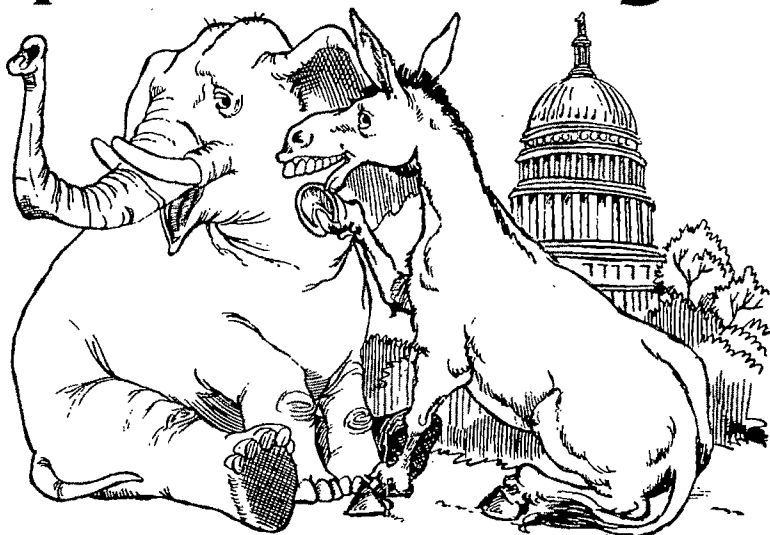
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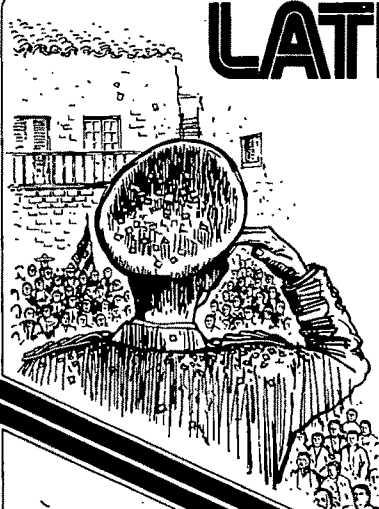
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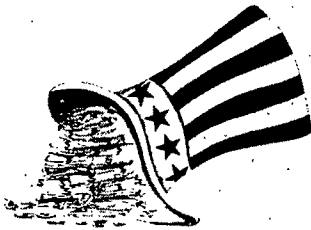
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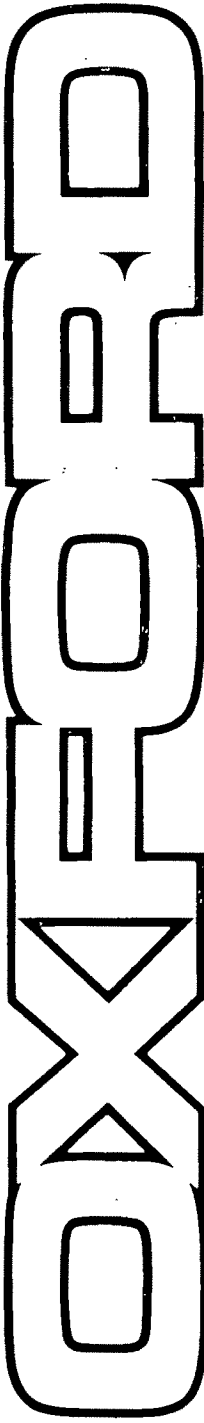
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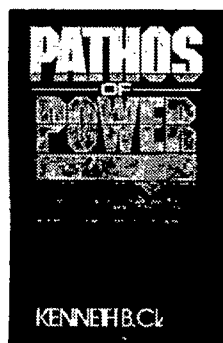
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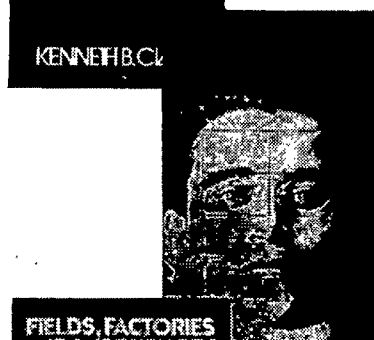
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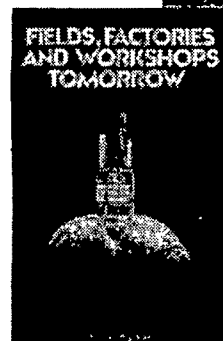


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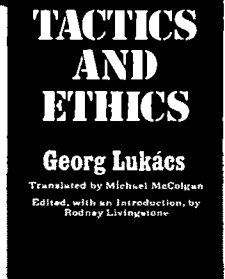
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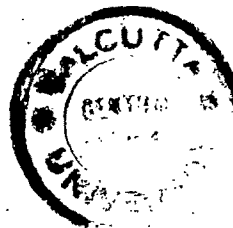
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